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The Impacts of Building Elite High Schools for Students from Disadvantaged Areas

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I. Introduction

Across the globe, students living in disadvantaged areas (rural, impoverished, remote) and from disadvantaged backgrounds (low income) are less likely than their advantaged counterparts to go to higher levels of schooling (Buchmann and Hannum 2001). In general, disadvantaged students repeat grades more, drop out more, and on average perform less well academically (Sirin 2005; Hannum and Wang 2006; Yi et al. 2012). They thus face serious challenges in taking advantage of education, an important channel for social mobility, as a means to help them and their households improve their long-term economic well-being (Glewwe 2002). Recognizing this, policy makers and researchers in developing countries have implemented a variety of interventions to improve the educational outcomes of disadvantaged students.

Thousands of studies from developing countries have attempted to assess the impacts of interventions on the educational outcomes of disadvantaged students (Glewwe et al. 2011). These have included demand-side interventions, which seek to provide disadvantaged students with incentives (or remove barriers) to go to and do well in school. For example, reducing or eliminating school fees (Kattan 2006), building schools nearer to students (and thus reducing transportation costs; Filmer 2004), or offering students meals (Bedi and Marshall 1999) have been found to improve school attendance to some degree. Conditional cash transfer programs, which emphasize the importance

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of schooling, have also been found to improve attendance and performance (Fizbein et al. 2009; Baird, McIntosh, and Ozler 2011; Behrman, Todd, and Parker 2011). Merit scholarships or even paying disadvantaged students directly to perform better in school have resulted in small to moderate positive impacts on educational aspirations, matriculation rates, or test scores (Angrist and Lavy 2002; Kremer, Miguel, and Thornton 2009; Behrman et al. 2011).

Supply-side interventions that address the quality of teaching and schools have also shown positive impacts on the educational outcomes of disadvantaged students. For instance, a review by Glewwe et al. (2011) notes that high-quality infrastructure, including walls, floors, roofs, and libraries, generally has small positive effects on student learning. Nutritional or medicinal interventions further have positive impacts on student populations with particular health risks (Miguel and Kremer 2004; Bobonis, Miguel, and Sharma 2006; Cutler et al. 2010; Behrman et al. 2011). Teacher quality also can have a substantial impact (Park and Hannum 2001; Rivkin, Hanushek, and Kain 2005), although there is little consensus on which particular aspects of teacher quality matter most for student outcomes.

The majority of studies from developing countries have focused on single and specific interventions for students, in contrast to more comprehensive reforms, which integrate demand- and supply-side interventions. One notable exception is Gertler, Patrinos, and Rubina-Codina (2007), who test for and find few if any synergies between demand-side (reducing school fees through subsidies) and supply-side (providing teacher training, supplies, and financing parent associations) interventions. In developed countries, only a few studies examine the (often mixed) impacts of establishing magnet schools, which offer a comprehensive set of demand-side incentives (lower fees, selective entry) and supply-side improvements (more spending per student, higher-quality facilities and teachers; Gamoran 1996; Ballou, Goldring, and Liu 2006; Esposito 2010).

The goal of this article is to examine the impact of a comprehensive package of demand- and supply-side educational interventions—namely, the building of free elite (or magnet) high schools targeted toward students from poor areas. In our study's context, students from poor areas in northwest China have lagged behind students from nonpoor areas in opportunities to attend college and elite colleges. Partially in response to these unequal college-going opportunities and partially in an attempt to alleviate poverty by promoting education in poor areas, policy makers in Ningxia, a province in northwest China, launched the Innovative High Schools intervention in 2003. The intervention established two elite high schools, Liu Pan Shan (LPS) High School (opened in fall 2003) and Yu Cai (YC) High School (opened in fall

2006). Both schools target students from poor areas and seek to improve their college admission outcomes. On the demand side, the Innovative High Schools intervention provides students from poor areas with (*a*) extra opportunities (seats) to attend elite academic high schools, (*b*) full tuition waivers, and (*c*) annual cash subsidies. On the supply side, the intervention invests substantial resources into the two elite schools to improve school and teacher quality (e.g., infrastructure, nutrition, teacher qualifications, and curriculum) so as to improve the college admission outcomes of students from poor areas.

This article conducts an impact evaluation of the Innovative High Schools intervention on the college admission outcomes of students in the poor counties of northwest China. We use a unique administrative data set that includes information on all students in the Ningxia region over 10 years, in combination with short interrupted time series (SITS) with comparison group designs, to estimate the causal effects of the intervention on the college admission outcomes of students from poor areas. By using this unique data set, we are able to estimate the impact of Innovative High Schools on the "typical student"—the average student in the entire age cohort—and not just the students who went to elite high schools. We also use the data and research designs to examine whether the establishment of two elite high schools (LPS and YC) created greater educational equality between poor and nonpoor counties by increasing the chances of students from poor counties to gain admission into college and elite colleges.

The rest of the article proceeds as follows. Section II describes (*a*) the background of the Innovative High Schools intervention, (*b*) the administrative data used in the analyses, and (*c*) our analytical strategy. Section III presents results on the impact of the intervention on the college admission outcomes of the typical student from poor counties. Section IV discusses the findings from Section III and concludes.

II. Research Design

A. Background on the Innovative High Schools Policy Intervention

Policy makers in the Ningxia region were motivated to introduce the Innovative High Schools intervention to help overcome the significant disparities in educational opportunities between students from poor and nonpoor counties. While Ningxia ranked eighteenth out of 31 provinces in the nation in terms of gross domestic product (GDP) per capita (¥21,470) in 2008, there have been significant economic disparities between Ningxia's 22 counties. For example, in 2008, the GDP per capita of Ningxia's 13 nonpoor counties ranged from approximately ¥9,000 to ¥30,000, while the GDP per capita of its nine nationally designated poor counties (mostly in the southern part of

Ningxia; see fig. 1) only ranged from approximately ¥2,500 to ¥7,000. Largely as a result of their less favorable economic situation, students from poor counties have been less likely than students from nonpoor counties to attend high school, elite high schools, college, and elite colleges (Loyalka et al. 2011). Similar economic and educational disparities in fact exist between poor and nonpoor counties in a number of other provinces in northwest and central China (Kanbur and Zhang 2005). In light of the widespread nature of these disparities, policy makers in Ningxia believed that the Innovative High Schools intervention, if successful, could serve as a model for improving the educational outcomes of students from poor areas across northwest and central China.

To improve students' educational outcomes, the Innovative High Schools intervention targeted a comprehensive package of demand-side incentives and supply-side inputs at students from poor counties. First, the intervention exclusively targeted students from nine poor counties—counties in the southern mountainous region of Ningxia that also have a higher proportion of rural students (see fig. 1). In other words, the Innovative High Schools intervention provided students from each poor county with opportunities to study in an elite academic high school (either LPS or YC)—an important consideration in China's heavily tracked system in which the total number of seats in academic high schools is limited.[1] Second, unlike other high schools in the area, LPS and YC high schools not only provided free education by waiving tuition and dorm fees but also provided stipends (on average ¥600–¥850 per student per year) to cover students' daily needs (for the full 3 years of academic high school, grades 10–12). Third, LPS and YC high schools attempted to provide a high-quality education for their students by (*a*) providing modern and new school facilities; (*b*) hiring teachers with relatively strong qualifications (e.g., 100% had at least a bachelor's degree, and a number of them graduated from distinguished normal universities in China); (*c*) providing instruction, curriculum, and extracurricular activities geared toward the special needs of students from poor areas; (*d*) potentially creating positive peer effects through selecting high-ability students and enabling more targeted instruction toward students of the same ability level; (*e*) strengthening the nutritional intake of students by supplementing their traditional diet of starchy vegetables with milk and eggs;

[1] Students who wish to go to academic high school (grades 10–12) in China take a high school entrance examination (HSEE) at the end of grade 9 (in June). On the basis of their HSEE scores and submitted choices, students are admitted into one (and only one) academic high school. Once admitted, students are rarely allowed to change to another academic high school. If students wish to change high schools, in addition to having to meet any academic requirements of the new high school, they must pay extraordinarily high tuition fees at the new high school.

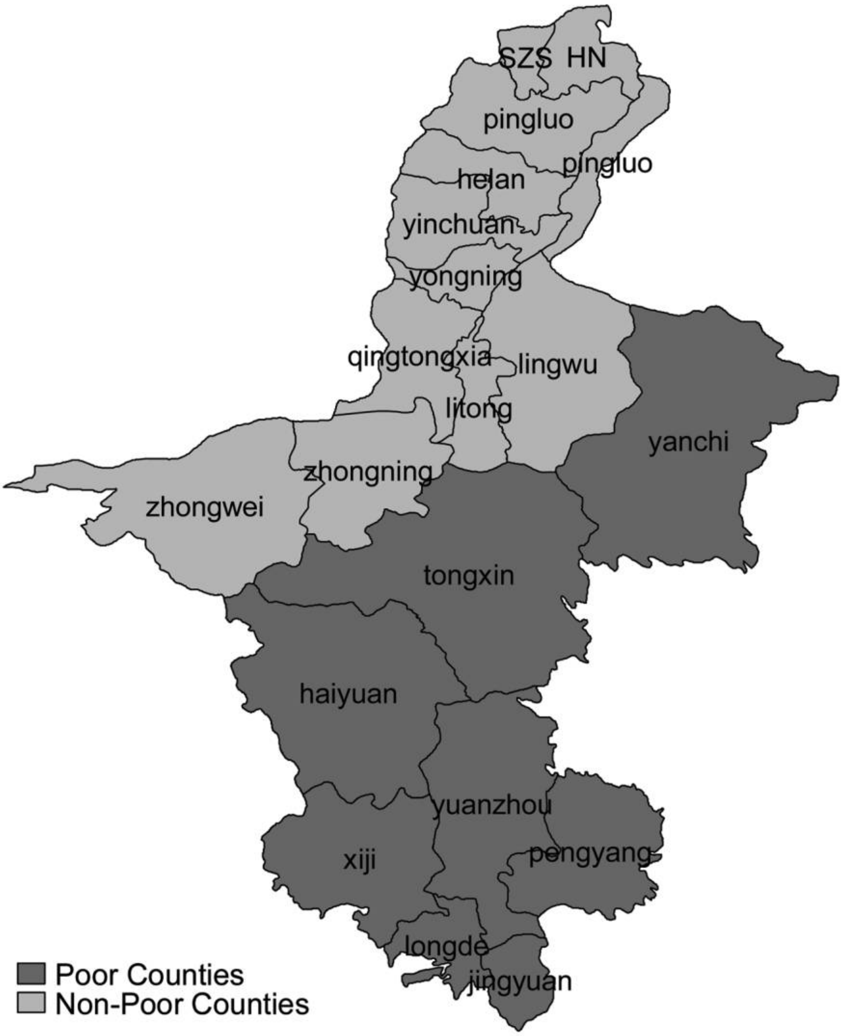

Figure 1. Poor (treatment) and nonpoor (control) counties in Ningxia. Yinchuan is composed of three distinct nonpoor, administrative districts (Xingqing District, Jinfeng District, and Xixia District). Yanchi is composed of two poor counties/districts (Yanchi County and Hongsibu District). SZS = Shizuishan; HN = Huinong.

and (*f*) having teachers live in rotation with students and be present to meet student academic and nonacademic needs. The mission and culture of the two elite high schools were also clearly formulated and communicated to various stakeholders (students, parents, teachers, school administrators, officials, and the community). The mission of the schools was to train students from poor counties so that they could attend colleges and elite colleges, from which they would one day earn high returns in the labor market and become a source of prosperity for their families and communities.

Given its comprehensiveness, the Innovative High Schools intervention was expected to have positive impacts on the college admission outcomes of students from poor counties. However, the policy may also have had negative effects on the college admission outcomes of students from poor counties for several reasons. First, since the two elite high schools absorbed some of the best students from local high schools in each poor county, students who remained in the local high schools in poor counties (with less able peers) may not have performed as well on the college entrance exam (Ding and Lehrer 2007).[2] Second, the subset of students from poor counties who were admitted into LPS and YC had to board at the schools since both schools were located in the capital city, a significant travel distance from their homes. Boarding students may have performed less well than those who lived at home because they had less household support at school (Coleman 1988).[3] Third, school administrators and teachers at LPS and YC reported that admitted students had difficulties acclimating to the larger urban environment around the schools, and this may have affected their performance. As such, despite what seems to have been an overwhelming positive set of demand- and supply-side interventions, it is still an open empirical question whether the policy intervention had a positive effect on the college admission outcomes of students from poor counties as a whole and, if so, to what degree.

There were two major stages to the Innovative High Schools intervention. LPS opened in September 2003 and attracted students from the top of the ability distribution (as measured by HSEE scores). YC opened in 2006 and also attracted high-ability students (although of slightly lower ability than LPS). Subsequent to their establishment, however, the two elite high schools admitted an increasing (and different) number of students from each county at the start of each academic year (see table 1). Therefore, as the elite high schools grew in size and targeted counties differently, the potential impact of the Innovative High School intervention on college admission outcomes may have changed over time as well. We take account of the fact that the potential impact of the intervention may have changed over time in our analytical models below (see Sec. II.C).

[2] But, lower-scoring students who remain behind in local high schools may receive more targeted instruction from teachers (who ordinarily would have catered to higher-scoring students), which would increase their learning (Duflo, Dupas, and Kremer 2009).
[3] Of course, students may still board at high schools in their local area (but are relatively closer to their families and can see them more often in this case).

B. *Data*

To assess the impact of the Innovative High Schools intervention on college admission outcomes, policy makers in Ningxia gave us access to a unique administrative data set. The administrative data contain information on all 342,485 students who just finished 3 years of academic high school and took the college entrance exam (CEE) in Ningxia from 2001 to 2010 (we call this our "CEE data"). The CEE is a standardized, high-stakes exam given to all high school graduates (in Ningxia) who seek to gain admission to college and elite colleges.

There are three major features of the CEE data that help us to assess the impact of the Innovative High Schools intervention on the college outcomes of students across Ningxia. First, the CEE, which is taken by the vast majority of academic high school graduates (in early June of the last year of academic high school—grade 12), provides us with each students' college admission result. That is, on the basis of the CEE scores and the college-major choices submitted after the CEE, each high school graduate is admitted (by a complex admissions matching procedure) into exactly one college and one major (or no college major if the student does not qualify for one of the college-major choices submitted). Second, the CEE data provide information on the county that each student was originally from, including whether a student was from a nationally designated poor county (our treatment variable, as it signifies access to the Innovative High Schools intervention). Third, the CEE data contain information on five high school graduate cohorts who took the CEE (in June 2001–5) before the first cohort of graduates from LPS took the CEE (in June 2006). The CEE data also contain information on five high school graduate cohorts who took the CEE from 2006 to 2010 (after the start of the Innovative High Schools intervention). We take advantage of variation in the presence of the intervention across years and across counties to help identify the causal effects of the intervention (see Sec. II.C).

One drawback of using the CEE data to assess the impact of the Innovative High Schools intervention on college admission outcomes is that the data only have information on students who took the CEE in various years. The CEE data thus do not represent the typical student in the population, as they exclude students who did not take the CEE. These students may have attended high school but decided not take the CEE, or they might have simply been students who did not attend high school. Whatever the case, it is likely that CEE takers are more motivated than non-CEE takers. As such, the CEE data only allow inferences about the impact of the Innovate High Schools program on a smaller (self-selected) group of individuals.

TABLE 1

CHARACTERISTICS OF THE STUDENTS IN POOR AND NONPOOR COUNTIES BEFORE AND AFTER THE INTERVENTION

	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
	Panel A									
No. of 18-year-olds (estimated from the 2000 census):										
Poor	42,386	46,688	46,846	50,661	55,368	54,087	55,289	66,316	52,899	51,016
Nonpoor	47,976	54,742	52,571	54,167	60,867	59,721	60,559	64,344	53,765	58,407
% of females in 18-year-old cohort:										
Poor	39.0	39.6	42.2	43.1	45.4	45.7	47.8	47.7	47.5	50.5
Nonpoor	50.1	49.1	51.0	50.8	52.5	51.6	52.7	52.8	52.9	53.3
	Panel B: College Entrance Exam Participants									
No. of high school students:										
Poor	6,781	7,117	10,412	10,976	12,478	13,989	14,794	15,790	17,086	17,085
Nonpoor	13,934	14,239	19,405	19,939	22,791	24,640	25,542	26,186	25,496	24,592
% of 18-year-olds:										
Poor	16.0	15.2	22.2	21.7	22.5	25.9	26.8	23.8	32.3	33.5
Nonpoor	29.0	26.0	36.9	36.8	37.4	41.3	42.2	40.7	47.4	42.1
% female students:										
Poor	39.0	39.6	42.2	43.1	45.4	45.7	47.8	47.7	49.5	50.5
Nonpoor	50.1	49.1	51.0	50.8	52.5	51.6	52.7	52.9	53.0	53.4

% minority students:										
Poor	27.0	27.1	29.4	29.1	29.0	29.8	30.1	30.4	30.6	32.7
Nonpoor	16.9	16.5	17.3	18.1	19.7	21.0	20.7	19.6	21.9	21.9
% rural students:										
Poor	75.1	72.8	75.0	73.2	73.9	74.7	76.3	76.8	79.5	79.2
Nonpoor	28.2	29.8	37.6	36.7	37.0	37.4	37.6	37.8	39.4	37.3
	Panel C: Elite High School College Entrance Exam Participants									
No. of students:										
Poor	1,028	1,192	1,389	1,675	1,462	2,732	3,153	3,361	4,081	4,601
Nonpoor	3,725	3,845	4,196	4,925	5,139	5,162	6,324	6,339	6,525	6,536
% female students:										
Poor	38.7	38.1	37.3	40.0	43.8	43.3	42.4	44.6	43.3	46.1
Nonpoor	48.3	49.8	49.5	49.0	50.1	50.3	50.2	52.1	51.6	51.4
% minority students:										
Poor	17.0	25.2	28.9	26.8	27.7	29.1	31.7	31.5	31.4	35.6
Nonpoor	19.8	19.2	22.1	20.6	23.1	26.2	26.7	24.2	25.3	25.6
% rural students:										
Poor	46.6	49.0	48.5	49.0	41.4	52.3	56.1	54.0	54.6	57.9
Nonpoor	7.3	6.6	10.3	10.4	9.6	9.5	10.5	10.9	14.8	15.9

To assess the impact of the Innovative High Schools intervention on the typical student in the population (including students who took the CEE and those who did not), we augment the CEE with data from China's 2000 population census. The augmenting procedure for the CEE data consists of four steps. (*a*) We first used the 2000 census data to approximate the number of individuals in the 18-year-old cohort from each county in Ningxia in each year 2001–10 (i.e., the number of "eligible" CEE takers). We used the number of 17-year-olds in each county in the 2000 census as a substitute for the number of 18-year-olds in the age cohort in each county in 2001, and so on. (*b*) Our next step was to count the number of students who took the CEE in each county in Ningxia for each year from 2001 to 2010 (the actual number of CEE takers in each county-year). (*c*) We calculated differences in the number of 18-year-olds in each county-year from the number of students who took the CEE in each county-year. (*d*) We then appended new individual-level observations to the CEE data equal in number to the differences from step *c* and filled in the values of the county, year, treatment, and college admission outcome variables for the new appended observations.

The "augmented CEE data" thus capture the size of the appropriate age cohorts in each county-year. Altogether, we added another 746,190 observations to the 2001–10 CEE data. We unfortunately are unable to obtain more detailed information on individual background characteristics from the 2000 census (with which to further augment the CEE data). We can, however, see the college admission outcome values for all of the students in the augmented CEE data, since all students who did not take the CEE did not gain admission to college or to elite colleges (and thus had an outcome value equal to 0). By adding the 2000 census information on year of birth, county of origin, and college outcomes, we can thus test whether the Innovative High Schools intervention affected the college admission outcomes of the average student. In technical terms, the augmented data set allows us to account for the censoring of student observations who did not attend high school but who may have also been affected by the intervention.

C. *Analytical Strategy*

The main way in which we estimate the impact of the Innovative High Schools intervention on the college admission outcomes of the typical students is by using a SITS design. In general, the SITS design is used to identify the effects of an intervention that alters trends in an outcome of interest at one or more specific points in time. If the intervention has a causal impact, the time series after the intervention (the postintervention series) would have a different counterfactual trajectory (e.g., level or slope) than if the intervention did not have a causal impact (Shadish and Cook 2009). Under certain conditions, the difference in preintervention and postintervention trajectories can be used to estimate the causal impact of the intervention.

To obtain unbiased causal estimates from the SITS design, the design has to meet four sets of conditions. First, the functional form used in the SITS analysis (in particular the function that predicts how the outcome changes over time) should be correctly specified (Steiner, Wroblewski, and Cook 2009). Although the functional form is easier to specify with a longer time series, having measures of the outcome at six time points (three preintervention points and three postintervention points; Bloom 2003) can also yield valid estimates. The preintervention points are mainly used to understand trends in the outcome before the time of the intervention. The postintervention points are used to understand the potential change in the trend of the outcome because of the intervention. We discuss how we test the robustness of our findings to changes in functional form in Sections II.C.1 and II.C.2 below.

Second, the internal validity of the SITS design can be threatened by historical events that took place at the same time as the intervention—these historical events may be the cause of changes in the outcome variable of interest rather than the intervention (Shadish, Cook, and Campbell 2002). In this type of situation, the internal validity of the SITS design can be strengthened by comparing the treated time series with comparison (control) time series that have never been subject to the intervention but that could have been subject to other historical interventions that the treated time series is also subject to (Wong and Cook 2009). This so-called *SITS with comparison groups* design enables us to compare cohort differences in treatment and control counties and is thus generally considered a stronger (in terms of internal validity) design than SITS alone for policy impact evaluation. SITS with comparison groups in fact has been applied recently to evaluate the impacts of the US No Child Left Behind policy on improving school performance (Wong and Cook 2009; Dee and Jacob 2011). We discuss other ways to address threats from historical events when we present our analytical models below (see Secs. II.C.1–II.C.3).

Third, the internal validity of the SITS with comparison groups design can be threatened by "changes in composition." That is, the way the outcome variable changes over time should not be due to unintended changes in the composition of the study population (Steiner et al. 2009). For example, it should not be the case that individuals who become aware of the intervention self-select themselves into the treatment or control groups (or out of the study population entirely).

Institutional factors in China, in fact, help limit changes in the composition of students before and after the intervention. Specifically, the residential permit (*hukou*) system in China prevents students from shifting from one county to another, while the HSEE system prevents students from transferring from one academic high school to another. The *hukou* system, for example, prevents students from poor counties from moving to nonpoor counties to go to academic high school (and vice versa). The HSEE system further prevents students from transferring high schools within the same county, much less across counties (with the Innovative High Schools intervention being an exception). The two systems are rigid such that even if students leave the province (e.g., with their parents who find work in another province), they will in most cases have to return to their home county to take the HSEE and CEE.

By examining our augmented data more closely, we can also see how the composition of students within poor (treatment) and nonpoor (control) areas for the most part remained stable or grew linearly from 2001 to 2010 (see table 1).[4] This is true of changes in the size of the 18-year-old age cohort and the proportion of females in each age cohort (panel A) or of changes in the percentage of female, minority, and rural students taking the CEE (panel B). While the number of students taking the CEE did climb more rapidly in poor counties compared to nonpoor counties (panel B), the increase could have been due to the introduction of LPS/YC. Indeed, the number of students attending elite high schools in poor counties also increased substantially after LPS and YC were established (note that poor areas had a few local elite high schools to which students from poor counties could exclusively apply before the creation of LPS and YC). For the most part, the composition of students from elite high schools who participated in the CEE also grew at a steady, linear rate (panel C).[5]

[4] We note here that the 2000 census statistics and the CEE data are the best data available from Ningxia for showing changes in the composition of students across counties before and after the intervention. This is despite the fact that the CEE data only focus on students who took the CEE (and not, e.g., on all students in high school or in elite high schools). Policy makers in Ningxia do not actually possess a separate set of complete and accurate administrative data on high school and elite high school enrollments for the time period.

[5] There appear to be two exceptions to this statement. First, in poor counties, the proportion of minority students from elite high schools who took the CEE jumped substantially from 2001 to 2002. This should not affect the internal validity of the SITS with comparison groups design since it was much before the timing of the intervention. Second, in nonpoor counties, the proportion of rural students from elite high schools who took the CEE jumped from 2008 to 2009 (when YC was introduced). If the increase in the proportion of rural students taking the CEE was due to another outside intervention in nonpoor areas, it would mean that the estimates from our SITS with comparison groups design are lower-bound estimates of the true effect of the intervention.

Fourth, the internal validity of the SITS with comparison groups design can be compromised by "threats of instrumentation"—that is, if the measurement or definition of the outcome variable(s) changed with the introduction of the intervention. Because we are using broad measures of college admission outcomes that are set at the provincial and national levels, and the definitions of these measures have not substantively changed over time, we are also not especially concerned with threats of instrumentation in our subsequent analyses.

1. SITS with Comparison Groups—Binary Treatments

With the above conditions in mind, our basic specification for the SITS with comparison group(s) analyses is as follows:

$$\begin{aligned} Y(ict) &= \text{constant} + D0 \times \text{year} + D1 \times \text{Tarea}(c) + D2(\text{year} \times \text{Tarea})(ct) \\ &+ D3 \times \text{PostLPSpolicy}(t) + D4(\text{years\_since\_LPS})(t) \\ &+ D5(\text{Tarea} \times \text{PostLPSpolicy})(ct) + D6(\text{years\_since\_LPS} \times \text{Tarea})(ct) \\ &+ D7 \times \text{PostYCpolicy}(t) + D8(\text{years\_since\_YC})(t) \\ &+ D9(\text{Tarea} \times \text{PostYCpolicy})(ct) + D10(\text{years\_since\_YC} \times \text{Tarea})(ct) \\ &+ X(ict)'D + \text{county fixed effects} + \text{error}(ict). \end{aligned} \tag{1}$$

In equation (1), $Y(ict)$ represents the outcome variable (various indicators for whether a student was admitted into any college, a more elite 4-year college, an even more elite tier 1 or 2 college, or one of the most elite 211/985 colleges). Since $Y(ict)$ is binary, we further run equation (1) as both a linear probability model and a logit model (paying more attention ex ante to the results from the logit model).[6] "Year" indicates the year in which a high school graduate in our sample took the CEE (equal to 1–10 for 2001–10, respectively); Tarea, or

[6] The marginal effects estimate for an explanatory variable is the change in the outcome variable for a unit change in that explanatory variable. Because many of the explanatory variables in our models are factor variables (i.e., dummies), we use the "margins, dydx" command in Stata to obtain "discrete first differences estimates" for those variables. By way of example, to obtain the discrete first differences estimate for Tarea in eq. (1), we first subtract a model evaluated at the base category for Tarea (where Tarea = 0) from eq. (1). "Margins, dydyx" evaluates that difference for each observation. "Margins, dydx" then averages the differences across observations to obtain the discrete first differences, or what we call the marginal effects estimate.

treatment area, is an indicator equal to 1 for the poor (treatment) counties and 0 otherwise; PostLPSpolicy indicates the first 3 years in which LPS graduates took the CEE and which were before YC graduates took the CEE (i.e., PostLPSpolicy is equal to 1 in years 6, 7, and 8 and 0 otherwise); PostYCpolicy indicates the years in which YC (and LPS) graduates took the CEE (i.e., PostYCpolicy is equal to 1 in years 9 and 10 and 0 otherwise); years_since_LPS indicates the number of years since graduates from LPS began to take the CEE until the time in which graduates from YC began to take the CEE (the variable is equal to 1 in year 2006, 2 in 2007, 3 in 2008, and 0 otherwise); years_since_YC indicates the number of years since graduates from YC began to take the CEE (the variable is equal to 1 in year 2009, 2 in 2010, and 0 otherwise).

We use $X(ict)$ in equation (1) to control for three major sources of county-year variation that could be correlated with the timing of the treatment and the outcome. Not controlling for such county-year factors could bias the estimates from equation (1). First, we control for county-year variation in economic levels (GDP per capita) and local government spending per capita. Increases in economic levels could have increased access to higher levels of schooling. Increases in local government spending per capita could have increased the quality of local (primary and secondary) schooling. Both factors could have improved students' chances of gaining admissions into college across county-years and yet also coincided with the timing of the Innovative High Schools intervention.

Second, we control for county-year variation in primary and secondary school enrollments that may have coincided with the Innovative High Schools intervention and that may have simultaneously affected college admissions. According to policy makers, almost all education policies in Ningxia from 2001 to 2010 were concerned with increasing enrollments in primary and secondary schooling; policy makers may have taken special steps to improve enrollment rates in primary and secondary schooling in poor counties in different years. Third, we control for changes in population and (18-year-old) cohort sizes across county years that reflect the amount of competition a student has (in a given county-year) when applying for academic high schools, elite high schools, and colleges.[7]

[7] For the sake of completeness, we also control for the size of the college or elite college quotas each year (quotas that are set by the provincial government before students take the CEE each year). For example, if the outcome is "any college," we control for the size of the any college quota. If the outcome is "4-year colleges," we control for the size of the 4-year college quota, and so on. We control for quota size because the quota size across years is not necessarily linear,

We pay attention to three types of estimates from equation (1). First, we examine whether the treatment and comparison group means are different after the intervention from what they are predicted to be from the groups' observed pretest means. The coefficient *D*5 represents the difference in mean changes between treatment and control groups after the establishment of LPS, while *D*9 represents the difference after the establishment of YC. Second, we examine whether the observed difference in treatment and comparison slopes is different after the intervention than before the intervention. The coefficient *D*6 represents the differences in slope changes between treatment and control groups after the establishment of LPS, while *D*10 represents the difference after the establishment of YC. Third, we examine whether the final difference between treatment and comparison groups in year 10 (in 2010, the last year of our data) differs from that predicted by preintervention means and slopes. We regard this final difference as the "total impact" of the Innovative High Schools intervention. For example, the total impact of YC is estimated as $D9 + D10 \times 2$, where 2 is the number of years since graduates from YC began to take the CEE (see Wong and Cook 2009).

We also use slightly different specifications of equation (1) to see whether the results of our analyses are robust to different functional forms. By using a linear year term and interacting this term with other variables, equation (1) assumes that the trend in the college admissions rate (for any college or for a particular college tier, depending on the dependent variable) is linear. Indeed, figure 2*B*–2*D* provides some support for this assumption by showing that a linear admissions rate trend generally holds for 4-year universities, tier 1 or 2 universities, and 211 or 985 (elite) universities. However, we also see from figure 2*A* that the trend in "any college" admission rates before the LPS and YC interventions could be nonlinear. The trend in the size of the admissions quotas for Ningxia (for all colleges) may have been nonlinear due to exogenous (e.g., college expansion) policy factors. To test the robustness of our functional form assumptions to these trends, we modify equation (1) by first adding squared year terms and then interact the squared year term with other model covariates in equation (2). We also omit the year and year interaction terms from equation (2) entirely. Section III focuses on the estimates from equation (1) only, since all three specifications yielded substantively similar results (results omitted for the sake of brevity). As a further robustness check, we run a more flexible dosage model (with and without year fixed effects), which we discuss immediately below.

may affect the outcomes of students in poor and nonpoor counties differently, and could be correlated with the timing of the Innovative High Schools intervention.

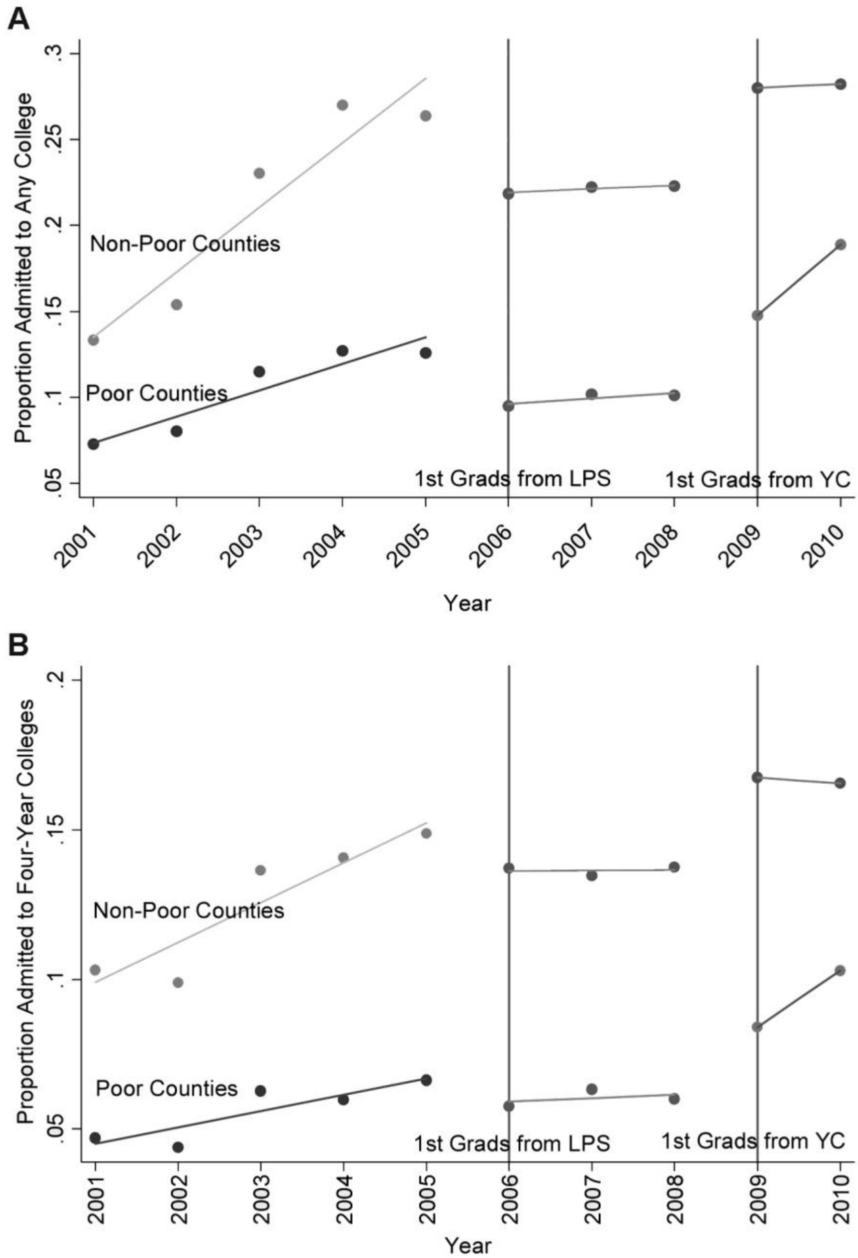

Figure 2. Admissions trends, any college (*A*), 4-year colleges (*B*), tier 1 or 2 colleges (*C*), 211/985 colleges (*D*).

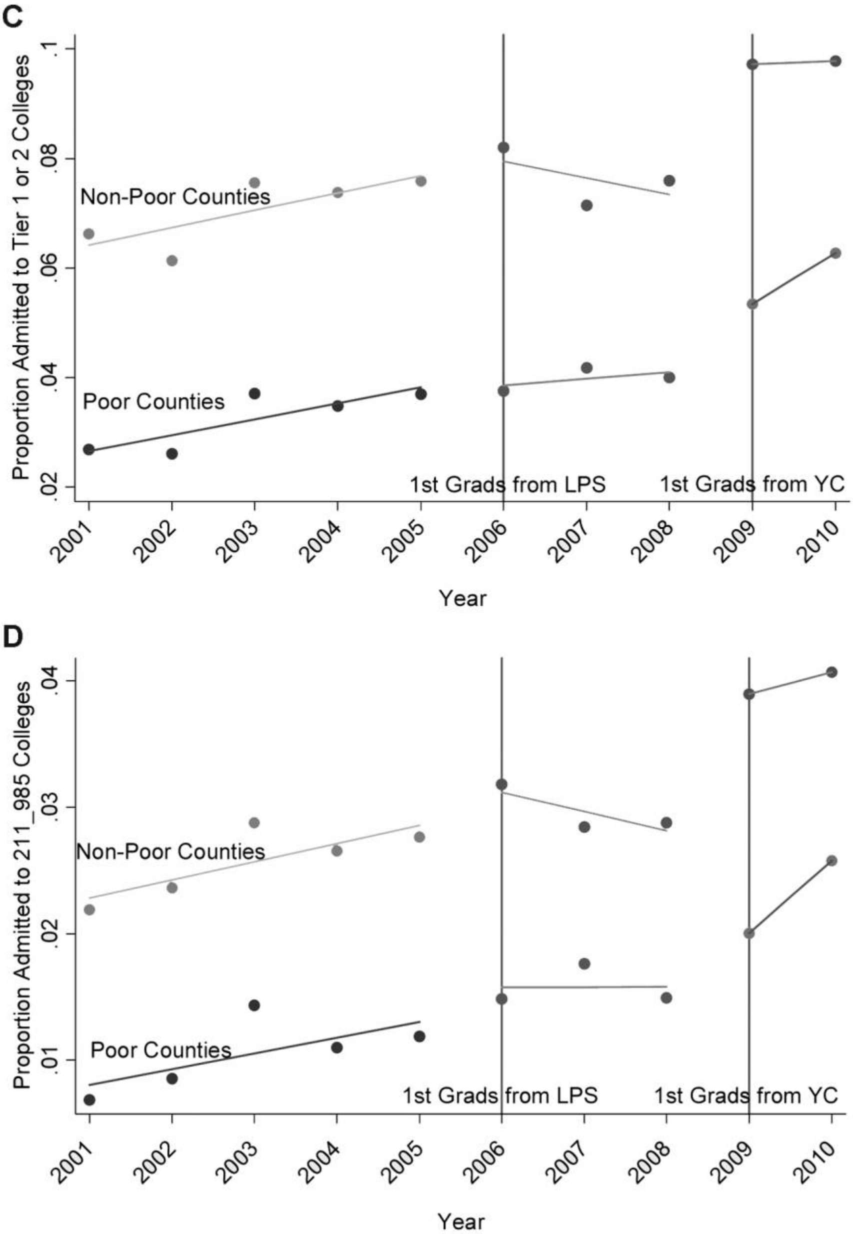

Figure 2. (*Continued*)

2. SITS with Comparison Groups—Continuous (Dosage) Treatments

In addition to basic specification for the SITS with comparison group(s) analyses (eq. [1]) above, we also look at the impacts of the intervention using a "dosage specification":

$$\begin{aligned} Y(ict) = \text{constant} + B0 \times \text{year} + B1 \times \text{Tarea}(c) + B2 \times \text{PostPolicy}(t) \\ + B3(\text{year} \times \text{Tarea})(ct) + B4(\text{year} \times \text{PostPolicy})(t) \\ + B5(\text{Dosage})(ct) + X(ict)'B + \text{county fixed effects} + \text{error}(ict), \end{aligned} \tag{2}$$

where $Y(ict)$ is a binary outcome variable for whether a student was admitted into any college, a 4-year college, a tier 1 or 2 college, or a 211/985 college; Tarea is an indicator equal to 1 for the treatment counties and 0 otherwise; and "year" indicates the year in which a high school graduate in our sample took the CEE (equal to 1–10 for 2001–10, respectively). Equation (2) also controls for county fixed effects and the same county-year factors as in equation (1).[8]

The main way in which the dosage specification differs from the specification in equation (1) is that it defines the treatment as a single continuous variable instead of two binary treatment variables. Specifically "Dosage" refers to the percentage of the 15-year-old age cohort in a particular county-year that can potentially access a seat in one of the elite high schools (LPS or YC). According to table 2, for example, the percentage of the 15-year-old age cohort in Yanchi County (a poor county) that can potentially access a seat in LPS in 2003 (and therefore take the CEE in 2006) is 1%. It is clear from table 2 that the dosage for each county increases over time after the establishment of LPS and especially after the establishment of YC. Furthermore, students from nonpoor areas (who did not have access to LPS or YC) had a dosage of 0%. Finally, the dosage specification also contains the binary indicator PostPolicy that equals 0 in the years before the first graduates from LPS take the CEE (years 2001–5) and 1 in the years in which graduates from LPS or YC take the CEE (years 2006–10).

The dosage specification relies on more information than the earlier binary treatment models since continuous variables (the dosage) contain more information than binary variables (the binary treatments). In particular, the

[8] We also run the dosage model with year fixed effects (instead of a linear year term and interactions with the year term). The dosage model with year fixed effects also finds that the intervention has a statistically significant impact on any college and 4-year college admission outcomes (results omitted for the sake of brevity).

TABLE 2
PERCENTAGE OF THE 15-YEAR-OLD AGE COHORT THAT COULD ACCESS A SEAT IN LPS AND YC (BY COUNTY AND YEAR)

County/District	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
Yinchuan	0	0	0	0	0	0	0	0	0	0
Yongning	0	0	0	0	0	0	0	0	0	0
Helan	0	0	0	0	0	0	0	0	0	0
Lingwu	0	0	0	0	0	0	0	0	0	0
Shizuishan	0	0	0	0	0	0	0	0	0	0
Litong	0	0	0	0	0	0	0	0	0	0
Qingtongxia	0	0	0	0	0	0	0	0	0	0
Shapotou	0	0	0	0	0	0	0	0	0	0
Zhongning	0	0	0	0	0	0	0	0	0	0
Yanchi	0	0	0	0	0	1.00	2.48	2.42	5.64	9.34
Yuanzhou	0	0	0	0	0	.70	.81	.63	1.56	2.52
Xiji	0	0	0	0	0	1.22	1.38	1.24	2.70	4.02
Longde	0	0	0	0	0	.90	1.53	2.32	4.18	6.20
Jingyuan	0	0	0	0	0	2.38	3.49	1.99	4.23	6.95
Pengyang	0	0	0	0	0	.73	1.34	1.24	3.11	2.62
Haiyuan	0	0	0	0	0	.91	1.80	1.47	3.43	4.44
Tongxin + Hongsibao	0	0	0	0	0	1.23	2.15	1.57	2.83	4.43

Note. Years are those in which LPS/YC (Liu Pan Shan/Yu Cai) graduates took the college entrance exam (CEE; 3 years after entering LPS or YC). For example, 1% of the 15-year-old students from Yanchi County entered LPS/YC in 2003 and then went on to take the CEE in 2006 (at approximately age 18).

dosage specification not only uses variation across the poor versus nonpoor counties but also uses the additional variation across the county-years of poor counties (counties that receive different dosages in different years). The dosage specification also allows us to account for the fact that the "strength" of the treatment (in terms of access to LPS/YC) changed somewhat over time after the establishment of LPS/YC. Although they rely on more information, we will see (in Sec. III below) that the analyses based on the dosage model yield qualitatively similar results to the analyses based on the binary treatment models.

3. Further Addressing Historical Threats to Validity to the SITS with Comparison Groups Design—Placebo Tests

As we discussed in Sections II.B and II.C.1 above, we attempted to address historical threats to the validity of the SITS with comparison groups design in two ways: (*a*) improving the validity of the SITS design by adding a comparison group series (especially as in the dosage specification in Sec. II.C.2) and (*b*) controlling for possible confounders that vary by county-year. To further address the possibility that our SITS with comparison groups design does not deal adequately with confounding local historical factors, we also run placebo tests of the impacts of the intervention on the preintervention data.

Specifically, we create "placebo" dosage variables (which falsely indicate that there were available seats in LPS/YC in the preintervention years) and use these dosage variables in a series of difference-in-difference (DID) models on the preintervention data (from 2001 to 2005). The DID specification is as follows:

$$\begin{aligned} Y(ict) &= \text{constant} + A1 \times \text{Tarea}(c) + A2(\text{Placebo\_Dosage})(ct) \\ &+ X(ict)'A + \text{county fixed effects} + \text{year fixed effects} + \text{error}(ict). \end{aligned} \tag{3}$$

We run equation (3) using different definitions of the Placebo_Dosage variable. In general, we define Placebo_Dosage for a given county-year as being equal to 0 for nonpoor counties and equal to the actual LPS/YC dosage 5 years later (the number of seats in LPS/YC) for poor counties. We then additionally set Placebo_Dosage equal to 0 for all poor counties in (*a*) year 1; (*b*) years 1 and 2; (*c*) years 1, 2, and 3; or (*d*) years 1, 2, 3, and 4. We use different definitions of the Placebo_Dosage variable (according to alternatives *a*–*d*) in the DID regressions to test whether the placebo treatment has a statistically significant impact on college admission outcomes. If the estimates of the placebo treatment are not statistically different from zero, then we have greater confidence that our SITS with comparison groups design successfully deals with threats to historical validity.

Finally, we note that the standard errors in our all of our above analyses (including the DID analyses and the earlier SITS with comparison groups analyses) are corrected for school-level clustering. Specifically, we use Huber-White standard errors, which relax the assumption that disturbance terms are independent and identically distributed within schools.

III. Results: Impacts on the College Admission Outcomes of the "Typical Student"

According to our basic SITS with comparison groups analyses (the analyses that use binary treatment variables; see Sec. II.C.2), the Innovative High Schools intervention had a positive impact on "any college" admissions (table 3, cols. 1 and 2). While the mean impacts of establishing LPS and YC on any college are not significantly different from zero at the 5% level, the slope estimates for both LPS and YC are positive and statistically significant (at the 5% level). More importantly, the total impact by 2010 is 14.4 percentage points according to the ordinary least squares (OLS) estimates and 8.6 percentage points according to the logit estimates. Thus, according to the logit estimates, the intervention increased the likelihood that a typical 15-

year-old student from a poor county would gain admission into any college by roughly 37%.[9]

The analyses also show that the intervention had a positive impact on 4-year college admissions (table 3, cols. 3 and 4). While the mean impacts of establishing LPS and YC on 4-year college admissions again are not significantly different from zero, the slope estimates for both LPS and YC were generally positive (although not always statistically significant, especially in the logit analyses). The OLS estimate of the total impact on 4-year college admissions by 2010 is 4.0 percentage points, while the logit estimate is 2.8 percentage points. Therefore, according to the logit estimates, by 2010 the intervention increased the likelihood that a typical 15-year-old student from a poor county could gain admission to a 4-year college by roughly 24%.

Despite the positive impacts on college and 4-year college admissions, the basic SITS with comparison groups analyses show that the intervention did not have a positive impact on admissions into elite colleges (table 3, cols. 5–8). In almost all cases, neither the estimates of the mean nor the slope impacts of establishing LPS and YC on tier 1 or 2 college or 211/985 college admissions are statistically significant. The total impact estimates for both outcomes are also close to zero and not statistically significant. Taken together, the results show that the intervention did not increase the likelihood that a typical 15-year-old student from a poor county would gain admission to an elite college.

We next use the basic SITS with comparison groups design and data from the years 2001–8 only (before the first cohort of students from YC take the CEE in 2009) to assess the impact of the establishment of only LPS on college admission outcomes. Table 4 shows that LPS had a small to negligible impact on college admissions. Whereas the OLS estimate of the total impact of LPS on any college admissions was 9.7 percentage points by 2008 (statistically significant at the 1% level), the logit estimate was smaller (2.7 percentage points) and statistically insignificant. Similarly, the OLS estimate of the total impact of LPS on 4-year college admissions was 3.2 percentage points by 2008 (statistically significant at the 5% level), whereas the logit estimate was close to zero in magnitude and not statistically significant. Because the outcome variable is binary and the logit regressions offer a somewhat better fit, we place more

[9] We also run, but do not present (for the sake of brevity), SITS with comparison groups analyses that do not adjust for the county-year controls. The estimates from the “unadjusted” SITS with comparison groups analyses (either the analyses that use binary treatments or the analyses that use dosage treatments) are qualitatively similar to (although slightly larger in magnitude) the “adjusted” analyses. Namely, the total impact estimates show that the intervention has a positive and statistically significant impact on any college and 4-year college admission outcomes but not on elite college admission outcomes.

TABLE 3

SITS WITH COMPARISON GROUPS ANALYSES OF THE IMPACTS ON THE COLLEGE ADMISSION OUTCOMES OF THE "TYPICAL STUDENT" (USING BINARY TREATMENTS)

	Any College		4-Year College		Tier 1 or 2 College		211 or 985 College	
	OLS (1)	Logit (2)	OLS (3)	Logit (4)	OLS (5)	Logit (6)	OLS (7)	Logit (8)
Year[a]	.007	−.002	−.002	−.003	−.004*	−.003*	.001	.001**
	(.008)	(.007)	(.005)	(.006)	(.002)	(.002)	(.001)	(.001)
Tarea[b]	−.422***	−.312***	−.305***	−.221***	−.169**	−.135***	−.048	−.048
	(.071)	(.044)	(.096)	(.058)	(.073)	(.049)	(.037)	(.029)
Year × Tarea	−.016**	−.006	−.002	.002	.004*	.005*	.001*	.002**
	(.006)	(.006)	(.003)	(.003)	(.002)	(.002)	(.001)	(.001)
PostLPSpolicy[c]	−.014	.006	.001	.003	.011***	.007**	.005***	.003*
	(.015)	(.014)	(.009)	(.008)	(.003)	(.003)	(.001)	(.002)
Years_since_LPS[d]	−.014	−.004	−.004	−.001	−.002	−.001	−.002	−.001
	(.008)	(.008)	(.005)	(.005)	(.003)	(.002)	(.001)	(.001)
Tarea × PostLPSpolicy	.007	−.017*	−.008	−.008	−.014***	−.008	−.005**	.000
	(.013)	(.009)	(.008)	(.006)	(.004)	(.005)	(.002)	(.003)
Years_since_LPS × Tarea	.030***	.016**	.013***	.005	.006**	.002	.003**	.000
	(.007)	(.007)	(.004)	(.004)	(.003)	(.002)	(.001)	(.001)
PostYCpolicy[e]	−.021	.018	−.001	.003	.010	.006	.002	.000
	(.027)	(.030)	(.018)	(.017)	(.008)	(.006)	(.005)	(.003)

Years_since_YC[f]	−.009	.003	.010	.013	.011	.010	.006	.005
	(.028)	(.021)	(.016)	(.013)	(.013)	(.010)	(.006)	(.004)
Tarea × PostYCpolicy	.031	.006	−.005	−.002	−.020	−.010	−.009	−.004
	(.030)	(.025)	(.023)	(.020)	(.015)	(.015)	(.007)	(.006)
Years_since_YC × Tarea	.057**	.040**	.023	.015	.006	.003	.003	.002
	(.025)	(.018)	(.015)	(.010)	(.011)	(.007)	(.005)	(.003)
County-year covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Total impact[g]	.144***	.086***	.040**	.028***	−.009	−.005	−.003	−.001
	(.039)	(.026)	(.015)	(.011)	(.013)	(.009)	(.006)	(.003)

Note. Cluster robust standard errors in parentheses, county fixed effects included. Marginal effects estimates reported, calculated through discrete first-differences estimates. SITS = short interrupted time series; OLS = ordinary least squares. N = 1,088,675.

[a] Year in which a high school graduate in our sample took the college entrance exam (CEE; equal to 1–10 for 2001–10, respectively).

[b] Indicator equal to 1 for the poor (treatment) counties and 0 otherwise.

[c] First 3 years in which LPS (Liu Pan Shan) graduates took the CEE and that were before YC (Yu Cai) graduates took the CEE (i.e., PostLPSpolicy is equal to 1 in years 6, 7, and 8 and 0 otherwise).

[d] Number of years since graduates from LPS began to take the CEE until the time in which graduates from YC began to take the CEE (the variable is equal to 1 in year 2006, 2 in 2007, 3 in 2008, and 0 otherwise).

[e] Years in which YC (and LPS) graduates took the CEE (i.e., PostYCpolicy is equal to 1 in years 9 and 10 and 0 otherwise).

[f] Number of years since graduates from YC began to take the CEE (the variable is equal to 1 in year 2009, 2 in 2010, and 0 otherwise).

[g] Final difference in outcome (any college, 4-year college, tier 1 or 2 college, 211 or 985 college) between treatment and comparison groups in year 10 (2010, the last year of our data).

* $p < .1$.
** $p < .05$.
*** $p < .01$.

TABLE 4

SITS WITH COMPARISON GROUPS ANALYSES OF THE IMPACTS OF LPS ON THE COLLEGE ADMISSION OUTCOMES OF THE "TYPICAL STUDENT" (USING A BINARY TREATMENT AND DATA FROM YEARS 2001–8 ONLY)

	Any College		4-Year College		Tier 1 or 2 College		211 or 985 College	
	OLS (1)	Logit (2)	OLS (3)	Logit (4)	OLS (5)	Logit (6)	OLS (7)	Logit (8)
Year[a]	.005	−.001	−.004	−.005	−.004**	−.003**	.001	.001**
	(.008)	(.007)	(.005)	(.005)	(.002)	(.001)	(.001)	(.000)
Tarea[b]	.488***	.340***	.383***	.247***	.209***	.142***	−.060	−.051
	(.080)	(.047)	(.054)	(.046)	(.066)	(.047)	(.044)	(.032)
PostLPSpolicy[c]	−.010	.006	.004	.006	.011***	.008**	.005***	.003**
	(.015)	(.012)	(.009)	(.008)	(.003)	(.003)	(.002)	(.002)
Tarea × year	−.016**	−.005	−.001	.003	.003**	.005**	.001**	.002***
	(.006)	(.005)	(.003)	(.003)	(.002)	(.002)	(.001)	(.001)
Years_since_LPS[d]	−.020**	−.011	−.006	−.003	−.001	.000	−.002**	−.001
	(.010)	(.007)	(.005)	(.004)	(.002)	(.002)	(.001)	(.001)
Tarea × PostLPSpolicy	.006	−.017	−.008	−.009	.015***	−.009*	−.005**	−.000
	(.015)	(.011)	(.009)	(.007)	(.004)	(.005)	(.002)	(.002)
Tarea × years_since_LPS	.030***	.015**	.014***	.004	.006**	.002	.003**	.000
	(.008)	(.007)	(.004)	(.003)	(.003)	(.002)	(.001)	(.001)
County-year controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Total impact[e]	.097***	.027	.032**	.005	.004	−.004	.004	.000
	(.030)	(.021)	(.013)	(.010)	(.008)	(.008)	(.003)	(.002)
R^2	.055	.057	.039	.053	.017	.036	.009	.038

Note. Cluster robust standard errors in parentheses, county fixed effects included. Marginal effects estimates reported, calculated through discrete first-differences estimates. SITS = short interrupted time series; OLS = ordinary least squares. N = 872,588.

[a] Year in which a high school graduate in our sample took the college entrance exam (CEE; equal to 1–10 for 2001–10, respectively).

[b] Indicator equal to 1 for the poor (treatment) counties and 0 otherwise.

[c] First 3 years in which LPS (Liu Pan Shan) graduates took the CEE and that were before YC (Yu Cai) graduates took the CEE (i.e., PostLPSpolicy is equal to 1 in years 6, 7, and 8 and 0 otherwise).

[d] Number of years since graduates from LPS began to take the CEE until the time in which graduates from YC began to take the CEE (the variable is equal to 1 in year 2006, 2 in 2007, 3 in 2008, and 0 otherwise).

[e] Final difference in outcome (any college, 4-year college, tier 1 or 2 college, 211 or 985 college) between treatment and comparison groups in year 10 (2010, the last year of our data).

* $p < .1$.

** $p < .05$.

*** $p < .01$.

weight on the logit estimates.[10] The OLS or logit estimates of the impact of LPS on tier 1 or 2 college or 211/985 college admissions are also close to zero and not statistically significant. The lack of a total impact of the intervention (LPS only) by 2008 in contrast to the significant impacts of the intervention (LPS/YC combined) by 2010 could be due to the substantial increase in the treatment dosage from 2008 to 2010 (see table 2).

Indeed, the results from the dosage specification (again using all 10 years of data from 2001 to 2010) indicate that dosage matters (table 5). According to both the OLS and the logit results, a 1% increase in the percentage of the 15-year-old age cohort that can access a seat in one of the elite high schools increases admissions into any college by 1.3–1.6 percentage points (table 5, cols. 1 and 2). The results are statistically significant at the 1% level. Furthermore, both the OLS and the logit results show that a 1% increase in the percentage of the 15-year-old age cohort that can access a seat in one of the elite high schools increases admissions into 4-year colleges by 0.5 percentage points. The OLS estimate is statistically significant at the 10% level (although the p-value for the logit estimate is slightly above .10). By contrast, the estimates of the impact of a 1% increase in the percentage of the 15-year-old age cohort that can access a seat in one of the elite high schools seems to have no discernible impact on admissions to elite colleges (tier 1 or 2 colleges or 211/985 colleges). In summary, the results indicate that an increase in the treatment dosage had a positive impact on any college and 4-year college admissions but no impact on tier 1 or 2 college or 211/985 college admissions.

Finally, our placebo tests provide support for the idea that there are no major historical threats to the internal validity of our SITS with comparison groups analyses (table 6). With only one exception, the tests of the impact of the placebo dosage do not show positive and significant impacts on college admission outcomes. The one exception (the impact of the placebo dosage on admissions into a 211 or 985 college when the placebo treatment years are 3, 4, and 5) is positive and statistically significant at the 10% level. Since 1 out of every 10 estimates should be statistically significant at the 10% level by chance, the fact that only 1 out of our 16 estimates is statistically significant at the 10% level provides support for the idea that the SITS with comparison groups analyses are internally valid.

IV. Discussion and Conclusion

The results of the various SITS with comparison groups analyses suggest that the Innovative High Schools intervention improves the college admission out-

[10] The discrepancy between the OLS and logit estimates was likely due to the sensitivity of this particular SITS with comparison groups analysis (which looks at the impact of LPS only) to functional form.

TABLE 5

SITS WITH COMPARISON GROUPS ANALYSES OF THE IMPACTS ON THE COLLEGE ADMISSION OUTCOMES OF THE "TYPICAL STUDENT" (USING DOSAGE TREATMENTS)

	Any College		4-Year College		Tier 1 or 2 College		211 or 985 College	
	OLS (1)	Logit (2)	OLS (3)	Logit (4)	OLS (5)	Logit (6)	OLS (7)	Logit (8)
Year[a]	.002	−.000	−.003	−.003	−.004*	−.004**	.001*	.002***
	(.007)	(.006)	(.005)	(.005)	(.002)	(.002)	(.001)	(.001)
Tarea[b]	−.436***	−.319***	−.310***	−.226***	−.168**	−.133***	−.048	−.048*
	(.076)	(.048)	(.097)	(.057)	(.072)	(.044)	(.037)	(.027)
PostPolicy[c]	−.050	−.055	−.031	−.030	−.009	−.007	−.002	−.001
	(.046)	(.050)	(.025)	(.027)	(.016)	(.015)	(.007)	(.006)
Year × Tarea	−.007*	−.003	.001	.003	.003**	.004***	.001*	.002***
	(.004)	(.004)	(.002)	(.002)	(.001)	(.001)	(.001)	(.001)
Year × PostPolicy	.005	.007	.004	.004	.002	.002	.000	.000
	(.008)	(.009)	(.004)	(.004)	(.003)	(.002)	(.001)	(.001)
Dosage[d]	.016***	.013***	.005*	.005	.000	.001	.000	.000
	(.004)	(.005)	(.002)	(.003)	(.002)	(.002)	(.001)	(.001)
County-year controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	.060	.060	.043	.055	.020	.039	.011	.041

Note. Cluster robust standard errors in parentheses, county fixed effects included. Marginal effects estimates reported, calculated through discrete first-differences estimates. SITS = short interrupted time series; OLS = ordinary least squares. N = 1,088,675.

[a] Year in which a high school graduate in our sample took the college entrance exam (CEE; equal to 1–10 for 2001–10, respectively).

[b] Indicator equal to 1 for the poor (treatment) counties and 0 otherwise.

[c] Equals 0 in the years before the first graduates from Liu Pan Shan (LPS) take the CEE (years 2001–5) and 1 in the years in which graduates from LPS or Yu Cai (YC) take the CEE (years 2006–10).

[d] Percentage of the 15-year-old age cohort in a particular county-year that can potentially access a seat in one of the elite high schools (LPS or YC).

* $p < .1$.

** $p < .05$.

*** $p < .01$.

TABLE 6

PLACEBO TESTS OF THE IMPACT OF THE INTERVENTION ON PREINTERVENTION COLLEGE ADMISSION OUTCOMES (DIFFERENCES-IN-DIFFERENCES ANALYSES USING DATA FROM 2001 TO 2005)

	Placebo Dosage in Years 2–5 (1)	Placebo Dosage in Years 3–5 (2)	Placebo Dosage in Years 4 and 5 (3)	Placebo Dosage in Year 5 (4)
Any college	−.006	−.007	−.006	.011
	(.026)	(.020)	(.017)	(.015)
4-year college	.008	.011	.002	.012
	(.033)	(.029)	(.024)	(.017)
Tier 1 or 2 college	.035	.037	.019	.016
	(.047)	(.045)	(.035)	(.022)
211 or 985 college	.051	.056*	.021	.015
	(.034)	(.032)	(.021)	(.013)

Note. Cluster robust standard errors in parentheses, county fixed effects included. Marginal effects estimates reported, calculated through discrete first-differences estimates.

* $p < .1$.

comes of students from poor counties to some degree. On the one hand, the intervention increases the chances that a typical student from a poor county will gain admission to any college and 4-year colleges (nonelite colleges). On the other hand, the intervention does not increase the chances that the typical student will gain admission to tier 1 or 2 or 211/985 colleges (elite colleges).

Another finding of our study is that the dosage of the intervention (the number of seats available in LPS and YC in each county) is important in affecting college admissions. Whereas by 2008, 0%–2.5% of the age cohort from different poor counties had the opportunity to attend one of the treatment schools, by 2010, 2.5%–9% of the age cohort from different poor counties had the opportunity (table 2). The treatment was therefore strong enough in 2010 (relative to 2008) to produce measurable effects.

A notable aspect of this study was our ability to measure the impact of the Innovative High Schools intervention on the typical student and not just those who attended. Indeed, the size of our estimated effects indicates that the intervention has not only direct impacts on students attending LPS/YC but also indirect impacts on students who do not attend LPS/YC. Even when we conservatively reduce our point estimates (from the logit dosage specification) by a standard deviation, we find that a 1 percentage point increase in the number of students from poor areas who can attend LPS/YC increases college admissions by 0.8 percentage points and 4-year college admissions by 0.25 percentage points.

How did the Innovative High Schools program improve the college enrollment outcomes of students in disadvantaged areas of China? First, by providing much greater access to elite academic high schools through the establishment of LPS/YC, the intervention enabled students who previously could not attend academic high school (extramarginal students) with important chances to gain admission to any college and 4-year colleges. In China's heavily tracked education system, if students cannot enter academic high school, their chances of entering college are slim. If they can gain access to academic high school, however, they have a high probability of gaining admissions into college (approximately three-fourths of the students who gain admission into high school in Ningxia gain admission into college). Second, greater access allowed inframarginal students (who were already attending low-quality academic high schools) to attend academic high schools of higher quality (since the new seats in LPS/YC were taken by students at the top of the ability distribution, and all other students along the ability distribution were shifted upward to higher-quality academic high schools). Attending a higher-quality school may, in turn, increase students' chances of gaining admissions to nonelite colleges (Park et al. 2010). Third, providing a large number of free seats in elite high schools may have a motivational effect on the typical student from a poor county (i.e., by encouraging him or her to participate more and perform better on the HSEE).[11]

The way in which the intervention affects students has slightly different implications for policy makers who are interested in improving college access for students from poor areas. In competitive education systems like China (where high school access is severely rationed), the effects may be more in terms of extramarginal students who are now able to attend high school (and by extension, college). If so, policy makers might instead increase college enrollments simply by increasing the number of available seats in academic high schools (no matter the quality of the academic high school) in poor areas. But, in less competitive education systems where students who want to enroll in high school are mostly able to attend, elite high schools may operate more through inframarginal students, who are able to move from lower-quality to higher-quality high schools. If so, policy makers might increase the number of seats in higher-quality academic high schools or in fact consider building elite high schools.

Of course, there is a final issue of cost effectiveness. Whether the effects are more through extramarginal or inframarginal students, policy makers may be able to resort to the cheaper option of expanding existing high schools rather than building and investing heavily in new elite high schools. In other words, unless the motivational effects of the Innovative High Schools intervention are large and persist from the beginning until the end of high school (an open, empirical question), a more cost-effective solution may be for policy makers to expand existing high schools rather than spend money on the capital construction and supply-side (school quality) aspects associated with the Innovative High Schools intervention.

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[11] We are unfortunately unable to provide rigorous evidence of motivational effects, due to data limitations.

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