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Abstract

How much do schools differ in their effectiveness? Recent studies that seek to answer this question account for student sorting using random assignment generated by central allocation mechanisms or oversubscribed schools. However, the resulting estimates, while causal, may also reflect peer effects due to differences in peer quality of non-randomized students. We exploit *universal* random assignment of students to high schools in certain areas of South Korea to provide estimates of school effects that may better reflect the effects of school practices. We find significant effects of schools on scores in high-stakes college entrance exams: a 1 standard deviation increase in school quality leads to 0.06-0.08 standard deviations higher average academic achievement in Korean and English languages. Analogous estimates from areas of South Korea that do not use random assignment, and therefore include the effects of student sorting and peer effects, are substantially higher.
JEL classifications: I21, J24
Keywords: School effects, Universal random assignment, Peer effects, School inputs

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1 Introduction

Do schools differ in the impact they have on academic achievement, and if so, by how much? Answering these questions is key to informing central elements of education policy. For example, if schools differ markedly in their effectiveness, disseminating best practices across educational establishments could lead to significant improvements in learning. Furthermore, in many instances, schools deemed ineffective based on academic performance have been targeted for reorganization and closure.[1] However, identifying the impact of schools on academic achievement, separately from the characteristics of the students who attend these schools, is complicated by the sorting of heterogeneous students into schools.

To account for student sorting, recent studies estimating school effects on academic achievement have exploited random assignment generated by oversubscribed schools that admit students by lottery (e.g., Cullen et al., 2006; Abdulkadiroğlu et al., 2011) and centralized allocation mechanisms (e.g., Deming et al., 2014; Abdulkadiroğlu et al., 2017). Yet, in most centralized allocation mechanisms, only some students are assigned in a randomized fashion. Even with schools that admit students entirely by lottery, those who do not get admitted generally end up in schools where other students were not randomly assigned. Hence, the resulting estimates of school effectiveness, while causal, also reflect peer effects due to differences in peer quality of non-randomized students, in addition to differences that arise from other school inputs and practices.[2] This is relevant for policy since school effects due to the influence of peer quality are likely outside the control of most schools or school districts.

In this paper, we derive estimates of how schools differ in their impact on academic achievement based on universal random assignment in South Korea (henceforth Korea). Our approach is related to those based on lotteries generated by centralized allocation mechanisms. But a unique feature of our setting is the universal nature of the random assignment. This is possible because certain districts in Korea randomly assigned all students to general high schools during the 1990s. With universal randomization, student composition is balanced across all students in the schools, avoiding both the confounding effect of student sorting and the possibility of peer effects from students who would not otherwise have been randomized into such schools. Therefore, we are

[1] In the United States, for example, the federal No Child Left Behind Act mandated that struggling schools be restructured, reopened as charters, or closed if they failed to improve test scores for five consecutive years. Many other state and local authorities have closed schools for low academic performance.

[2] As noted by Deming et al. (2014), "these papers share the limitation that they cannot unpack the impact of changing school assignment into changes in peer quality, teacher quality, or other important inputs."

able to provide evidence on the importance of "pure" school effects that reflect the influence of school inputs and practices over which schools may have more control than peer quality.[3]

We estimate the variation in these pure school effects on student scores in national college entrance exams. We use individual-level administrative data for the period 1995-1997 and focus on a subset of administrative divisions where all students attending general high schools were randomly assigned to schools within districts. To document the extent of variation in school effectiveness, we estimate the standard deviation of pure school effects. These estimates can be interpreted as the expected increase in learning if students were moved to schools that are one standard deviation higher in the school effectiveness distribution. They are analogous to those estimated in recent studies documenting the variation in classroom and teacher effectiveness (Chetty et al., 2014a; Araujo et al., 2016).[4]

In addition to the randomness of student assignment within selected areas, Korea's high school system is well suited for this analysis. First, it is characterized by virtually universal high school attendance and very low repetition, attrition, or inter-school student movements. These features alleviate substantially the concern that our estimated pure school effects might be contaminated by heterogeneity in student composition. Second, the high-stakes nature of college entrance exams, taken by 99% of students at the end of general high school, makes them a reliable measure of academic achievement. Third, the main results in this paper use data for about 78,000 randomized students in each year. The sheer size of these data allows for the precise estimation of the variation in school effects, even in smaller sub-samples.

Our experimental estimates indicate that schools vary considerably in their effectiveness. A 1 standard deviation increase in school quality within a district results in 0.07 standard deviation higher test scores in language, on average. The estimates are similar for boys and girls, but tend to be higher in the English language (up to 0.10 standard deviation) than in the native language (0.06 standard deviation). Interestingly, the magnitude of our estimates is comparable to non-experimental estimates of school value-added in Charlotte-MecKlenburg (Deming, 2014), as well as to experimental and quasi-experimental estimates of classroom and teacher effectiveness in the United States and Ecuador (Chetty et al., 2014a; Jackson et al., 2014; Araujo et al., 2016).

[3] Raudenbush and Willms (1995) distinguish between school effects that are due to school practices and those due to school contexts outside of the control of school administrators and teachers. Whereas parents deciding which school to send their children likely consider the "total" school effect from both school practices and school contexts, it is the former that matters for evaluating teachers and administrators.

[4] Note that experimental studies estimating teacher effects almost always involve the universal random assignment of students to classrooms, in contrast to the existing studies estimating school effects.

To further support the interpretation of our estimates, we perform two additional analyses. First, we estimate the standard deviation of school effects in Korean administrative divisions where students were sorted into schools based on an application process that involves student preferences, performance in exams, and middle school GPA. Second, we perform a similar analysis for other countries using data from PISA 2000 tests. In both these cases, the dispersion in learning outcomes across schools can be explained by pure school effects, as well as student sorting and peer effects. We find that the standard deviation of CSAT scores across schools is substantially larger in non-randomized areas in Korea. The dispersion in learning outcomes across schools is almost 10 times larger than that observed in randomized areas. The estimates obtained using country data from PISA are also substantially larger, ranging from 0.23 to 0.83 standard deviations. Taken together, this evidence underscores the importance of accounting for the impacts of student selection and peer effects when estimating school effects, as made possible by the system of universal random assignment in Korean high schools.

An important consideration in interpreting our findings, and in evaluating their external validity, is the extent to which the degree of variation in pure school effects is influenced by institutional factors that potentially affect the quality of key school inputs, such as the ability to choose principals and teachers. We examine this issue by comparing the variation of pure school effects across public and privately-founded schools. In privately-founded schools, that account for about two-thirds of schools in randomizing areas, there is a school-specific selection process of principals and teachers, who can either be dismissed or stay in the school for long periods of time depending on performance. By contrast, public schools, which account for the other third of schools within randomizing areas, do not have any discretion on the selection of teachers and principals. We find that the standard deviation of pure school effects is not systematically larger among privately-founded schools. Interestingly, using data from the cross-section of countries surveyed in PISA 2000, we also document that schools in Korea have a relatively high degree of autonomy and display considerable heterogeneity in observed inputs. This evidence suggests that our main results are likely to apply more broadly.

Our paper relates to several strands of existing research. As already mentioned, the studies closest to our own are those estimating the effectiveness of schools using lotteries from centralized allocation mechanisms and from schools that admit students entirely through lotteries (Bloom and Unterman, 2014; Cullen et al., 2006; Abdulkadiroğlu et al., 2011; Deming, 2014; Abdulkadiroğlu et al., 2017). Several related studies examine the validity of value-added methods for estimating the effect of schools on academic achievement by comparing them to those derived from random assignment (Deutsch, 2013; Deming, 2014; Abdulkadiroğlu et al., 2017). There are also some recent studies that examine the effect of schools on non-test scores outcomes such as socio-emotional learning, school behavior and educational attainment (Loeb et al., 2018; Jackson et al., 2020).

Another set of studies examines the effect of being admitted to more selective schools using regression discontinuity designs (Pop-Eleches and Urquiola, 2013; Jackson, 2013; Abdulkadiroğlu et al., 2014; Dobbie and Fryer Jr, 2014). With this research design, the role of peer effects is likely to be even more pronounced since students who just barely get admitted to a selective school, and thus surrounded by peers who score substantially better, are compared with students who just barely miss getting admitted, surrounded by peers in a less selective school who scored substantially worse.[5] Indeed, Jackson (2013) finds that a substantial fraction of the effect of admission to high achievement schools in Trinidad and Tobago can be attributed to higher-achieving peers.

There is also an extensive literature on the effectiveness of different school inputs and practices, such as class-size (e.g., Krueger, 1999; Angrist and Lavy, 1999), teacher quality (e.g., Rockoff, 2004; Rivkin et al., 2005) and peer effects (e.g., Hanushek et al., 2003; Lavy et al., 2012). More recently, Dobbie and Fryer Jr (2013) correlate school effects with a variety of different school inputs and practices in charter schools. They show that school practices such as frequent teacher feedback, intensive tutoring, and instructional time explain more of the variation in school effectiveness than traditional input measures, such as class size, per-pupil expenditure and teacher training.

Finally, some studies have exploited the random assignment of students to high schools in Korea to analyze how different school features affect learning and other outcomes. In particular, Park et al. (2013) examine how attending a single-sex school impacts test scores in college entrance exams and college attendance, and Park et al. (2018) study how attending these types of schools influences students' interests and major choices. Furthermore, Hahn et al. (2018) examine how attending privately-founded schools, as opposed to public schools, impacts test scores and college attendance.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background that gives rise to the natural experiment and the data employed, before providing evidence supporting the validity of the experimental design. Section 3 presents the empirical strategy. Sec-

[5] This also implies that students just barely admitted have a very different relative ranking from those who just barely did not get admitted. See Murphy and Weinhardt (2020) on the effects of relative ranking.

tion 4 presents the main results while Section 5 shows that these results are robust to alternative samples and specifications. Section 6 provides a discussion on the external validity of our findings. Section 7 concludes the paper.

2 Experimental Design and Data

2.1 Institutional Background

In Korea, children between the ages of six and 15 are required to attend school. Compulsory education consists of six years of elementary school, followed by three years of middle school. Students typically attend their local elementary and middle schools, and do not have considerable school choice until the end of compulsory education. After completing middle school, students enter high school, which takes another three years to complete. Although enrollment in high school is not mandatory, about 97% of students from the corresponding cohort graduated in 2005.

High schools are classified as either general, vocational, or selective. General high schools provide advanced general education along with elective courses, which students select on the basis of their intended university studies. Vocational high schools offer the education necessary to enter a specific profession and are frequently focused on one occupational area, such as agriculture, commerce, or technology. Selective schools provide a more specialized curriculum, have greater autonomy, and select students in a competitive process based on GPAs and interviews. Selective high schools absorbed less than 1% of students entering high schools in randomizing administrative divisions in Korea, while vocational high schools absorbed about one quarter of these students. The remaining three-quarters of students entering high school were randomly assigned to general high schools.[6]

During the relevant period for our analysis, the process for assigning students to high schools had two rounds. In the first round, common to all administrative divisions in the country, interested students applied and were assigned to selective or vocational schools. The second round, which allocated the remaining non-assigned students to general high schools, varied across three groups of administrative divisions. In a first group, there was universal random assignment of

[6]These proportions were computed using administrative data from CSAT, described in detail below. Data from KELS (also described below) reveal that in 2008 vocational and selective high schools absorbed, respectively, about 26% and 2% of students entering high school in Korea, whereas general high schools absorbed the remaining 72% of students.

students to general high schools within school districts. In a second group, students applied to high schools and assignment decisions were made individually by schools largely on the basis of test scores and middle school GPAs. Finally, in a third group, a subset of districts randomized students to high schools, while other districts followed the application and admission procedure.

The geographic variation regarding the assignment of students to general high schools stems from the partial implementation of the "High School Equalization Policy" across administrative divisions. The central feature of this policy was the randomization of students to high schools. It was adopted largely in response to a status quo characterized by fierce competition for elite high schools. In addition to balancing the composition of students across high schools through random student assignment, the equalization policy initially aimed at equalizing the quality of teachers and facilities across schools. Monetary transfers were centralized and balanced across schools, education facilities were upgraded, and teacher training was provided. However, these other components were not successfully implemented, as budgetary constraints made it infeasible to incur the relatively high costs associated with teacher training and facility improvement (Korea Education Development Institute, 1998).

The equalization policy was first implemented in 1974 in Seoul and Busan (the largest metropolitan cities). It was then progressively expanded to include other metropolitan cities, provincial capitals and finally major regional cities.[7] The equalization policy was never implemented in some smaller administrative divisions. Between 1980 and 1995 the system remained essentially stable. For cohorts that started in 1996 and took the national exam in 1998, some limited choice was reintroduced in certain administrative divisions where the equalization policy was implemented. In particular, students identified the two or three schools of their preference. Schools then filled 30 to 40% of slots by random selection among students who showed preference for the school. The remaining slots were then randomized across students residing in the corresponding school district who were not assigned to their preferred school. For this reason, our empirical analysis focuses on the cohorts that took the national exam in 1995-1997 and therefore did not experience this increase in choice.[8]

The high school system was characterized by virtually universal enrollment and very low repetition, attrition, or inter-school student movements. There were different types of general high

[7] Lee (2012) provides detailed information on this process, while Park (2013) offers a comprehensive description of the equalization policy in Korea.

[8] Until 1993, Korea had a universal college entrance exam named the Student Achievement Test (the *hakruk* exam), but data are not available. The College Scholastic Ability Test started in 1994, but with a slightly different format. Hence, data from this test are available only since 1995.

schools: privately-founded and public schools, as well as single-sex and coeducational schools. High schools also varied in size. All schools operated under similar centralized policies regarding fees and tuition, curriculum, and the qualifications and salary schedules of teachers. Principals controlled daily operations and the allocation of budget and other school resources. All general high schools, regardless of their type, were subject to random student assignment in the corresponding district. Because the majority of general high schools were single-sex, random assignment of students to high schools was performed separately for boys and girls. Students had to accept the randomly assigned school unless they moved to a different school district. If that district was also subject to the equalization policy, these students would be allocated by random assignment. Although it was possible for students not to comply with the random assignment through geographical mobility, evidence suggests that non-compliance was very limited (Park et al., 2013; Park, 2013).

There was substantial variation across public and privately-founded schools regarding personnel matters. In public schools, teachers were government employees who were hired in a centralized fashion based on their performance on a standardized exam. Teachers had to move to a different school within the administrative division every four or five years. Principals in public schools were selected by the regional educational office and could remain in their position at most for two four-year terms. Thus, public schools did not have any discretion regarding the selection of teachers and principals. In contrast, there was substantial discretion regarding personnel decisions in privately-founded schools. The school's board of directors was responsible for the appointment and promotion of principals. Principals in turn had control over the hiring and dismissal of teachers, and the length of their contracts. Thus, privately-founded schools had discretion in the selection and dismissal of principals and teachers.

2.2 Data

We use data from three main sources. First, to assess how schools differ in their impacts on student learning, we use individual-level data from the College Scholastic Ability Test (CSAT). This is a high-stakes test required for entry to university, and performance has a major impact on students' subsequent educational prospects. The CSAT college entrance exams are taken by 99% of students attending general schools at the end of high school. We use CSAT data for the years 1995 to 1997, corresponding to students who were assigned to high schools between 1992 and 1994 before the equalization policy was partially reversed in some areas.[9] For each year, the CSAT data include information on gender, the identity of the high school attended by each student, the name of the administrative division, and the raw scores in each subject.

The structure of the CSAT exam is as follows: two-thirds of the exam are identical across the whole country. This common component assesses proficiency in Korean and English languages, as well as in part of the Mathematics curriculum. The remaining third is choice-based and tests proficiency in Science or Social Science or in the remainder of the Mathematics curriculum (depending on one's curricular focus). Our analysis focuses on test scores in Korean and English, as these scores are comparable across students, schools and districts. We normalize the raw score of each subject to have mean zero and standard deviation one in the full CSAT sample in each year. In addition, we construct a summary measure of academic achievement by averaging the two standardized subject scores, and re-standardized this variable so that it has mean zero and standard deviation one in the full CSAT sample. The CSAT administrative data contain information on the names of the school and the administrative division, but not on school type (single-sex, public) or school district. We merged this information from the annual statistics book of each administrative division.

Second, to examine the validity of the experimental design, we use individual-level data from the Korean Education Longitudinal Study (KELS). This is an annual longitudinal survey that has been conducted since 2005 by the Korea Educational Development Institute, a government-funded research institute. The first cohort of the KELS consists of 6,908 students in the first year of middle school in 2005. The student and school samples are drawn as a stratified random sample to reflect the national population of seventh-graders in middle schools.[10] Students sampled by KELS are administered a series of socio-demographic and school-related questionnaires. In each wave of KELS, student academic performance is measured by achievement tests for three subjects: English, Korean, and Mathematics. We consider a sample of students in the randomizing areas excluding Seoul.[11] Since the KELS data were collected starting in 2005, and the equalization policy started to be reversed in 1996, these data may include some areas where student assignment

[9] In Korea, the school year typically runs from March to February. Students take the CSAT test in November of a given year and graduate from high school in February of the following year. The CSAT data are coded by graduation year.

[10] In a first step, 150 schools are selected nationwide in consideration of the regional distribution of schools and students. In each school, 50 students from the target grade are drawn at random, while all students are drawn if there are fewer than 50 students in the target grade.

[11] As described in more detail in subsection 2.3, we do not include Seoul when using the CSAT data to estimate the variation of school effects among students that were randomly assigned to general high schools. Hence, for consistency, we also drop Seoul when checking for randomization.

was not fully random. KELS does not provide information on school districts, and hence we use the middle school as a proxy for the school district.[12]

Finally, to provide a benchmark for the main empirical analysis, we use data from the 2000 round of the Program for International Student Assessment (PISA). PISA is a standardized international assessment coordinated by the Organization for Economic Cooperation and Development that measures academic achievement of 15-year-old students in Mathematics, Reading, and Science every three years. We use these data to document how Korea compares to other countries with regard to: i) the degree of variation in academic achievement across schools; ii) the degree of heterogeneity in observed school inputs across schools; and iii) the degree of school autonomy in academic, personnel and budgetary decisions. The school population included in PISA consists of all schools that have at least one 15-year-old student attending the school. We use student-level weights to generate a representative sample at the national level. For consistency with the analysis using the CSAT and KELS data, we drop observations for vocational schools in constructing the PISA sample. After dropping observations for Canada (which did not provide a representative sample) and for Norway and Poland (which have missing values for measures of school autonomy), we are left with data for 40 countries.

2.3 Sample Construction

In this section, we describe how we construct the samples used in the main analysis to estimate the variation in school effects. We start with the individual-level data from CSAT for students taking exams in 1995-1997. We drop students in the CSAT data who were not taking the exam for the first time, as well as those in vocational and selective high schools. The resulting sample contains about 1,150,000 student observations. Additionally, we drop students attending schools with low enrollments of less than 100 students (although we check that our main results are robust to the inclusion of smaller schools). This restriction excludes small schools from remote areas and ensures a minimum sample size for estimating school effects. Imposing this restriction reduces the sample by 2.7%. Finally, we impose two minor constraints which leaves us with the working sample of 1,083,237 student observations over 1995-1997.[13]

We start by defining the "randomized sample," which we use to estimate pure school effects. This sample is composed of students who were randomly assigned to general high schools within

[12]Table A1 in the Appendix provides summary statistics on these data.

[13]First, we drop observations with inconsistent information regarding single-sex status. That is, we drop 83 girls attending all-boys schools. Second, we drop 586 students taking the exam in a city with only two schools.

districts, and for whom we have information on the composition of the school district. To construct this sample, we start by focusing on the 6 out of 17 major administrative divisions that randomized all students to high schools in the final round of the assignment process.[14] We then drop Seoul and Incheon because we do not have sufficient information to specify the universe of schools to which students can be randomly assigned.[15] In contrast, in the metropolitan cities of Busan, Daegu, Gwangju and Daejeon each student was randomized to the set of general high schools included in the corresponding school district, and we are able to determine the exact composition of all school districts in each of these metropolitan cities.

We define a "non-randomized sample" composed of students in the administrative divisions of Gangwon-do and Jeollanam-do that did not randomize any student in the second round of the admission process. The rest of the sample contains students in "mixed divisions," which failed to meet the conditions for inclusion in the randomized sample for varying reasons. Some divisions comprise both urban and rural areas and implemented random assignment in the former areas but not in the latter. For this reason, we restrict the randomized and non-randomized CSAT samples refer to the period 1995-1997. Figure 1 depicts the administrative divisions that compose each of these samples.

Table 1 reports summary statistics for the CSAT data in 1995-1997. Column (1) refers to the full CSAT data, while columns (2) and (3) report statistics for the randomized and non-randomized samples, respectively. The statistics in column (2) reveal that, in the randomized sample, about 71% of students were enrolled in privately-founded schools, 96% of students attended single-sex schools, and the average number of students enrolled in each schools was 540. These proportions are larger than in the full sample, and much larger than in the non-randomized sample, where only 42% of students were enrolled in privately-founded schools, about 69% of students attended single-sex schools, and the average number of students enrolled in each school was 374. They also reveal that students in the randomized sample tend to perform considerably better in the CSAT exams than students in the non-randomized sample.

[14] See Table A2 for a list of Korea's administrative divisions.

[15] In Seoul, students were randomly assigned to high schools for which the commuting time from their homes (using public transportation) was estimated not to exceed 30 minutes (Kim and Kim, 2015). For Incheon, we were unable to obtain the exact composition of school districts

2.4 Validity of the Experimental Design

The existence of universal random assignment of students to high schools in certain areas of Korea is well documented in the existing literature. To provide some statistical evidence on the validity of the experimental design, we use individual-level data from the KELS data. We use these data to examine whether middle school test scores and household socio-demographic attributes predict observed characteristics of the high school that students attend. We estimate the following equation:

$$y_{im} = \alpha + \beta X_{im} + \phi_m + \varepsilon_{im} \tag{1}$$

where i indexes the individual student and m the middle school she attended; y_{im} is a different observed high school attribute in each regression (that is, a dummy variable indicating whether the high school the individual is assigned to is privately-founded or single-sex, the total school enrollment and the average class size); X_{im} is a vector of observed attributes of the student or the corresponding household, notably the test scores in middle school, parental education and income, and family size; ϕ_m are fixed effects for the middle school of origin; and ε_{im} is the error term, which is clustered at the middle school level. Given random assignment of students to high schools within districts, we would not expect to observe a systematic association between the attributes of students and the corresponding high schools.

It is important to note that the KELS data have three limitations for the purpose of confirming that students in the randomized sample in CSAT were randomly assigned to general high schools within districts. First, as noted above, KELS began in 2005, and the equalization policy started to be reversed in 1996. Hence these data may include some areas where student assignment was not fully random. Second, KELS does not identify each administrative division but instead contains information on whether students are in a randomized area. Hence we are including all students in randomizing areas. Third, we cannot include district fixed effects because schools are anonymized in the available KELS data, although we addressed this issue by adding middle school origin dummies (students attending the same middle school typically belong to the same high school district).

Table 2 reports the estimation results for randomized and non-randomized samples, respectively, separately by girls (panel A) and boys (panel B). The point estimates in column (1) show that students assigned to privately-founded and public schools tend to have similar test scores in middle school, similar levels of parental education and household income, and to be from families of similar size. Similarly, columns (2)-(4) show that these student attributes are also unrelated to class size, enrollment, and whether the high school is a single-sex school. The sole exception concerns parental education, which has a positive and weakly significant coefficient for enrollment in the case of girls.

In contrast, the analogous results for the non-randomized sample suggest that student characteristics do predict some of the attributes of the high schools that students end up attending. Most notably, columns (6)-(8) reveal that test scores in middle school are systematically related with school attributes in non-randomized areas, where student admission is determined by entrance exams, middle school test scores, or both. This can also be observed by the higher R-squared values for the non-randomized sample than the randomized sample. Even if this sample might contain some contamination, it is reassuring that we do not observe a systematic association between observed attributes of students and the corresponding high schools.

3 Estimation Strategy

This section presents the empirical models used to examine whether schools differ in the effects they have on student learning. In line with the literature on classroom and teacher effects (Chetty et al., 2011; Araujo et al., 2016), we assess the role of pure school effects in shaping academic achievement by estimating an equation of the form:

$$y_{ids} = \gamma_d + \phi_s + \varepsilon_{ids} \tag{2}$$

where y_{ids} is the test score for student i from school district d and school s, γ_d is a district fixed effect, ϕ_s are school effects, and ε_{ids} is the error term. Randomization pools are defined by the interaction of gender, district and year. We will therefore estimate separate regressions by gender and year.

The district fixed effects γ_d account for the heterogeneity of students across districts, which might be expected to affect test scores. Because students are randomly assigned to schools within each district, student baseline ability should be orthogonal to ϕ_s. In this framework, ϕ_s are pure school effects, i.e., bundles of all school-level attributes (observed and unobserved) that vary within districts, including, among others, learning time, school resources and curriculum, attributes of principals and teachers, organizational practices and school culture. To assess whether pure school effects matter for achievement, we estimate (2) using a fixed effects specification for ϕ_s. Following Araujo et al. (2016), we focus on estimating $V(\phi_s)$, where $V(.)$ indicates variance. Note that the variation in school effects within districts could be lower than the variation in school effects across the entire country.

A complication arises because $V(\phi_s)$ overestimates the true variance of the school effects because of sampling error. Again, in line with this literature, we subtract out a term that corrects for over-dispersion due to sampling error in the fixed effects, thereby obtaining the appropriately *shrunken* school effects. For studies examining teacher effects, the adjustment for measurement error tends to reduce the estimates considerably. Given that we have a relatively large number of students per school in our setting, this adjustment only leads to a minor reduction in our estimates.

Exploiting the orthogonality condition, we can also examine the influence of a vector of observed school attributes, X_s on test scores, by estimating an equation of the form:

$$y_{ids} = \alpha + \gamma_d + \beta X_s + \varepsilon_{ids} \tag{3}$$

where the observed school attributes are measured in the corresponding year of observation and the remaining variables have the meaning defined above. In this case, the estimation does not include school fixed effects.

An alternative approach uses random effects to estimate school effects. Assuming that these random effects are normally distributed, we can compute the standard deviation of school effects. These estimates are presented in Section 5.2, and they are very similar to those estimated using fixed-effects models.

4 Results

4.1 Main Results

We begin by comparing the estimates of school effects on academic achievement between randomized and non-randomized Korean high school districts using the fixed-effects methodology described in the previous section. Table 3 presents these estimates of school effects for randomized and non-randomized districts, showing separate estimates by gender (Panels A and B), the two compulsory subjects on the high-stakes college entrance exams (English and Korean), and for each year of the sample period (1995, 1996 and 1997). The results reveal a striking difference between the estimates of school effects for randomized and non-randomized districts.

In school districts where students are randomly assigned to general high schools, the estimated school effects on academic achievement range from 0.03 to 0.10 standard deviation units. These estimates capture school effects on academic achievement without the confounding effects of student sorting. Moreover, the fact that that all students are randomly assigned implies that these effects do not reflect peer effects due to differences in peer quality of non-randomized students. In contrast, for school districts where students are not randomly assigned to general schools, the estimated school effects on academic achievement range from 0.52 to 0.66 standard deviation units. These substantially larger school effects reflect the variation in student attributes (including peer effects) as well as school effectiveness.

Looking across the different estimates for randomized districts in columns (1) to (3), we observe that the standard deviation of school effects are very similar across the years 1995, 1996, and 1997, respectively. They also appear to be quite similar by gender. On the other hand, the estimates of school effects are generally larger in English, ranging from 0.08 to 0.10, than in Korean, where they range from 0.03 to 0.06. This is consistent with the notion that schools have a stronger impact on proficiency in a foreign language, which is not commonly used at home or in everyday life, than on proficiency in the native language.

It is instructive to compare the magnitude of our estimates in randomized districts with the estimates reported by Deming (2014) for Charlotte-Mecklenburg, United States. Using a variety of non-experimental school "value-added" measures which include controls for demographics and prior test scores, Deming (2014) finds that the standard deviation of school effects ranges from 0.05 to 0.10. Interestingly, the magnitude of our estimates is also comparable to recent experimental evidence of teacher and classroom effectiveness in Ecuador. Araujo et al. (2016) assigned two cohorts of kindergarten students to teachers within schools with a rule that is as good as random, and find substantial classroom effects: a 1 standard deviation increase in classroom quality results in 0.11, 0.11, and 0.07 standard deviations in test scores in language, math and executive function. Earlier studies using quasi-experimental measures for the United States also find quantitatively similar estimates for teacher effects. For example, using large administrative data for the United States, Chetty et al. (2014b) report evidence that a 1 standard deviation improvement in teacher value added raises end-of-grade test scores by 0.10 standard deviations on average.[16]

[16] See Jackson et al. (2014) for a review of earlier non-experimental evidence documenting comparable estimates of "teacher effects".

4.2 Public versus Privately-Founded schools

An important consideration for interpreting our estimates of pure school effects is the extent they are influenced by institutional factors that potentially affect the variation in key school inputs, such as the ability to select principals and teachers. It is possible that public schools in Korea, characterized by regular rotation of teachers across schools, may be different from those prevailing in areas where there is more sorting of teachers to schools. To provide evidence on this issue, we compare the variation of pure school effects between public and privately-founded schools. In privately-founded schools, there is a school specific selection process of principals and teachers, who can either be dismissed or stay in the school for long periods of time depending on performance. By contrast, public schools do not have any discretion regarding selection of teachers and principals.

Table 4 reports the results for public and privately-founded schools in 1996. For girls, we see that the standard deviation of school effects is indeed larger in privately-founded schools than in public schools: 0.07 versus 0.05 (the magnitude of this gap is even greater in 1995 and 1997, in results not shown here). For boys, however, the evidence is less clear. In 1996, the standard deviation of school effects is larger in public schools than in privately-founded schools (but this pattern is reversed in 1995 and 1997). Interestingly, these results suggest that the variation in school effects documented in the main sample is broadly similar to that obtained in settings where there is considerably greater sorting of principals and teachers.

4.3 Effects of Observed School Attributes

The analysis above suggests that school effects matter for academic achievement. But which factors are able to explain the differences in effectiveness documented across schools? In Table 5 we examine whether observed school attributes are important drivers of academic achievement in college entrance exams. Column (1) estimates the model specified in equation (3) for areas with universal random assignment of students to high schools. For this sample, the results show relatively weak associations between observed high school attributes and performance on college entrance exams (with the exception of enrollment among girls). Column (2) presents point estimates from a similar analysis for the non-randomized sample. Here, the estimates suggest that girls and boys enrolled in public and large schools tend to perform substantially better in college entrance exams in non-randomized areas. Obviously, in the absence of random assignment, the variation in academic achievement across school types may simply reflect the heterogeneity in student composition across schools. Overall, these results suggest that, once student baseline attributes are balanced across schools, the observed attributes of schools in Korea have a limited impact on academic achievement.

5 Robustness

One important concern when comparing across randomized and non-randomized samples is that they differ in other dimensions. As noted earlier, the equalization policy leading to the random assignment of students to high schools was implemented only in urban areas. Therefore, we conduct an alternative analysis at the city level where we can adjust for differences in observed attributes across randomized and non-randomized cities. The downside of this approach is that we can only to compare average achievement across schools within cities rather than within districts (so our estimates are not completely clean). In addition, we conduct several robustness checks to verify that our estimates are robust to alternative specifications.

5.1 City-Level Comparisons

The city-level analysis includes three sets of cities: i) the four administrative divisions/cities used in the baseline analysis where all students attending general high schools were randomly assigned to schools within clearly delineated districts; ii) other cities that randomized students to schools, including Seoul and Incheon,[17] as well as cities in administrative divisions where only some cities featured student randomization; and iii) cities that did not randomize students to schools, including those in the two administrative divisions used in the baseline analysis that did not feature student randomization, as well as other cities that did not randomize students in administrative divisions where only some cities featured student randomization. We then restrict attention to cities with at least 4 schools for boys or girls in each respective year. Table A3 in the Appendix lists this initial set of randomized and non-randomized cities included in the city-level analysis.

We merge the CSAT data with city-level characteristics from the 1995 population census using the 1999 School Directory, which contains identifiers for both the high-school and the city.[18] Given potential concerns about the comparability of the randomized and non-randomized samples, we

[17] As explained in Section 2 we dropped Seoul and Incheon from the baseline analysis because we were unable to obtain the exact composition of school districts within these cities.

[18] We use CSAT data from 1997 to match city-level characteristics since it is the closest year in our sample to the 1999 school directory. Using other years yields similar results.

examine how these three sets of cities vary in terms of their 1995 characteristics. Figure 2 plots several city-level characteristics (fractions enrolled in school, under age 18, migrated, married, graduated from high school, and unemployed) against log population for the baseline randomized cities (red), the other randomized cities (black), and the non-randomized cities (gray). While most of the city-level characteristics appear similar across cities, there are striking differences in terms of log population: baseline randomized cities have almost uniformly higher population than non-randomized cities such that their distributions do not overlap at all. It is only cities among the additional randomized cities that have common support with the set of non-randomized cities.

Table A4 in the Appendix reports summary statistics on city characteristics for the baseline set of randomized cities, the set of non-randomized cities, as well as a sample of randomized cities within the common support of the non-randomized sample in terms of population (i.e., we restrict the set of randomized cities to those with populations within the range of population of the non-randomized cities). Columns (1), (2), and (3) show the mean city-level characteristics; columns (4) and (5) present the "raw" estimated differences between the randomized cities and the non-randomized cities; columns (6) and (7) present analogous results, but controlling for log city population. These results reveal that almost all of the differences between randomized and non-randomized cities are eliminated once we control for city population or restrict to the sample of common support.[19] Thus, in estimating school effects, we compare two sub-samples of randomized cities: the sample of cities within our baseline sample, and the sample of randomized cities which have common support with the non-randomized cities.

To provide a consistent analysis across all randomized and non-randomized cities, we do not include district fixed effects. Rather, we compare the average achievement of all schools within each city. As before, the estimates for randomized cities will represent pure school effects, albeit within cities rather than within districts; the estimates for non-randomized cities will reflect differences in student attributes and peer effects as well differences in school effectiveness. This approach makes it possible to provide estimates on the standard deviations of school effects for all cities in a consistent way and allows us to assess whether the large differences in the standard deviations of school effects between randomized and non-randomized areas documented in Table 3 can be attributed to the system by which students are allocated to high schools.

Figure 3 depicts the standard deviation of school effects for our three sets of cities, in each sub-sample by year and gender. Two clear patterns stand out. First, the standard deviation

[19] The exception is the percentage of married people, which remains higher in almost all of the specifications.

of school effects varies relatively little across cities featuring random student assignment to high schools.[20] Second, for any given level of population, the standard deviation of school effects tends to be much larger for cities with non-random assignment. Unlike in Figure 2, which displayed how city attributes varied with population, there is a stark difference in the magnitude of the standard deviation of school effects across cities with and without random student assignment whether considering the baseline randomized cities or the other randomized cities.[21]

Table 6 confirms the stark differences in the standard deviation of school effects across cities with and without random assignment.[22] Whether we look at the main randomized sample (corresponding to the districts in the main analysis) or the randomized sample in the common support of log population as the non-randomized sample, the patterns are similar. The standard deviation of school effects ranges from 0.04 to 0.13 standard deviation units in cities with random assignment, while the estimates range from 0.51 to 0.70 in cities with non-random student assignment. These estimates are similar to those from the main analysis presented in Table 3 in which we included district dummies within cities. Taken together, these findings suggest that differences in the variation of schools effects across cities can be attributed to the system of allocation of students to high schools.

5.2 Sensitivity Checks

We conduct a couple of sensitivity checks to verify that the general pattern of these estimates is robust to alternative specifications. First, we consider estimates from fixed-effects models that do not employ an Empirical Bayes shrinkage procedure. Second, we estimate school effects using a random-effects estimator instead of a fixed-effects estimator. These results are shown in Table A5 in the appendix. As expected, given the relatively large number of observations per school, whether or not we apply the Empirical Bayes shrinkage procedure does not alter the estimates much. The magnitudes of the school effects are also similar when using random effects. Thus, the striking difference between the estimates for randomized and non-randomized districts remains under these alternative specifications.

[20] Among cities with random assignment, the standard deviation of schools effects tends to be larger in Seoul.

[21] Among the cities in the non-randomized sample, the city of Iksan has a relatively low standard deviation of school effects. This may be because Iksan was created in 1995 with the merger of Iri city and Iksan gun, and the former city featured random assignment between 1980 and 1990.

[22] These results are based on 1997 CSAT data, but the patterns are similar when using data from 1995 or 1996

6 External Validity

To gain insight into the external validity of our estimates, we compare our baseline results with those obtained from a broad sample of countries that participated in the 2000 round of the PISA test. We restrict our attention to reading scores in the PISA 2000 data, because not all students took the math and science test.[23] For consistency with the analysis of our CSAT data, we normalize the test scores so that the mean is zero and the standard deviation is one in each country. Of course, in the absence of random assignment to schools, the estimated school effects from PISA reflect what we are calling "pure" school effects, as well as student sorting and peer effects.

Table 7 reports results based on student-level data from the PISA 2000 for each country. The standard deviation of school effects varies from 0.23 for girls in Finland and 0.28 for boys in Sweden up to 0.83 in Austria and 0.82 in Hungary for girls and boys, respectively. Korea is characterized by low to intermediate levels in the variation of school effects (0.35), above Finland, Sweden, New Zealand, Iceland and Ireland, and just below Spain, Denmark and Australia. Note, however, that the variation in school effects for Korea combines schools in randomizing and non-randomizing areas. Overall, the variation of school effects observed in the cross-section of countries surveyed in PISA 2000 appears to be of a similar order of magnitude to those in non-randomizing districts based on our CSAT data.

We explore whether the variation of school effects in Korea can be representative of other countries by performing two additional analysis using data from PISA 2000. First, we examine how the degree of heterogeneity in observed school inputs in Korea compares with that of other countries. This can shed some light on the extent to which the variation in school effects documented in Korea may be generalized to other contexts. For example, if schools in Korea exhibit smaller heterogeneity in observed inputs than schools in other countries, we might expect greater variation in school effects in other countries. Second, we document the degree of school autonomy with regards to academic, personnel and budgetary decisions in Korea and in other countries. We would expect greater variation in school effects in educational systems for which schools have greater autonomy in key management decisions.

To examine the heterogeneity in observed school inputs, we consider the following indicators: annual hours in school, school size, student-teacher ratio, computer-student ratio, proportion of

[23] The Mathematics test was taken by 50% of students, as was the Science test. Only 25% of students took tests on the three subjects.

teachers certified and qualified, additional support to students, private versus public ownership, and single-sex versus coeducational schools.[24] Table 8 reports results from this analysis for each observed school input considered. Column (1) reports statistics for Korea, while the remaining columns present simple averages for countries in Europe, Asia and Oceania, and the Americas. The results reveal that Korea has a relatively high degree of heterogeneity in several school inputs, most notably teacher-student ratio, annual hours in school, and indicators for whether the school is privately-owned or single-sex. By contrast, Korea displays low heterogeneity with regard to additional services to students, as well as in the share of teachers certified or qualified. On average, Korea displays a degree of heterogeneity in observed school inputs that is in line with other countries in Asia and Oceania, higher than Europe and lower than the Americas.

To examine the degree of school autonomy across countries, we follow Hanushek et al. (2014) who compute measures of autonomy along different dimensions at the country-level. In particular, we use data on 6 questions (measuring school autonomy on defining courses, content, textbooks, hiring policies, salaries and budget) to quantify school autonomy along three dimensions: academic, personnel and budgetary. For each country, we compute averages across schools, thereby obtaining country-level measures of autonomy. Table A6 reports the results on these measures for each dimension of autonomy. The results reveal that the degree of school autonomy in Korea is higher than in Europe and the Americas, while below that observed in the rest of Asia and Oceania. While schools in Korea have relatively low autonomy with regard to personnel matters, they have relatively high autonomy over academic and budgetary matters.

Figure 4 summarizes these findings by depicting Korea's relative position in the cross-section of countries with regard to the heterogeneity in observed school inputs and school autonomy. It reveals that the values for Korea are close to average as compared to these 40 countries in terms of both heterogeneity in school inputs and school autonomy. Thus, the evidence presented in this section suggests that the key findings of this paper are likely to apply more broadly across the world.

[24] We examine the degree of heterogeneity in each of these school inputs by computing the standard deviation of each indicator across schools for each country, ranking countries by the standard deviation of the indicators, and calculating the corresponding percentiles.

7 Conclusion

Since the highly influential *Coleman Report*, researchers and policymakers have devoted considerable effort to assessing the importance of student, school and peer effects in shaping educational outcomes. However, in the absence of universal random assignment of students to schools, it is difficult to cleanly unpack the effects of schools on academic achievement associated with inputs that can be directly manipulated by schools.

In this paper, we exploit universal random assignment of students to high schools within school districts in Korea to circumvent this difficulty. We present quasi-experimental evidence on whether and how much "pure" schools effects matter for academic achievement. By estimating these school effects for scores in high stakes college entrance exams in areas characterized by universal random assignment of students to high schools, we confirm that schools vary considerably in their effectiveness. A 1 standard deviation increase in school quality within a district results in 0.06 to 0.08 standard deviation higher test scores in language, on average. The estimates are similar for boys and girls, and tend to be higher in English (up to 0.10 standard deviation) than in their native Korean (0.06 standard deviation). Interestingly, the magnitude of our estimates is comparable to non-experimental estimates of school value added in Charlotte-MecKlenburg (Deming, 2014), as well as to experimental and quasi-experimental estimates of classroom and teacher effectiveness in the United States and Ecuador (Chetty et al., 2014a; Jackson et al., 2014; Araujo et al., 2016).

We also show that the standard deviation in school effects within randomizing areas is only one tenth of that observed in comparable areas that allow for student selection. Moreover, the variation in pure school effects is not systematically larger in privately-founded schools, which have discretion to select principals and teachers. In the cross-section of countries, schools in Korea exhibit a relatively high degree of autonomy and heterogeneity in observed school inputs. Taken together, this evidence suggests that our main results are likely to apply more broadly.

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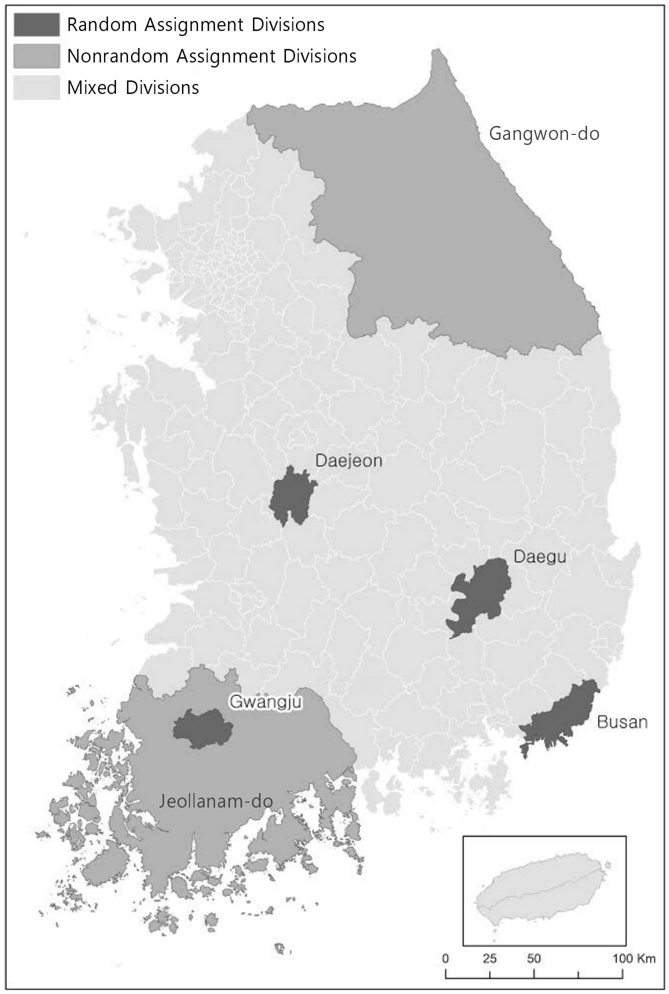
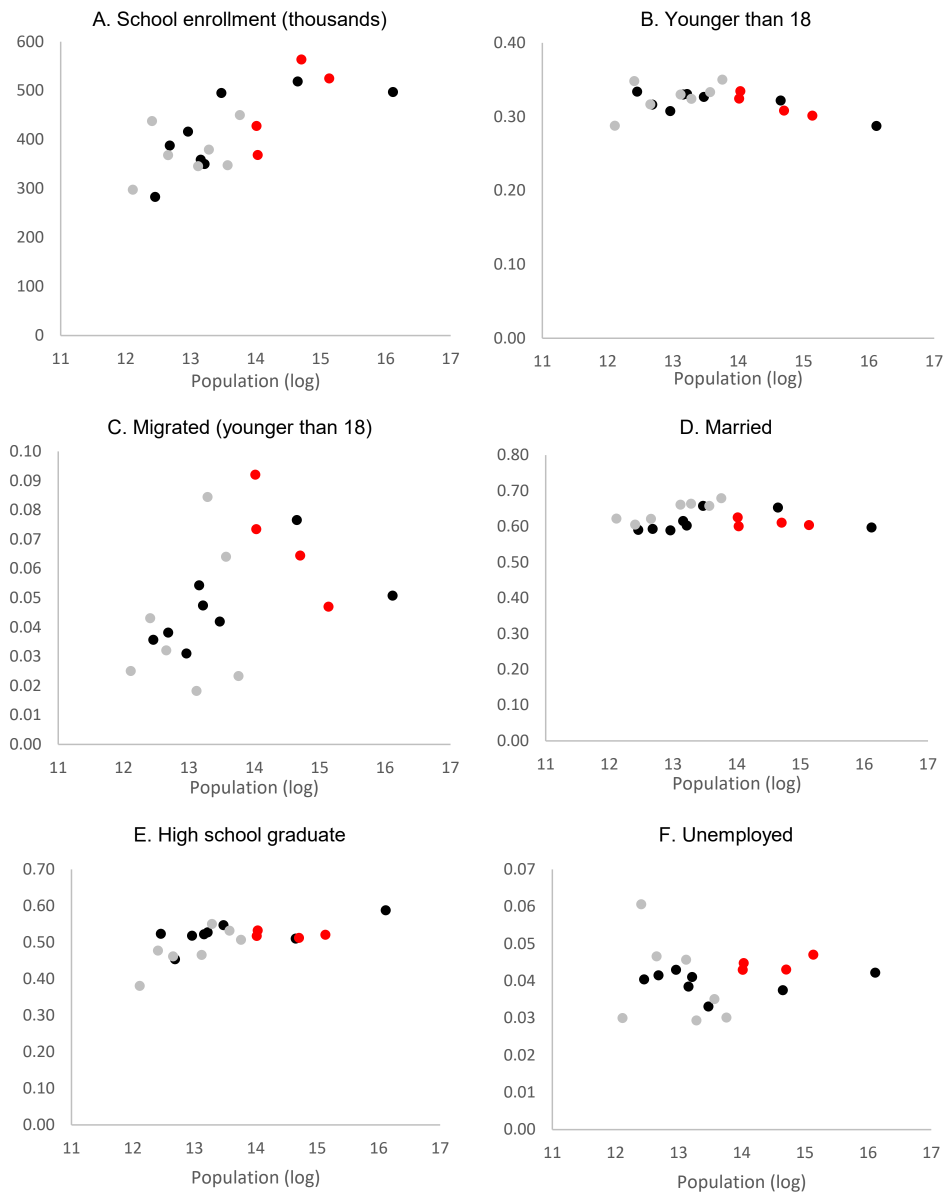
Figure 1: Randomizing and Non-Randomizing Administrative Divisions



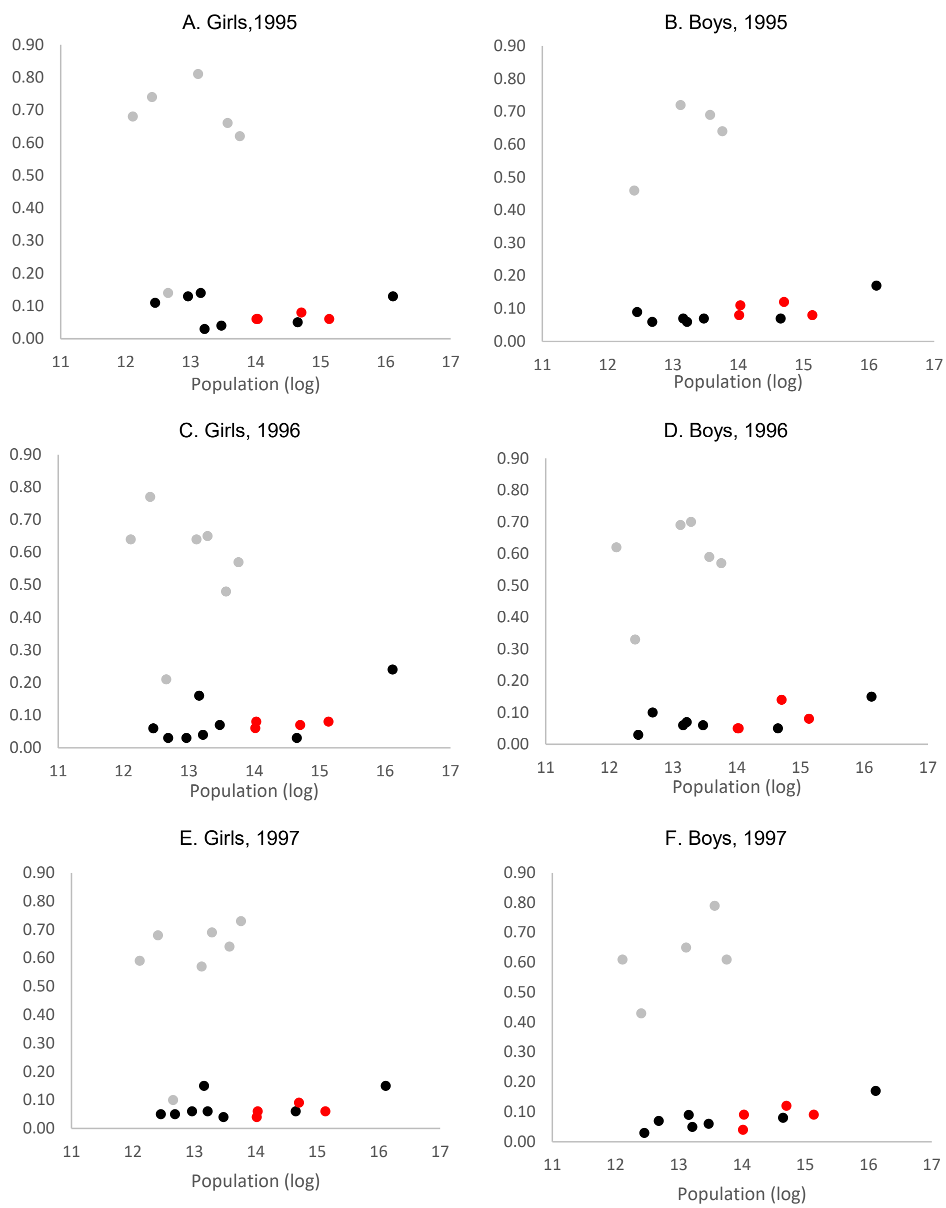
Notes: The main random assignment sample is composed of the metropolitan cities of Daejeon, Daegu, Busan and Gwangju. The main nonrandom assignment sample is composed of the administrative divisions of Gangwon-do and Jeollanam-do.

Figure 2: Average Characteristics of Cities



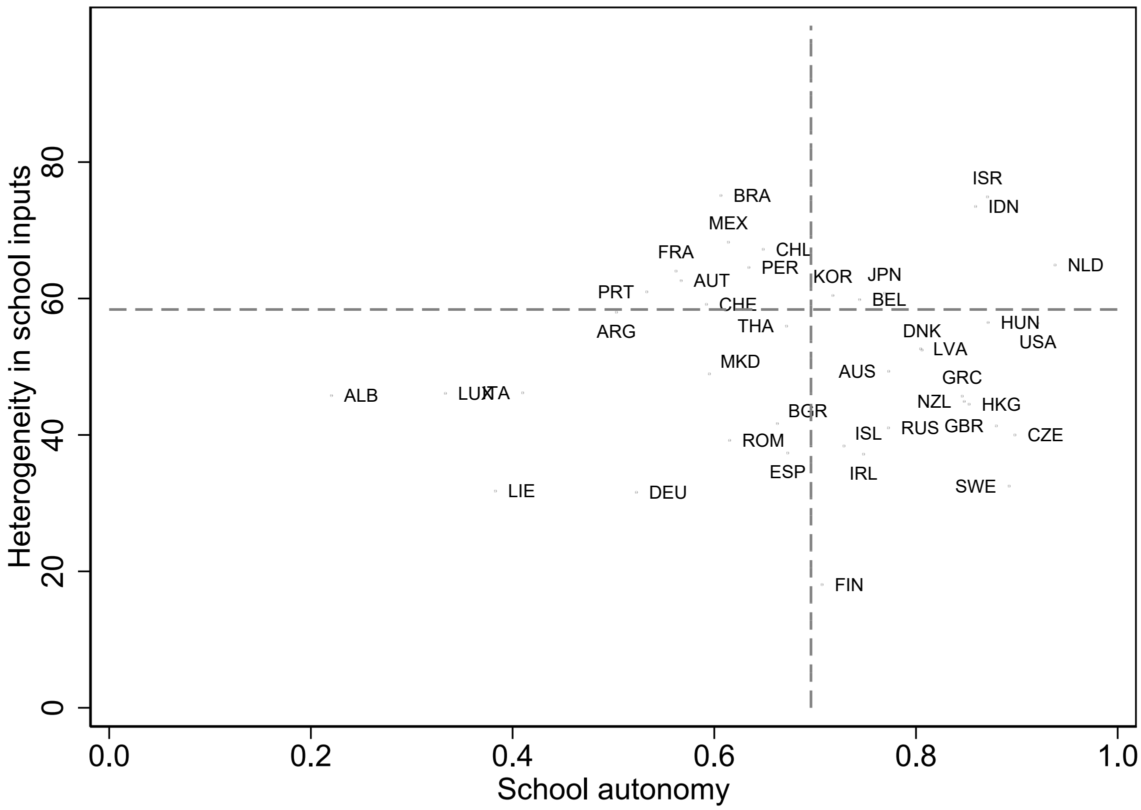
Notes: City-level population and other characteristics were obtained from the 1995 population census. Each point represents one city. Only cities with at least 4 high schools attended by girls in 1997 are included in the analysis. Red dots correspond to cities in the random assignment administrative divisions (those used in the main analysis in Tables 3, 4 and 6). Black dots correspond to other randomized cities. Gray dots correspond to non-randomized cities.

Figure 3: Standard Deviations of School Effects at the City Level



Notes: This figure presents standard deviation of school effects at the city level based on the 1995-1997 CSAT. City-level population was obtained merging the CSAT data with the 1995 population census using the 1999 School Directory (that contains high school identifier and city identifier). Each point represents one city. Only cities with at least 4 high schools attended by girls in 1997 are included in the analysis. Red dots correspond to cities in random assignment administrative states (these cities were used in the main analysis presented in Tables 3, 4 and 6). Black dots correspond to other randomized cities. Grey dots correspond to non-randomized cities. Standard deviations of school effects are computed following the methodology described in Section 3.

Figure 4: Heterogeneity of School Inputs and School Autonomy across Countries



Notes: This figure reports measures of i) the degree of heterogeneity in observed school inputs across schools and ii) measures of school autonomy for 40 countries based on data from PISA 2000. The dashed lines identify the average for each dimension. For consistency with the analysis using CSAT and KELS, the estimation sample from PISA does not include observations for vocational schools.

Table 1: Summary Statistics (CSAT data)

	All	Randomized sample	Non-randomized sample
	(1)	(2)	(3)
A. Girls			
School characteristics			
Privately-founded	0.62	0.71	0.42
Single sex	0.80	0.96	0.69
Enrollment	505.41	540.40	374.40
Mean test scores			
Average	0.08	0.41	-0.14
Korean	0.09	0.37	-0.14
English	0.06	0.39	-0.12
N (students)	482,281	100,957	33,860
B. Boys			
School characteristics			
Privately-founded	0.65	0.70	0.58
Single sex	0.79	0.97	0.74
Enrollment	511.89	527.97	377.04
Mean test scores			
Average	-0.07	0.17	-0.26
Korean	-0.08	0.14	-0.27
English	-0.05	0.17	-0.21
N (students)	600,956	132,539	38,513

Notes: The sample includes students who graduated from high school in 1996 and took the CSAT in that year. Students attending selective and vocational schools are excluded. Schools with less than 100 students are dropped. Randomized sample includes students in the administrative divisions who satisfy these conditions: i) students are randomized to schools in all school districts; ii) there are no distance-based rules that introduce exemptions to the randomization (i.e., Seoul is dropped); and iii) we were able to identify the sample of schools included in each school district (i.e., Incheon is dropped). The randomized sample includes these administrative divisions: Busan, Daegu, Gwangju, and Daejeon. The non-randomized sample includes the administrative divisions in which there is no school district in which students are randomized to schools. This sample includes these administrative divisions: Gangwon-do and Jeollanam-do. Average test scores are computed using scores in English and Korean.

Table 2: Randomization Tests (KELS data)

	Randomized sample				Non-randomized sample			
	Private	Single-sex	Class size	Enrollment	Private	Single-sex	Class size	Enrollment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Girls								
Scores in middle school	-0.03	-0.01	0.07	0.00	-0.00	0.05	0.47*	0.06**
	(0.03)	(0.02)	(0.15)	(0.01)	(0.04)	(0.04)	(0.23)	(0.02)
Parents' education	-0.00	0.01	0.10	0.01*	0.01	-0.01	-0.01	-0.00
	(0.01)	(0.01)	(0.06)	(0.01)	(0.01)	(0.01)	(0.06)	(0.01)
Log household income	0.00	0.02	-0.00	0.00	-0.03*	-0.01	0.00	0.00
	(0.01)	(0.02)	(0.08)	(0.01)	(0.01)	(0.02)	(0.05)	(0.01)
Family size	0.03	0.04	0.15	0.01	0.01	-0.00	0.28	0.01
	(0.04)	(0.04)	(0.21)	(0.02)	(0.02)	(0.02)	(0.26)	(0.02)
R-square	0.47	0.45	0.45	0.51	0.50	0.65	0.89	0.84
N students	499	499	499	499	498	498	498	498
B. Boys								
Scores in middle school	0.02	0.04	0.15	0.01	0.04	0.07**	0.66**	0.06**
	(0.03)	(0.03)	(0.16)	(0.01)	(0.03)	(0.03)	(0.17)	(0.01)
Parents' education	-0.01	0.00	0.07	0.00	0.02	-0.01	0.04	0.01**
	(0.01)	(0.01)	(0.07)	(0.00)	(0.01)	(0.01)	(0.03)	(0.00)
Log household income	0.04	0.00	-0.15	-0.01	-0.03*	0.02	0.17	0.00
	(0.04)	(0.03)	(0.24)	(0.02)	(0.01)	(0.01)	(0.14)	(0.01)
Family size	0.04	0.02	0.14	0.01	-0.01	0.02	0.20	0.03*
	(0.05)	(0.02)	(0.16)	(0.01)	(0.03)	(0.02)	(0.14)	(0.01)
R-square	0.22	0.50	0.54	0.57	0.49	0.59	0.87	0.87
N students	481	481	481	481	596	596	596	596

Notes: The sample consists of individual-level data on test scores from KELS, which collected information from seventh-graders from 150 middle schools in 2005 and followed them over time. High school characteristics (privately-founded, single-sex, class size and enrollment) were extracted from wave 4 collected in 2008. Class size is calculated for the whole school. Enrollment is also for whole school and is divided by 1,000. Data on test scores in middle-school, log household income and family size come from wave 3 (collected in 2007). Data on parental education are only available in wave 1 (collected in 2005). KELS does not provide information on school district. Middle-school is used as a proxy for the school district. Standard errors are clustered by middle-school. ** significant at 1% level; * significant at 5% level.

Table 3: Standard Deviation of School Effects

	Randomized sample			Non-randomized sample		
	1995	1996	1997	1995	1996	1997
	(1)	(2)	(3)	(4)	(5)	(6)
A. Girls						
Average	0.06	0.07	0.06	0.66	0.63	0.62
English	0.10	0.10	0.09	0.64	0.61	0.60
Korean	0.04	0.06	0.03	0.61	0.55	0.56
N students	33,711	33,175	34,071	10,774	11,454	11,632
N schools	67	68	69	45	47	48
N districts	9	9	9	9	10	10
B. Boys						
Average	0.08	0.07	0.07	0.53	0.52	0.55
English	0.09	0.08	0.10	0.49	0.52	0.52
Korean	0.06	0.06	0.05	0.50	0.46	0.50
N students	44,999	43,814	43,726	11,800	13,387	13,326
N schools	90	90	91	54	56	56
N districts	9	9	9	9	10	10

Notes: Estimates are based on individual-level data on test scores in CSAT exams for high-school graduates in the randomized sample described in Table 1. Column titles indicate the year used for the analysis. Average corresponds to the mean of English and Korean test scores. Standard deviations of school effects are computed following the methodology described in Section 3.

Table 4: Standard Deviation of School Effects by School Type, 1996

	Randomized sample		**Non-randomized sample**	
	Privately- founded	Public	Privately- founded	Public
	(1)	(2)	(3)	(4)
A. Girls				
Average	0.07	0.05	0.43	0.66
English	0.11	0.07	0.42	0.63
Korean	0.06	0.04	0.39	0.58
N students	23,636	9,539	4,711	6,743
N schools	47	21	18	29
N districts	9	9	7	10
B. Boys				
Average	0.06	0.08	0.41	0.55
English	0.08	0.09	0.41	0.55
Korean	0.05	0.07	0.37	0.48
N students	30,679	13,135	7,799	5,588
N schools	61	29	26	30
N districts	9	9	8	9

Notes: Estimates are based on individual-level data on test scores in CSAT exams for high school graduates in 1996 in the randomized sample described in Table 1. Column titles indicate sub-samples used for the analysis. Standard deviations of school effects are computed following the methodology described in Section 3.

Table 5: Associations of School Characteristics and Academic Achievement, 1996

	Randomized sample	Non-randomized sample
	(1)	(2)
A. Girls		
Privately-founded	-0.01	-0.62***
	(0.02)	(0.18)
Single-sex	-0.06*	0.36
	(0.03)	(0.22)
Enrollment	0.32**	2.20***
	(0.13)	(0.71)
N students	33,175	11,454
R-square	0.007	0.259
B. Boys		
Privately-founded	-0.01	-0.63***
	(0.02)	(0.14)
Single-sex	0.02	0.04
	(0.04)	(0.13)
Enrollment	-0.06	2.97***
	(0.11)	(0.49)
N students	43,814	13,387
R-square	0.019	0.216

Notes: Estimates are obtained from regressions of individual-level average test scores in CSAT exams for high school graduates in 1996 on school characteristics. The randomized and non-randomized samples are described in Table 1. Test scores are averages of English and Korean. Regressions control for school district fixed effects. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 6: **Standard Deviation of School Effects by Subject (city-level analysis, main analysis sample, 1997)**

	Randomized sample (Main)	**Randomized (Common Support)**	**Non-randomized sample**
	(1)	(2)	(3)
A. Girls			
Average	0.06	0.07	0.57
English	0.04	0.06	0.51
Korean	0.11	0.10	0.64
N students	33,344	12,799	14,504
N schools	66	32	40
B. Boys			
Average	0.06	0.07	0.57
English	0.06	0.04	0.67
Korean	0.13	0.09	0.70
N students	43,160	12,377	17,683
N schools	89	33	48

Notes: This table presents standard deviation of school effects at the city level for students participating in the 1997 CSAT. Only cities with at least four high schools are included in the analysis. Standard deviations of school effects are computed following the methodology described in section 3. Average corresponds to the mean of English and Korean test scores.

Table 7: Standard Deviation of School Effects, PISA 2000

	Girls	Boys		Girls	Boys
Finland	0.23	0.33	Israel	0.56	0.65
Sweden	0.27	0.28	Portugal	0.56	0.59
New Zealand	0.31	0.40	Hong Kong	0.57	0.67
Iceland	0.32	0.29	Luxembourg	0.58	0.60
Ireland	0.34	0.42	Brazil	0.60	0.67
Korea	0.35	0.38	Argentina	0.60	0.60
Spain	0.37	0.47	Romania	0.61	0.60
Denmark	0.37	0.41	United Kingdom	0.61	0.72
Australia	0.40	0.43	France	0.63	0.71
Latvia	0.46	0.52	Netherlands	0.63	0.65
Greece	0.46	0.66	Czech Republic	0.64	0.67
United States	0.47	0.56	Belgium	0.64	0.72
Thailand	0.49	0.55	Chile	0.65	0.68
Albania	0.50	0.60	Italy	0.65	0.72
Russia	0.50	0.60	Mexico	0.67	0.68
Liechtenstein	0.53	0.68	Peru	0.69	0.69
Japan	0.53	0.66	Bulgaria	0.69	0.79
Macedonia	0.54	0.61	Hungary	0.71	0.82
Indonesia	0.55	0.52	Germany	0.71	0.68
Switzerland	0.55	0.58	Austria	0.83	0.78

Notes: This table reports the standard deviation of school effects for 40 countries. School effects are estimated using individual-level data on test scores in reading from PISA in the year 2000. For consistency with the analysis using CSAT and KELS, the estimation sample from PISA does not include observations for vocational schools. Standard deviations of school effects are computed following the methodology described in Section 3.

Table 8: Heterogeneity in Observed Inputs across Schools in PISA 2000

	Korea	Europe	Asia-Oceania	Americas
	(1)	(2)	(3)	(4)
Annual hours in school	80.0	39.1	62.8	88.3
School size	62.5	42.8	52.8	85.8
Teacher-student ratio	77.5	54.6	43.4	47.1
Computer-student ratio	72.5	51.8	55.0	43.8
Private school	100.0	41.9	70.3	69.7
Coed school	95.0	40.2	83.1	56.7
Additional services to students	51.3	41.0	59.9	82.5
Teachers certified (%)	2.6	54.1	39.5	56.1
Teachers with qualifications (%)	2.6	48.4	58.0	54.3
Average	60.4	46.0	58.3	64.9

Notes: This table reports measures of heterogeneity in observed school inputs in Korea and in different regions of the world based on school-level data for 40 countries from PISA in the year 2000. Heterogeneity measures in columns (2)-(4) correspond to the simple average of the heterogeneity measures for the corresponding countries.

Table A1: Summary Statistics, KELS Data Set

	Randomized Sample (1)	Non-Randomized Sample (2)
A. Girls		
Parents' education	13.22	12.76
Log household income	12.71	12.71
Number of siblings	2.21	2.34
Test scores in middle-school	0.42	0.22
Single-sex	0.71	0.36
Privately-founded	0.49	0.38
Enrollment	1.35	1.00
Class size	37.09	34.46
N (obs.)	499	498
B. Boys		
Parents' education	13.42	12.76
Log household income	12.83	12.73
Number of siblings	2.11	2.21
Test scores in middle-school	0.07	-0.02
Single-sex	0.72	0.32
Privately-founded	0.54	0.38
Enrollment	1.22	0.95
Class size	35.68	33.68
N (obs.)	481	596

Notes: Sample consists of individual-level data from KELS, which collected information from seventh-graders from 150 middle schools in 2005 and followed them over time. High school characteristics like privately-founded, single-sex, class size and enrollment were extracted from wave 4 collected in 2008. Class size is calculated for the whole school. Enrollment is also for whole school and divided by 1,000. Data on test scores in middle school, log household income and family size come from wave 3 (collected in 2007). Data on parental education are only available in wave 1 (collected in 2005).

Table A2: Population of Major Administrative Divisions, 1997

Name	Administrative division status	Population
Seoul	special city	10,389,057
Busan	metropolitan city	3,865,114
Daegu	metropolitan city	2,501,928
Incheon	metropolitan city	2,460,906
Gwangju	metropolitan city	1,326,478
Daejeon	metropolitan city	1,323, 009
Ulsan	metropolitan city	1,013,070
Gyeonggi-do	province	8,514,716
Gyeongsangnam-do	province	3,058,479
Gyeongsangbuk-do	province	2,811,586
Jeollanam-do	province	2,166,247
Jeollabuk-do	province	2,007,379
Chungcheongnam-do	province	1,822,543
Gangwon-do	province	1,540,307
Chungcheongbuk-do	province	1,475,448
Jeju	special self-governing province	528,360

Notes: This table lists the administrative divisions of Korea and their population estimates in 1997. Population estimates are from the National Statistical Office of the Republic of Korea.

Table A3: Cities Included in the City-Level Analysis

City	Major administrative division
A. Randomized	
Seoul	Seoul
Incheon	Incheon
Busan^{B}	Busan
Daegu^{B}	Daegu
Gwangju^{B}	Gwangju
Daejeon^{B}	Daejeon
Cheongju^{cs}	Daejeon
Suwon^{cs}	Gyeonggi-do
Jeonju^{cs}	Jeollabuk-do
Masan^{cs}	Gyeongsangnam-do
Jinju^{cs}	Gyeongsangnam-do
B. Non-randomized	
Ulsan	Gyeongsangbuk-do
Bucheon	Gyeonggi-do
Anyang	Gyeonggi-do
Pohang	Gyeongsangbuk-do
Iksan	Jeollanam-do
Gyeongju	Gyeongsangbuk-do
Yeongju	Gyeongsangbuk-do
Mokpo	Jeollanam-do
Gunsan	Jeollabuk-do
Andong	Gyeongsangbuk-do

Notes: This table lists the set of randomized and non-randomized cities included in the city-level analysis.

^{B}: Cities in baseline analysis.

^{cs}: Cities in common support analysis.

Table A4: City Characteristics at the City Level

	Randomized baseline	Randomized common support	Non-randomized	Raw diff. baseline	Raw diff. CS	Adj. diff. baseline	Adj. diff. CS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Population (millions)	2.16	0.50	0.42	1.74**	0.09	0.46	-0.06
				(0.38)	(0.13)	(0.46)	(0.04)
School enrollment (thousands)	0.47	0.40	0.36	0.11*	0.04	0.00	0.02
				(0.04)	(0.03)	(0.05)	(0.03)
% Population younger 18	0.32	0.32	0.32	0.00	0.00	-0.03	0.00
				(0.01)	(0.01)	(0.02)	(0.01)
% Migrated (younger 18)	0.07	0.04	0.04	0.03**	0.01	0.02	0.00
				(0.01)	(0.01)	(0.02)	(0.01)
% High school graduates	0.52	0.51	0.46	0.06*	0.05	-0.05	0.02
				(0.03)	(0.03)	(0.03)	(0.02)
% Married	0.61	0.61	0.64	-0.03*	-0.03	-0.07***	-0.04***
				(0.01)	(0.01)	(0.02)	(0.01)
% Unemployed	0.04	0.04	0.04	0.00	0.00	0.01	0.00
				(0.01)	(0.01)	(0.01)	(0.01)
N (cities)	4	5	10	14	15	14	15

Notes: This table presents statistics on city characteristics and standard deviation of school effects at the city level. City-level characteristics were obtained merging the CSAT data with the 1995 population census using the 1999 School Directory (that contains high school identifier and city identifier). Only cities with at least four high schools attended by boys are included in the analysis. Standard deviations of school effects are computed following the methodology described in Section 3. Test scores are averages of English and Korean. Column (1) presents means for cities in random assignment administrative divisions (the cities used in the main analysis presented in Tables 3, 4 and 6). Column (2) presents means for all randomized cities. Column (3) presents means for non-randomized cities. Columns (4) to (7) present coefficients and standard errors from OLS regressions. Columns (4) presents estimated differences between the randomized cities included in the main analysis (column (1)) and the non-randomized cities (column (3)). Columns (5) presents estimated differences between all randomized cities (column (2)) and the non-randomized cities (column (3)). Results in column (6) and (7) present results analogous to columns (4) and (5) but controlling for log city population. Standard errors are reported in parentheses. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table A5: Alternative Specifications for Estimation of School Effects

	Randomized sample			Non-randomized sample		
	1995	1996	1997	1995	1996	1997
	(1)	(2)	(3)	(4)	(5)	(6)
A. Girls						
FE-Empirical Bayes	0.06	0.07	0.06	0.66	0.63	0.62
FE-unshrunken	0.07	0.08	0.06	0.66	0.65	0.63
Random Effects	0.06	0.07	0.05	0.64	0.58	0.60
N students	33,711	33,175	34,071	10,774	11,454	11,632
N schools	67	68	69	45	47	48
N districts	9	9	9	9	10	10
B. Boys						
FE-Empirical Bayes	0.08	0.07	0.07	0.53	0.52	0.55
FE-unshrunken	0.09	0.08	0.09	0.54	0.53	0.55
Random Effects	0.08	0.07	0.07	0.51	0.51	0.52
N students	44,999	43,814	43,726	11,800	13,387	13,326
N schools	90	90	91	54	56	56
N districts	9	9	9	9	10	10

Notes: Estimates are based on individual-level data on test scores in CSAT exams for high-school graduates in the randomized sample described in Table 1. Column titles indicate the year used for the analysis. Test scores are averages of English and Korean.

Table A6: Measures of School Autonomy

	Korea	Europe	Asia-Oceania	Americas
	(1)	(2)	(3)	(4)
Academic	0.97	0.69	0.95	0.82
Personnel	0.25	0.41	0.48	0.43
Budget	0.93	0.90	0.95	0.72
Average	0.72	0.67	0.80	0.65

Notes: This table reports measures of school autonomy in Korea and across different regions of the world based on school-level data for 40 countries from PISA in the year 2000. Autonomy measures in columns (2)-(4) are simple averages of autonomy measures in the corresponding countries.