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Why Are Single-Sex Schools Successful?

Abstract

We exploit two unusual policy features of academic high schools in Seoul, South Korea—random assignment of pupils to high schools within districts and conversion of some existing single-sex schools to the coeducational (coed) type over time—to identify three distinct causal parameters: the between-school effect of attending a coed (versus a single-sex) school; the within-school effect of school-type conversion, conditional on (unobserved) school characteristics; and the effect of class-level exposure to mixed-gender (versus same-sex) peers. We find robust evidence that pupils in single-sex schools outperform their counterparts in coed schools, which could be due to single-sex peers in school and classroom, or unobservable school-level covariates. Focusing on switching schools, we find that the conversion of the pupil gender type from single-sex to coed leads to worse academic outcomes for both boys and girls, conditional on school fixed effects and time-varying observables. While for boys, the negative effect is largely driven by exposure to mixed-gender peers at school-level, it is class-level exposure to mixed-gender peers that explains this disadvantage for girls.

JEL-Codes: I200, J160.

Keywords: gender, single sex schools, school inputs, random assignment.

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1. Introduction

As with most programs and policies evaluated outside the lab, schools come as a package: different schools differ in various observable and unobservable inputs that can affect student achievement (Behrman and Birdsall 1983; Card and Krueger 1996; Hanushek 1979, 1986; and Lazear 2001). However, unless the researcher can specifically vary one particular aspect of a school and only that (e.g., through (quasi-)experimental variation in class size as in Krueger (1999) and Angrist and Lavy (1999)), it is often difficult to isolate the effect of a single element inside the package called a "school." One context in which this evaluation problem is particularly salient is single-sex education, a policy tool that has been considered in many contexts and in many nations (see U.S. Department of Education (2005) for a review) due to its potential to close various types of gender gaps.[1]

Random or quasi-random assignment of pupils to single-sex versus coeducational schools can identify the *composite* or *total* effects of attendance at one school type over another. However, this parameter may not necessarily show us the effects of having same-sex (versus mixed-gender) peers if existing single-sex and coed schools differ also along other (unobservable) dimensions. Progress can be made when schools switch status, say from single-sex to coeducational type. However, even in this case, standard fixed effects or difference-in-differences (DiD) estimators may not identify the effect of class-level exposure to mixed-gender (versus single-sex) peers, but a mixture between this effect, and the effects of mixed-gender pupils at the school level and unobserved school-wide changes that go along with school type conversion.

[1] These include boys lagging behind girls in cognitive and non-cognitive achievements (Bertrand and Pan 2013; Fortin et al. 2013; Goldin et al. 2006; Jacob 2002) and the gender gap in mathematics (Fryer and Levitt 2010; Joensen and Nielsen 2016).

In this paper, we address this identification problem by exploiting various features of academic high schools (or "high schools" hereafter) in Seoul, South Korea. The first feature is random assignment of pupils to schools within school districts at each cohort. This allows addressing the problem of student self-selection into schools, which hinders analysis of school features on achievement. Second, we exploit the conversion of some existing single-sex schools to the coeducational type over time. This allows separating the schools' pupil gender type from unobserved (and time-invariant) school characteristics. Third, we use the fact that high schools in South Korea consist of three grades and the school-type conversion was done at the cohort level. This enables us to separate class-level exposure to mixed-gender (versus same-sex) peers from school-level exposure and potential unobserved changes instigated at school level that accompany the school type change. We do that by comparing two adjacent cohorts in switching schools, where one has been exposed to mixed-gender environment at both school- and class levels while the other had such exposure at school level only, and where both cohorts have been exposed to unobserved changes at school level that go along with school type changes.

This parameter, to the extent that we can eliminate the effects of school-level coed environment and other unobserved school-wide adjustments, shows the *net* effect of exposure to mixed-gender (versus single-sex) classroom environment. It is not the *pure* effect of mixed-gender pupils, as it includes endogenous responses of teachers and parents to the gender composition of the classroom (see Duflo et al. (2011) and Pop-Eleches and Urquiola (2013) for a discussion). However, what matters for policy is the *net* rather than the *pure* effect, as any policy that changes classroom type from single-sex to coed will necessarily induce endogenous responses, which—together with the *pure* effect—will form the basis for policy decisions.

To proceed, and as a benchmark, we first estimate the causal *total* (or *composite*) effect of attending one school type over another (i.e., single-sex versus coed) on achievement, making use of random assignment within school districts. This is a relevant parameter: for a parent considering sending her child to a single-sex school, this total effect is what matters. Using multiple waves of data for 1996-2009, we confirm the prior findings of Park et al. (2013) who estimated—for a single cross section (1999) of data for high school students in Seoul— the total effects: attending a coed (versus single-sex) school lowers achievements for both boys and girls by 4 to 10 percent, with similar estimates across subjects (which include Korean, English and Math). Interestingly, when we condition on a large array of observable school characteristics, the disadvantage of attending a coed school drops in magnitude and becomes statistically insignificant. This, however, does not imply that having mixed-gender (versus single-sex) peers has no effect on achievement. Rather, it suggests that schools' pupil gender type is correlated with observed school characteristics and test scores alike.

In the next step, we exploit the fact that due to a government policy that favored coeducation, some of the existing single-sex schools in Seoul converted to the coed type over the period, 1996-2009.[2] We first use the switching of schools to eliminate—in addition to time-varying school-level observables—time-invariant school-level unobservables, by comparing cohorts who had mixed-gender peers in a coed school environment with cohorts who had single-sex peers in a single-sex school environment. This parameter measures the effect of coed exposure at class (and school) level as well as unobserved school-level changes that accompany

[2] The changing schools were *not* randomly selected, and we do not know exactly what triggered their selection. However, this shall not affect the causal interpretation of the fixed effects estimates that exploit that variation as long as pupils' assignment to schools is (conditionally) random and selection is based on time-invariant school characteristics (observed and unobserved). We discuss this below.

the school type conversion. We find that the within-school estimates of single-sex to coed conversion are negative for both boys and girls.

We then proceed to analysis that makes use of the multiple cohort feature of high schools. Specifically, the first cohort admitted under the coed regime is exposed to mixed-gender peers at class (and school) level for three years as well as to potential changes in school-level inputs that we do not observe. The preceding cohort, while not being exposed to mixed-gender peers at class level, will also be exposed to school-level coed environment and any school-wide changes undertaken due to the switch, for the last two (out of the three) years of their high school experience. To the extent that school-level exposures to the newly coed environment affect the two cohorts similarly, the difference in attainment between the two cohorts (and the corresponding difference in non-switching schools) allows us to isolate the net effect of class-level exposure to mixed-gender (versus same-sex) peers. Our DiD estimates based on these adjacent cohorts show that for girls, class-level exposure to mixed-gender (versus same-sex) peers for three years leads to a significant negative effect on achievement. Specifically, as we exogenously change the share of girls in own cohort from 100 to around 50 percent, the achievement of girls in languages decreases by 8 to 15 percent of a standard deviation in the score distribution. For boys, however, the benefits of having same-sex (versus mixed-gender) peers at classroom level are small and statistically insignificant. These findings are invariant to the inclusion of time-varying school-level observables.[3]

Overall, our results suggest that while the effect of exposure to mixed-gender (versus same-sex) peers at class- and school level is negative for both boys and girls, the underlying

[3] The fact that controlling for observables makes little difference to estimates may corroborate the argument that unobservable differences are likewise small for the two adjacent cohorts compared in our diff-in-diff analysis (see Altonji et al. 2005a, 2005b for discussion).

mechanisms are different. For boys, the disadvantage is largely due to *school-level* coed environment whereas for girls, it is *class-level* exposure to mixed-gender (versus same-sex) peers that explains the disadvantage.

Existing studies estimate the effects of attendance at single-sex schools (Choi et al. 2014; Choi et al. 2015; Jackson 2012, 2016; Park et al. 2013) or the effects of being assigned to single-sex classes within a coed institution (Booth et al. 2013; Eisenkopf et al. 2015; Lee et al. 2014).[4] The first literature tends to find robust positive effects of attending a single-sex (versus a coed) school for both boys and girls whereas the second literature reports mixed findings on the benefits of single-sex (versus mixed-gender) classrooms for boys and girls, respectively. We contribute to both strands of literatures by estimating and contrasting three distinct causal parameters: the between-school effect of attending a coed (versus a single-sex) school; the within-school effect of school-type conversion, conditional on (unobserved) school characteristics; and the effect of class-level exposure to mixed-gender (versus same-sex) peers. By pointing out the school- and class-level coed environment as an explanation for the disadvantage of coed (versus single-sex) school attendance for boys and girls, respectively, we help consolidate the existing findings in the two strands of literatures above. Our approach also sheds light on understanding why existing single-sex schools may outperform their coed counterparts and its policy relevance: If the success is due to school-specific unobservables, there would be little scope for replicating the success elsewhere. As shown, however, a school's pupil

[4] The second aspect can also be related to the literature that examines the impact of having a larger or smaller share of girls in a coed classroom (Anelli and Peri 2013; Black et al. 2013; Hoxby 2000; Lavy and Schlosser 2011; Oosterbeek et al. 2014; Schneeweis and Zweimüller 2012; Whitmore 2005).

gender type—a variable in policy maker's choice set—is indeed capable of altering student outcomes: boys through school-level coed exposure and girls through class-level exposure.[5]

More broadly, this paper also speaks to the recent and growing literature (Angrist et al. 2013; Clark 2010; Clark and Del Bono 2016; Cullen et al. 2006; Deming 2010; Deming et al. 2014; Dobbie and Fryer 2013, 2015; Fryer 2014; Hahn et al. 2016) that tries to go beyond treatment effects and to understand the roles of specific elements that characterize high-performing schools. We add to this literature by examining the role of schools' gender type in specific while accounting for school-level unobservables in a previously unexplored research design.

The rest of the paper is organized as follows. In the next section, we provide some institutional details and describe our data. Section 3 discusses our empirical strategy whereas the results of our empirical analysis are presented in Section 4. Section 5 consists of some concluding comments.

2. Background and Data

2.1 High School Equalization Policy (HSEP) in Seoul

The random assignment of students to academic high schools (or "high schools" hereafter) *within* districts in Seoul has been well documented in prior research, see e.g., Park et al. (2013); Choi et al. (2014); Choi et al. (2015); and Hahn et al. (2016).[6] The policy traces its roots back to

[5] Again, we are referring here to the *net* effect that is inclusive of potential endogenous responses of pupils, parents and teachers to the pupil gender type rather than the *pure* effect of the pupil gender type, which in itself cannot be isolated based on a design that relies on random assignment into treatment unless agents' behavior can somehow be kept constant.

[6] In South Korea, while the curricula at the primary (grades 1-6) and the lower-secondary (grades 7-9) levels are uniform for all individuals, for upper-secondary (grades 10-12) level, students have to choose among three different types of high schools: academic, vocational, and special-purpose high schools. The primary objective of academic high schools is to prepare individuals for college admission and this is the default option for most students. As of 2009, 74 percent of South Korean high school students were enrolled in academic high schools as opposed to

the "High School Equalization Policy (HSEP)" that was instituted in 1974 by the South Korean government. Prior to that, students were admitted to high schools based on school-specific entrance exams. Under the exam-based regime, the hierarchy of high schools was quite evident and it was directly reflected in their performance in advancing their graduates into elite universities. With the rapid increase of population who pursued a high school education in the late 1960s and early 1970s, the competition for entry into elite high schools was intensified to the point of being deemed "unhealthy" by many. In order to "equalize" high schools, the government therefore mandated the abolition of exam-based sorting and instituted the HSEP, which randomly allocates middle school graduates among academic high schools within districts. First implemented in Seoul in 1974, the HSEP was expanded to other metropolitan areas subsequently.[7] Until its relaxation in 2010 (which affects the 12th graders in 2012)—which is outside our sample period (i.e., the 12th graders in 1996-2009)—the HSEP has been meticulously enforced in Seoul for over three decades.

The HSEP randomly allocates students to academic high schools according to the student's residential districts as of the final year of middle school (grade 9). The school lottery is computerized. *Conditional on* school districts, students cannot express preferences for a particular school or school type and no other information about the individual students is utilized. Results of the school lottery are revealed in February. Each student is assigned to one high school only. Students usually matriculate in that school (otherwise, the student will be left

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vocational or special-purpose high schools (Statistical Yearbook of Education, Korean Educational Development Institute, http://cesi.kedi.re.kr). The random assignment is used for academic high schools only, which are, therefore, the focus of our analysis. For vocational and special-purpose high schools, there is a separate admission process, which takes place prior to the lottery-based assignment to academic high schools.

[7] Kang et al. (2007), Kim et al. (2008) and Lee (2014) analyze the effects of moving from exam-based sorting to district-based random assignment.

with no school to attend). The school year then starts in March. For a fuller description of the HSEP and other practical details, please also see Park et al. (2013) and Hahn et al. (2016).

There are 11 school districts in Seoul.[8] Each district is large with 14 schools that boys can attend and 13 schools that girls can attend, on average. The assigned school can be single-sex or coeducational—previously studied in Park et al. (2013)—or public or private establishment type—previously studied in Hahn et al. (2016), etc. However, due to tight government regulations and heavy subsidies, the curriculum and tuition are common across schools or school types including "private" schools.[9] Also reflecting active government intervention, school resources such as pupil-teacher ratios and class size are highly comparable across schools, as shown below.

Of course, districts can differ in their average school quality due to historic reasons and residential sorting of families by socioeconomic status. Parents can affect the *ex ante* quality of schools that one's child will be facing through residential choices (Lee 2014). Hence, the random assignment in question is always *conditional on* the sorting of families into their preferred districts (that has occurred by the time the child is in the 9th grade). Once conditioned on districts, assignment between academic high schools is random; interference with the school lottery either before or after school assignment is virtually impossible, a fact well understood by

[8] Due to excess capacity at schools in the "Central District" of Seoul, this district was given permission to recruit students from across Seoul prior to the random assignment procedures taking place in other districts. This allows students from any part of Seoul to apply to a school of their choice in the Central District. To be comparable with existing studies such as Park et al. (2013), we use all 11 districts. However, all our analyses are robust to exclusion of the Central District from the sample. Also, there are no switching schools in the Central District.

[9] In South Korea, "public" and "private" school types co-exist but they do not have the same connotation as in the US or the UK. Although founded by different entities historically, both are subject to the High School Equalization Policy and "private" schools do *not* admit students on their own. Therefore, as far as students are concerned there are no differences between the school types. Both public and private establishment types charge the same fees and teach same curricula. See Hahn et al. (2016) for details.

South Korean students and parents.[10] If, for any reason, a student were to change school, his or her entire family has to move to a different school district and establish residence there. In the new district, the student will again be subject to random assignment (Park et al. 2013). Although the incidence of transfers or dropouts is rare, in Section 4.5, we address the concern of selective attrition (e.g., differential turnover between pre and post cohorts and between switching and non-switching schools) in detail.

2.2 The Expansion of Coeducation and School Type Changes

South Korea is a country in which gender inequality is quite pervasive and persistent despite the nation's impressive recent economic growth and development. In the Global Gender Gap Index 2011, South Korea ranked 107 out 135 countries surveyed (The *World Economic Forum Gender Gap Report*, 2011). The liberal government that was in office in the late 1990s saw coeducation as a step towards achieving gender equality. Consequently, it actively promoted the expansion of coeducation throughout South Korea during that period both by building new coed schools and by converting some pre-existing single-sex schools to coed schools (Chung et al., 2009). Consistent with the national policy, there was also an expansion of coeducation in Seoul in the late 1990s and early 2000s. The ratio of boys attending coed high schools in the city increased from 20 percent in 1996 (CSAT cohort) to 35 percent in 2009, and that for girls increased from 23 percent to 36.

[10] The HSEP has been in place for over three decades in Seoul. We failed to find any legal cases, news reports, or postings on social media referring to corruption involving high school lottery. The inability to influence the school assignment—beyond residential sorting—is quite consistent with the strong sentiment shared among South Korean parents towards the education of their children; had any parents learned about another parent's "successful" manipulation of the high school lottery, they would have immediately protested and brought the case to the attention of the media and to the court.

At various points during our sample period, seven all-boys schools and four all-girls schools were converted to the coeducational type. The converting schools were *not* selected randomly among existing single-sex schools and we do not know exactly why those particular schools chose to convert. However, random allocation of pupils to all these schools *at every cohort* (during our sample period, 1996-2009) ensures that student sorting does not compromise our design. As long as the decision to switch is based on time-invariant school characteristics (which we account for by school fixed effects), the fixed effects or DiD estimates will identify the combined effects of a school's gender type and possible school-level changes that accompany the school-type conversion. That is, the estimates will have a causal interpretation under the assumption that the achievement trends would be the same between the switching and non-switching schools in the absence of the switch, which we examine in Section 4.

While common "level" prior to treatment is not required for causal identification of the DiD parameter, understanding whether the switching single-sex schools are typical of non-switching schools matters for interpreting the results. For instance, if the switching single-sex schools (treatment group) happen to be concentrated on the upper (lower) part of the distribution and if treatment effects are heterogeneous, our estimate—under common trend assumption between treatment and control groups—will tell us the causal effect of converting initially high (low) performing single-sex schools to coed. To understand who the switching schools are, we therefore examine where the switching schools stand in the distribution of school fixed effects (which is an estimate of school quality). As Figure 1 shows, the switching schools are spread across the school quality distribution, and they do not appear to be systematically drawn from one part of that distribution, and not concentrated on either low performing or high performing schools.

[Figure 1]

2.3 Data and Descriptives

To measure students' academic achievement, we use administrative data on the national college entrance exam, the College Scholastic Aptitude Test (CSAT), taken by the 12th graders in 1996-2009.[11] The CSAT score is required for admission to any college in South Korea. Therefore, about 96 percent of students in academic high schools each year take this test whether or not they end up going to college. Ideally, we would have liked to look at outcomes beyond academic achievement. Our design, however, requires a relatively long panel to encompass school type changes and CSAT scores are the only measure that is consistently available for the duration of our sample period. For analysis, we use the standardized scores of individual students on Korean, English, and math tests. We standardize the raw CSAT scores to z-scores (to have mean 0; standard deviation 1). Our sample includes all individuals for whom the scores for Korean, English, or math are available.[12]

In South Korea, an academic year runs from March in a calendar year to February in the following year. The CSAT test is taken usually in November, towards the end of grade 12. Therefore, by the time an individual takes the CSAT, he/she has already spent almost three academic years in a high school. Besides the scores of the test, the CSAT data also provide some rudimentary information on each examinee, including gender; school ID; and city and district

[11] We exclude the CSAT year 2007 from our analysis because for that year raw scores on the CSAT were not reported.
[12] A vast majority of the students take all three subjects. However, from 2004 onward, the math section of the CSAT was no longer mandatory for admission to some colleges. Therefore, the number of observations in math is generally smaller than that for Korean and English in the data. For our empirical analysis, we use all observations available in each subject. However, interpretation of math scores is subject to this caveat.

information. Based on the school ID, we matched the score data with the relevant school-year level characteristics.

The school-level data come from the 1996-2009 issues of the Seoul Education Statistics Annual (SESA). We digitized the various issues of the SESA and compiled school-year level information, such as year of establishment, establishment type (public versus private), school size (total enrollment), class size, pupil-teacher ratios, pupil-administrator ratios, percentage of female teachers, and percentage of female administrators. Based on this information, we constructed the school characteristics that are relevant to each CSAT cohort. For all the time-varying school-level characteristics, we use the information as of a student's final year of high school.[13]

Table 1A shows descriptive statistics at the school-level. In the SESA data, information broken out by gender becomes available beginning in 1999. Therefore, the following school characteristics are available for 1999 onwards only: share of girls in own cohort; share of girls in school (i.e., across all grades); share of female teachers; and share of female administrators. All other school characteristics are available for 1996-2009.

[Tables 1A and 1B]

There are 68 all-boys schools that remain single-sex throughout; 61 all-girls schools that remain single-sex throughout; 64 coed schools that remain coed throughout; 7 all-boys schools that switch to coed between 1999 and 2009 CSAT cohorts; and 4 all-girls schools that switch to coed between 1999 and 2009 CSAT cohorts. For switching schools, we also report the summary

[13] We also constructed the variables to reflect the average characteristics during the student's three years of high school attendance. However, doing so reduces our sample size significantly since the Seoul Education Statistics Annual (SESA) reports school characteristics broken out by gender only from 1999 onwards. Since either measure leads to similar results whenever both measures are available, we prefer to use the measure based on characteristics as of the final year of high school to be able to keep the observations from the earlier periods in the sample.

statistics separated by pre and post periods, where post indicates the school's coed (versus single-sex) status as of a CSAT cohort's final year of high school.

We first compare the characteristics of single-sex versus coed schools that do not change types (columns 1, 2, and 3). Consider the share of girls by school types. As expected, it is zero or unity in single-sex schools and close to 0.5 in coed schools. Coed schools are more likely to be public (versus private) and are more likely to be established in recent years (between 1997 and 2007). The standard measures of school resources such as class size and pupil-teacher ratios are generally comparable between school types, reflecting government guidance and heavy subsidies to maintain the High School Equalization Policy. In terms of school size (total enrollment), single-sex schools tend to be slightly larger than coed schools. Interestingly (and perhaps as expected), boys-only schools tend to have a lower percentage of female teachers and administrators than coed schools, while girls-only schools tend to have a slightly higher percentage of female teachers.[14]

Next, we examine the characteristics of switching schools. The pre and post periods for switching all-boys (all-girls) schools are around 1998 and 2005 (2000 and 2006) whereas the mean year in the data is 2003. Hence, the comparison between columns 5 and 6 or between columns 8 and 9 may reflect not only the school's pupil gender type but secular changes in school resources that affected all schools in Seoul.[15] Based on panel A, we see that the share of girls (either in own cohort or at the school level) rises from zero to around 40 percent as all-boys schools switch to the coed type. Similarly, the share of girls drops from unity to around 60

[14] While we view female teachers as part of the school characteristics that may differ between coed and single-sex school types, Bettinger and Long (2005), Dee (2007), Hoffmann and Oreopoulos (2009), and Carrell et al. (2010) investigate the effect of teacher gender itself in a context where all schools are coeducational.
[15] For instance, there was a secular rise in female teacher share and a secular decline in class size and pupil-teacher ratio during the period 1996-2009.

percent as all-girls schools switch to coed. In all-boys schools, the switch to coed is accompanied by a rise in female teacher share although with this simple difference between pre and post, we cannot rule out the role of secular trends. Columns 4 and 7 in panel B show that switching all-boys (all-girls) schools are mostly public (private). In terms of school resources such as class size, pupil-teacher ratio, or pupil-administrator ratio, switching schools are hardly different from non-switching schools, again reflecting government guidance and subsidies. In an event-study framework, we later examine whether and how these school-level inputs, in particular the percentage of female teachers, adjust during the course of school type conversion.

Table 1B provides the summary statistics on student achievement on the CSAT for 1996-2009. The CSAT scores are standardized by subject and year to have a mean of zero and standard deviation of 1. Entries in the table report—for boys (panel A) and girls (panel B)—the mean, standard deviation, and number of observations of test scores in Korean, English and math. We distinguish between schools that always remain single-sex, schools that always remain coed, and schools that convert from all-boys to coed or from all-girls to coed. Focusing on non-switching schools (columns 1 and 2), two patterns emerge. First, girls outperform boys in all three subject areas and across school types, which is consistent with the pattern observed in the U.S. (Goldin et al., 2006; Fortin et al. 2013) and other developed countries. For instance, in single-sex schools, while the mean test score in Korean for boys is about 9.7 percent of a standard deviation below the overall mean (combining boys and girls in all school types), it is 16.6 percent above a standard deviation for girls, resulting in an overall difference of 26.3 percent of a standard deviation. Second, for both boys and girls, achievement is slightly lower in coed schools than in single-sex schools. Whether this is due to coeducational schools being different from single-sex schools in their (observable and unobservable) school characteristics or due to pupil gender type is what we investigate below. A simple difference between pre and post periods among switching schools reveals that the achievement is lower in the post periods for both boys (columns 3 and 4 in Panel A) and girls (columns 5 and 6 in Panel B), which is suggestive. In our analysis below, we will refine the comparison through combining switching and non-switching schools and also exploiting the multi-grade structure of high schools.

3. Estimation Strategy

Consider an outcome of interest y_{ijkt} (e.g., scores on the college entrance exam) and the following relationship:

$$(1)\ \ y_{ijkt} = \alpha_0 + \alpha_1 Coed_{jt} + \phi_{kt} + e_{ijkt},$$

where *i*, *j*, *k*, and *t* are indices for individual, school, district, and cohort (or equivalently, the year in which the cohort sits the CSAT exam). The outcome y_{ijkt} is measured at the end of individual *i*'s 12th grade. We omit the index for gender, as all the results we present as well as the school assignment itself are separate for boys and girls. The variable $Coed_{jt}$ is an indicator variable measuring whether school j for cohort t is coed (versus single-sex) type, ϕ_{kt} represents district-specific cohort effects, and e_{ijkt} a residual error component.

Estimating equation (1) using OLS identifies the parameter α_1, which is causal due to randomization of pupils into schools within districts. This parameter measures the *composite* or *total* effect of attending a coed (versus single-sex) school. It is of considerable relevance: For instance, when parents decide about a suitable school for their children, it is this parameter they are interested in.

Suppose now that x_{jt} and ψ_j represent time-variant observable and time-invariant unobservable school characteristics, respectively, that affect test scores, and that may be correlated with the school's coed status. An extended relationship of (1) is then given by

$$(2)\quad y_{ijkt} = \lambda_0 + \lambda_1 Coed_{jt} + \lambda_2 x_{jt} + \psi_j + \phi_{kt} + \epsilon_{ijkt},$$

where the parameter λ_1 captures the effect of exposure to mixed-gender (versus same-sex) peers while accounting for observed time-varying and unobserved time-invariant differences in school-level inputs between single-sex and coed schools.[16] Assume for simplicity that x_{jt} and ψ_j are scalars, and imagine the linear projections $L\left(x_{jt}\middle|Coed_{jt}, \phi_{kt}\right) = b_0 + b_1 Coed_{jt}$ and $L\left(\psi_j\middle|Coed_{jt}, \phi_{kt}\right) = c_0 + c_1 Coed_{jt}$. Then the OLS estimate of α_1 in (1) has $plim\ \hat{\alpha}_1 = \lambda_1 + \lambda_2 b_1 + c_1$, which is equal to λ_1 only if $\lambda_2 = \psi_j = 0$ (i.e., x_{jt} and ψ_i do not affect the outcome y_{ijkt}) or if $b_1 = c_1 = 0$ (i.e., observed and unobserved school characteristics that matter for achievement are not correlated with the coed status of the school).[17] The parameter λ_1 cannot be identified from cross-section data. It is identified, however, if some schools change its pupil gender status, say from single-sex to coeducational. Identification of λ_1 in (2) using difference estimators does not require random selection of schools (from all existing schools) that change status. The identifying assumption for λ_1 is the conditional independence: $\{y_{0ijkt}, y_{1ijkt}\} \perp\!\!\!\perp Coed_{jt} | x_{jt}, \psi_j, \phi_{kt}$, where y_{0ijkt} and y_{1ijkt} denote the potential outcomes under the scenarios that the school stays single-sex and the school converts to coed, respectively (see e.g., Wooldridge 2010; Angrist and Pischke 2009). That is, conditional on x_{jt}, ψ_j and ϕ_{kt}, $Coed_{jt}$ can

[16] Equation (2) can alternatively be expressed as: $y_{ijkt} = \lambda_0 + \lambda_1 Switcher_j \times Post_{jt} + \lambda_2 x_{jt} + \psi_j + \phi_{kt} + \epsilon_{ijkt}$, where $Switcher_j$ indicates whether the school ever changes its type from single-sex to coed and $Post_{jt}$ indicates whether the cohort in that school was exposed to the coed regime.

[17] Other possibilities are the hybrid cases: $\lambda_2 = c_1 = 0$ or $b_1 = \psi_j = 0$.

be said to be "as good as randomly assigned."[18] It should be noted that there is no issue of sorting of pupils to schools in response to the school type change due to the randomization of pupils to schools *at every cohort*.

What does the parameter λ_1 identify? Suppose all high schools are one grade schools, so that pupils spend only one year in high school. The parameter λ_1 then measures the effects of exposure to coed (versus single-sex) environment at class *and* school level, and unobserved school-level changes that accompany the school type conversion. The parameter λ_1, however, may not identify the effect of class-level exposure to mixed-gender (versus single-sex) environment only. In fact, if high schools were of single grade, the effect of class-level exposure cannot be separated from school-level exposure even with school type changes.

In our case, however, high schools have multiple (three) grades and the conversion was done one cohort at a time for incoming classes only. That means the first cohort admitted in the coed regime is exposed to mixed-gender peers at class (and school) level for three years as well as to potential changes in school-level inputs that we do not observe. The preceding cohort, while not being exposed to mixed-gender peers at class level, will also be exposed to school-level coed environment and any school-wide changes undertaken due to the switch, for the last two out of the three years of their high school experience. To the extent that school-level exposures to the new environment affect the two cohorts similarly, the difference in attainment between the two cohorts allows us to isolate the *net effect* of class-level exposure to mixed-gender peers.

The particular manner in which the school type conversion was implemented is illustrated in Figure 2, for the transition of a formerly all-boys school to a coed type. Regardless of school

[18] See Dustmann and Rochina-Barrachina (2007) for more details on panel data estimators with selection.

type change, the peer configuration *within own cohort* (determined at grade 10) is always maintained as the given cohort progresses to the next grade. Prior to year t* only boys attended this school. Beginning in year t* and for all subsequent years, the incoming class becomes coed. Normalizing the 12th graders in t*-1 as event year (τ) equal to zero, we have cohorts who—during the three years of high school—were exposed to single-sex environment only ($\tau \leq 0$); spent non-zero years in a coed school but never had coed peers in own cohort ($\tau = 1, 2$); and always had coed peers in a coed school ($\tau \geq 3$).

[Figure 2]

In Figure 3 we plot the share of girls in own cohort by event years. There is a sharp discontinuity between $\tau = 3$ (the first cohort admitted in coed regime) and $\tau = 2$ (the preceding cohort) (i.e., 50 versus 0 percent share of girls) even though these cohorts overlapped in the same (newly coed) school for two years.

[Figure 3]

To implement this in our estimation design, consider the following difference-in-differences (DiD) equation:

$$(3)\quad y_{ijkt} = \gamma_0 + \gamma_1 Switcher_j \times CSPost_{jt} + \psi_j + \phi_{kt} + \omega_{ijkt}.$$

Here the variables $y_{ijkt}, \psi_j, \phi_{kt}$ are defined as in (2). The variable $Switcher_j$ indicates whether school j changes type from single-sex to coed during the sample period. $CSPost_{jt}$ indicates whether cohort t in school j had class-level exposure to mixed-gender (versus single-sex) peers for three years during high school attendance. According to the designation of event year τ in Figure 2, $CSPost_{jt} = 0$ if $\tau = 1, 2$ and $CSPost_{jt} = 1$ if $\tau \geq 3$. We estimate (3) using cohorts who had any exposure to school-level coed environment at switching schools (i.e., $\tau \geq 1$) and their counterparts at non-switching schools.

The parameter γ_1 will thus identify the *net effect* of class-level exposure to mixed-gender (versus same-sex) peers for three years. For two adjacent cohorts such as $\tau = 2$ and $\tau = 3$ who overlap for two years in the same (newly coed) school, the effect of school-level coed exposure and school-level changes undertaken as part of the school type conversion is likely to be comparable (note that time-invariant school characteristics are always accounted for by the school fixed effect, ψ_j). This assumption may be less likely to hold the further away the treated (i.e., $CSPost_{jt} = 1$) cohorts are from the benchmark group (i.e., $CSPost_{jt} = 0$). We start with a common effect γ_1 for all $\tau \geq 3$ initially and later examine whether the effect varies as we move away from $\tau = 3$ to later cohorts.

The objective of our analysis below is therefore to estimate and compare three relevant causal parameters: First, the between-school effect of attending a coed (versus a single-sex) school, by estimating specification (1). Second, the effect of school- *and* class-level coed exposure, by comparing cohorts that were exposed to single-sex versus coed environment on both school- and class levels. And finally, the net effect of having mixed-gender (versus single-sex) peers at class level for three years, exploiting the multi-grade nature of high schools.

4. Results

4.1 The Between-School Effect of Attending a Coed (versus a Single-Sex) School

In Table 2, we present between-school estimates (equation (1)) of attendance at a coed (versus single-sex) school for CSAT Korean, English and Math, for boys and girls, respectively. All scores are standardized to have a mean zero and standard deviation one. In all regressions, district-specific year effects are included. Standard errors are clustered by school.

[Table 2]

Panel A is based on the full sample (1996-2009), while panel B restricts the sample to the period 1999-2009 for which we have available the full set of school-level characteristics. Take column 1 of Panel B, which looks at CSAT score in Korean for boys. It shows that the total effect of attending a coed (versus all-boys) school lowers achievement by 7.4 percent of the standard deviation. Estimates for other subjects are of similar magnitude. Columns 4-6 show that the effect of coed (versus single-sex) attendance is similarly negative for girls, with estimates ranging from reductions of 5 to 7 percent. Overall, this table shows that the causal effect of attending a coed (versus single-sex) school on the CSAT exam scores is negative across samples and across subjects and for both boys and girls. This finding confirms what Park et al. (2013) showed on the basis of 2009 cross-sectional data. In fact, our estimates are quite similar to theirs, which range between 6.5 and 10 percent of a standard deviation.

As single-sex and coed schools may differ not just in their pupil gender type but also along other dimensions that may affect achievement, we condition in Table 3 on school-level observables. These include an indicator for private (versus public) establishment type; indicator for a recently established school; percentage of female teachers; percentage of female administrators; class size; pupil-teacher ratios; pupil-administrator ratios; and school size. Column 1 uses no controls. In columns 2 through 9, we include one school characteristic at a time and column 10 includes all the school characteristics together.[19] The upper part of Table 3 shows results for boys and the lower part that for girls.

[Table 3]

Some interesting patterns emerge. For both boys and girls, the private (versus public) dummy (column 2) seems to dampen the negative coefficient on Coed, which could be explained

[19] The full coefficients of column 10 are displayed in Appendix Table A.1.

by the independent positive effects of private (versus public) establishment types as documented in Hahn et al. (2016) and the correlation between private and single-sex school type (as shown in Table 1A). Moreover, for boys, once we condition on the share of female teachers (column 4), the coefficient on Coed drops in magnitude and is no longer significant. For both boys and girls, when we condition on all the school characteristics together, the coefficients on the Coed dummy drop in magnitude, and are no longer statistically significant. The patterns found in Table 3 are robust to excluding schools that switch from single-sex to coed from the sample (see Appendix Table A.2).

The contrast between the unconditional and conditional estimates highlights the challenge to separating the net effect of exposure to mixed-gender (versus single-sex) peers from other school-level characteristics, even when pupils are randomly assigned to schools. One should thus be cautious in interpreting the between-school estimates as indicative of mixed-gender (versus single-sex) classroom environment. Further, while the unconditional estimates in Table 2 measure the causal effects of attending a coed (versus single-sex) school on test scores in Seoul—a context-specific parameter that may be of interest to the parents—the conditional estimates in Table 3 have no clear interpretation.

Importantly, however, the estimates in Table 3 do *not* imply that having mixed-gender (versus single-sex) peers in itself has no effect on students' achievement. Rather, it suggests that schools' pupil gender type is correlated with observed school characteristics and test scores alike. To investigate further what effect school- and class-level exposure to mixed-gender (versus same-sex) peers might have on attainments of boys and girls, we now turn to analysis where we exploit school type changes over time together with random assignment of pupils to switching versus non-switching schools.

4.2 What Happens When a Single-Sex School Converts to a Coed Type?

We start by presenting the effect of school-type conversion (from single-sex to coed) in an event study framework. We focus on event years -5 to 7 (-2 to 7 for variables that require gender-specific information) for boys and -5 to 5 for girls.[20] Using the CSAT scores in various subjects as the dependent variable, Figure 4 plots the estimated coefficients of

$$(4) \quad y_{ijkt} = \beta_0 + \sum_{\tau \neq 0} \beta_{1\tau} Switcher_j \times I(jt = \tau) + \psi_j + \phi_{kt} + u_{ijkt},$$

where τ indicates the event year as defined in Figure 2. As before, y_{ijkt} shows the score on the CSAT exam for individual *i* in school *j* in district *k* and in cohort *t*. The variable $Switcher_j$ indicates whether school j changes type from single-sex to coed during the sample period. The indicator $I(jt = \tau)$ maps each school-cohort to an event year (see Figure 2). In addition, ψ_j denotes the school FE and ϕ_{kt} district-cohort FE. The coefficient for $\tau = 0$ (last cohort in purely single-sex school) is normalized at zero. The coefficients $\beta_{1\tau}$ show the event year specific changes in outcomes relative to the benchmark ($\tau = 0$). We also plot 95 percent confidence intervals. As we control for school (and district-year) FE, the effects of time-invariant school-level features such as private (versus public) establishment type and whether the school is recently established are accounted for throughout. Standard errors are clustered by school.

[Figure 4]

Panel A shows the results for boys. While there is no systematic difference between switching and non-switching schools prior to the school-type change, achievement starts to go down for event years 1 and 2. Recall that event years 1 and 2 are exposed to school-level coed

[20] Conversion of boys' schools starts in earlier part of our sample period and that of girls' schools in later part of the sample period. Hence, our event window is constrained at the front end for boys and rear end for girls. As mentioned above, the school characteristics that require gender specific information are available from 1999 onwards only.

environment (for one and two years, respectively) but never to class-level coed environment. This may suggest that the presence of girls in the same school (even if not in own cohort) distracts boys from academic to other pursuits (Coleman 1961; Hill 2015). The drop in attainment may also be due to new measures that the school introduces as part of the school type conversion (e.g., hiring of more female teachers), or the excitement/disruption created by the school type change itself.

Event year $\tau = 3$ is the first cohort that has exposure to mixed-gender environment at both the school- and class level. From the figures, there seems to be little difference in achievement between $\tau = 2$ and $\tau = 3$. This is quite striking since these two cohorts—while sharing a common school-level environment—differ radically in their class-level peer gender mix (zero versus 50 percent of girls). It may suggest that for boys, the class-level gender mix has little impact once conditioned on the school-level coed environment, which we investigate further in Section 4.4

Panel B shows the patterns for girls. Unlike for boys, we find little difference between event years 0 and 1. This may imply that the presence of boys in the same school (if not in own cohort) does not distract girls from academic pursuits; that new measures, if any, undertaken as part of the school type conversion (e.g., hiring of more male teachers) are uncorrelated with achievement of girls; or that girls are not affected much by the school type change itself. The third aspect would be consistent with Deming et al. (2014) who showed that girls are in general more resilient than boys to changes in the school environment. Comparing event years 2 and 3 however—who overlapped in the same (newly coed) school for two years sharing the various aspects discussed above to a large extent—we notice a significant drop in achievement in Korean and English though not in math.[21] Recall that event year 3 had school- and class-level exposure to mixed-gender peers for three years whereas event year 2 had school-level exposure only. The drop in achievement from $\tau = 2$ to $\tau = 3$ therefore seems to suggest that for girls, the effect of having mixed-gender (versus same-sex) peers at class level is likely negative.

We now investigate this further in a regression framework. Our analysis proceeds in stages. First, we estimate the model in equation (2) where we omit the two transition cohorts that were exposed to mixed-gender peers at school level only but not at class level (i.e. event years $\tau = 1,2$), and compare cohorts that were exposed to single-sex school- and class environment for full three years ($\tau \leq 0$) with cohorts that were exposed to mixed-gender school- and class environment for three years (i.e. cohorts $\tau > 2$). This replicates our thought experiment in Section 3, where schools have only one grade, and identifies the effects of coed exposure at both school- *and* classroom level, and associated changes at school level on attainment.

Second, we isolate the effect of class-level exposure to mixed-gender (versus single-sex) peers from the combined effects of school- and class-level coed exposure and unobserved changes accompanying the school type conversion, by estimating equation (3) where we compare cohorts $\tau = 1, 2$ with cohorts $\tau \geq 3$. This amounts to comparing the first cohort who experienced mixed gender peers at both school- and classroom level with the preceding cohorts that were exposed to mixed-gender environment at school level only while both cohorts were exposed to the same school-wide changes that may go along with the conversion of school type. This comparison identifies the effect of class-level exposure to mixed-gender (versus same-sex) peers for three years.

[21] As mentioned above, from 2004 onward, the math section of the CSAT became no longer mandatory for admission to some colleges. Since not everyone took the math section, interpreting the effects on math requires caution.

4.3 The Effect of Converting School- and Class-Level Environment from Single-Sex to Coed

In Table 4 we present the estimates of equation (2), the DiD estimates of school-type conversion. We omit the transition cohorts $\tau = 1,2$ and compare pupils that were exposed to mixed-gender peers at both school- and class-level over the entire three years of curriculum ($\tau \leq 0$) to pupils exposed only to single-sex peers ($\tau = 3$ or later). We report results for boys in the upper two panels, and for girls in the lower two panels. School and district-year fixed effects are always included. Panels A and C presents estimates without further controls, while regression results presented in Panels B and D control for the full set of time-varying school-level observables. Even-numbered columns also allow for differential trends for switchers (relative to non-switchers).

[Table 4]

For both boys and girls, the within-school estimates of conversion from single-sex to coed pupil type—which controls for school-specific unobservables—are negative. The estimates vary slightly across subjects and gender, but point consistently at coed environment being detrimental for exam scores, in comparison to single-sex environment. For instance, for boys, the conversion from all-boys to coed pupil type leads to a reduction in English test scores by 15 percent of a standard variation, while the effect for girls amounts to 16 percent (Panels A and C, column 4).

These estimates, while eliminating school fixed effects, do not control for possible changes in school inputs. To investigate whether such changes are partly accountable for the decrease in attainment we see in Panels A and C, we condition on school-level variables that we observe. These include the share of female teachers, class size, pupil-teacher ratios, the log of school size, pupil-administrator ratios, and percentage of female administrators. To illustrate the relation between these variables and the change of school types from single-sex to coed, we report in Appendix Figures A.1 (boys) and A.2 (girls) event study graphs (similar to those in Figure 4) based on a variant of equation (4), where the dependent variable is now the respective school characteristic (such as pupil-teacher ratio). Overall, these graphs do not suggest a systematic relationship between the change from single sex to coed, except perhaps for the female teacher ratios, which increase in all-boys schools and decreases in all-girls schools when changing to coed.

Panels B and D of Table 4 report estimates where we condition, besides school fixed effects, on these school characteristics. The table entries show that inclusion of these variables hardly affects the magnitude and significance of the estimates, suggesting that changes in observables seem not to be systematically correlated with student attainment and conversion to coed status at the same time. That the estimates are invariant between Panels A and B (or between Panels C and D) is a strong indication that any remaining changes in time-varying unobservables will likewise have—if at all—only a small impact on estimated parameters (see Altonji et al. 2005a, 2005b). Thus, we may conclude from these findings that unobserved changes at school level that accompany the shift of school's gender type from single-sex to coed are unlikely to be a major factor for the negative impact of the school type change (from single-sex to coed) as estimated in Table 4.

4.4 The Effect of Class-Level Exposure to Mixed-Gender (versus Same-Sex) Peers

The estimates in Table 4 may be due to school- or class-level coed environment or both. A first indication of the possible reasons for these estimates is given by the event analysis in Figure 4, which suggests that it may be school-level coed environment that harms boys, while it is class-level coed environment that is detrimental for girls. To investigate this further, we now make use of the multi-grade nature of South Korean high schools, implying that changes from single-sex to coed school status took place gradually: While the first cohort entering a school that has just changed status experienced mixed gender peers at both class- and school levels, the preceding cohort will experience mixed-gender peers at school level only. Thus, comparison of cohorts who had mixed-gender peers at both school- and class levels (i.e. cohorts $\tau \geq 3$) and those who had mixed gender peers at school level only (i.e. $\tau = 1,2$) should allow us to isolate the effect of class level exposure to mixed-gender peers from the school effect. The underlying assumption is that any school-level environment that impacts on attainment of pupils is comparable for these adjacent cohorts, especially between $\tau = 3$ and $\tau = 2$ who overlapped in the same school for two (out of three) years during high school experience, which we believe is a plausible assumption.

The estimates of equation (3) are presented in Table 5, which has the same structure as Table 4. Interestingly, the negative effects for boys have largely disappeared, suggesting that once conditioned on the common school-level (coed) environment, class-level exposure to mixed-gender (versus same-sex) peers has little detrimental effect on boys' attainment. For girls, on the other hand, estimates for languages are similar in magnitude to those in Table 4; estimates for math are likewise negative, but smaller in size and imprecisely estimated. The estimates show that girls who had mixed-gender peers at both class- and school-level do worse by 8 to 15 percent of a standard deviation in languages, compared to girls who had mixed-gender peers at school level only, holding school-level environment as comparable as possible. For both boys and girls, and similar to our findings in Table 4, conditioning on changes in observables (Panels B and D) has hardly any effect on our estimated parameters. Thus, these findings point at the *net* effect of coed exposure at classroom level being negative for girls, and likely close to zero for boys.

[Table 5]

That the net effect of having single-sex peers for three years is strongly positive for girls but not for boys may be reconciled based on a combination of factors. First, as Lu and Anderson's (2014) recent study from China reveals, girls (boys) benefit from more girl (boy) peers through enhanced peer interaction when teacher quality and teacher behavior are held constant.[22] Second, in our data, girls on average outperform boys in all subjects (Table 1B). According to the work of Hoxby (2000) and Lavy and Schlosser (2011), classroom gender composition can affect achievement through changing classroom atmosphere. Holding constant academic abilities, if boys are on average more disruptive than girls, then having a larger share of more disruptive classmates (i.e., boys) have negative consequences on the test scores of peers (Figlio 2007). Moreover, even if boys are not more disruptive than girls in terms of classroom behavior, having a high proportion of low-ability students—which happen to be boys, in our context—may lower the academic achievements of regular students, by diverting teacher attention from regular to struggling students (Lavy et al. 2012). From the perspective of girls in our context, having coed (versus all-girls) peers means they are subject to the negative aspects of both mechanisms discussed above whereas for boys, having coed peers exposes them to the positive side of the mechanism in Lavy et al. (2012) and the negative side of the mechanism in Lu and Anderson (2014).

[22] Since Lu and Anderson (2014) exploit very fine variation *within* coed classrooms, they can effectively suppress the influence of classroom-level common factors such as teachers.

4.5 Robustness Checks

Novelty Effects. In Table 5, we impose a common effect on the difference between $\tau \geq 3$ (who have coed exposure at school- and class level) versus $\tau = 1, 2$ (who have coed exposure at school level only). However, the further away the cohorts being compared are, the less likely might be that the school-level environment remains comparable. Moreover, event year 3 is the first time that the teachers at the switching school have to teach to a mixed-gender audience. Therefore, even holding the composition of teachers constant, it may take time for the (same) teacher to figure out how to teach to a mixed-gender audience. To address this concern, we allow for the coefficient γ_1 in equation (3) to differ across $\tau = 3$, 4, and 5. The estimated coefficients along with the 95 percent confidence intervals are plotted in Figure 5.

[Figure 5]

As shown, for boys, effects are small and insignificant for all three years, reflecting our findings in Table 5. For girls, the negative effects are stable over time and do not become smaller (for three event years at least), which suggests that our estimates in Table 5 are unlikely to be driven by the novelty (initial difficulty) effects alone.

Small Number of Switchers. Our diff-in-diff estimates are based on a relatively small number of switchers (7 boys schools and 4 girls schools) although we have a large number of non-switchers, with an overall cluster size of 143 for boys and 135 for girls.

Given the small number of policy changers, we want to make sure that our findings above are not driven by a particular school. Therefore, we re-estimate our main specification while excluding one switcher at a time. The results are provided in Appendix Tables A.3A (boys) and

A.3B (girls). As shown, the patterns are very similar to our main findings reported in Table 5, suggesting that our main findings are not driven by a particular school.

Selective Attrition. The High School Equalization Policy (HSEP) and random assignment of pupils to schools within district was in place throughout our sample period (1996-2009). Therefore, even though the school-type conversion was voluntary on the part of the individual school, random assignment of students to schools makes the student quality orthogonal to the school type change at least at the point of assignment. However, selective attrition (i.e., differential turnover between pre and post cohorts and between switching and non-switching schools) by the 12th grade (or the point of exam taking) may compromise the causal interpretation of our diff-in-diff estimates. The CSAT data being repeated cross section in nature, we cannot follow individual students over time. However, we have information on enrollment at the school-year-grade level. Based on that information, we estimate a variant of equation (3) and examine whether there is any selective attrition (i.e., differential turnover between treated and non-treated cohorts and between switching and non-switching schools) in terms of enrollment and in terms of exam taking.

[Table 6]

Results are reported in Table 6, where—using the specification in (3)—the dependent variable is "turnover" (columns 1, 2, 5, and 6) and "exam taking" (columns 3, 4, 7, and 8). The variable "turnover" measures the number of enrollment in the 12th grade in a given school divided by the number of enrollment in the 10th grade two years prior to that, to capture the turnover rates for the same cohort in that school. The variable "exam taking" measures the number of CSAT takers divided by the number of enrollment in the 12th grade in the same year, to capture the share of currently enrolled students who take the test. The mean of the dependent variable is reported below the column headings. As shown in the table, attrition is very small at baseline (i.e., "turnover" is close to zero and "exam taking" is close to unity) and most importantly largely orthogonal to the treatment of interest ($Switcher_j \times CSPost_{jt}$).[23] As a way of comparison, the dropout rates (based on 16-24 year olds) in the US for this period were around 10 percent for the overall population and close to 20 percent for the Hispanic population.[24] The relatively small rate of turnover in our context may reflect the emphasis put on education by the South Korean society in general and also the fact that our sample focuses on academic high schools which educate students seeking college admission who are likely more academically inclined than the general population.[25]

5. Discussion and Conclusion

In this paper, we exploit various policy features of academic high schools in Seoul, South Korea: random assignment of pupils to high schools within districts, conversion of some existing single-sex schools to the coeducational type over time, and the multi-grade nature of high schools. This allows us to identify three distinct causal parameters: First, the between-school effect of attending a coed (versus a single-sex) school, answering the question "what is the attainment difference between single-sex and coed schools for boys

[23] Lee (2009) proposes bounds in cases where there is differential selection between treatment and control groups (i.e., in our diff-in-diff framework, differential turnover between the old and young cohorts and between switching and non-switching schools). The orthogonality of the student churn to the treatment status, as presented in Table 6, means that the trimming proportion "p" in Lee (2009) is zero in our case, so that the weight for the marginal group (as opposed to the inframarginal group) approaches zero. Given Lee (2009)'s monotonicity assumption, it therefore follows that the difference in the observed population means for treatment- and control-groups (as presented in Table 5) identifies the causal treatment parameter.

[24] Source: US Department of Education, National Center for Education Statistics (2016). The Condition of Education 2016 (NCES 2016-144), Status Dropout Rates.

[25] During our sample period, about 70 percent of high school graduates enrolled in some type of college (Source: Statistical Yearbook of Education, Korean Educational Development Institute, http://cesi.kedi.re).

and girls?." Second, the within-school effect of school type conversion, answering the question "what is the combined effects of coed (versus single-sex) exposure at school- *and* class level, and unobserved school-level changes that accompany the school type conversion?." And third, the effect of class-level exposure to mixed-gender (versus same-sex) peers, keeping school-level exposure constant, answering the question "what is the effect of class-level exposure alone to mixed-gender (versus same-sex) peers?."

Based on between-school analysis, we find robust evidence that pupils in single-sex schools outperform their counterparts in coed schools, by 5 to 10 percent of a standard deviation for boys and 4 to 7 percent for girls. This causal effect could be due to schools' pupil gender type, and/or school-level covariates that differ between single-sex and coed schools.

Exploiting school type changes, and comparing cohorts that were exposed to either a single-sex or coed environment on both school- and class levels, we find that the conversion of pupil gender type from single-sex to coed leads to worse academic outcomes for both boys and girls, conditional on school fixed effects. Conditioning on a large set of time-varying school level observables hardly affects these estimates, which may suggest that unobservable school level changes are unlikely to be a key driver of these estimates, and that it is likely the exposure to a mixed-gender environment that leads to deterioration in exam results for both boys and girls.

In a third step, and making use of the multi-grade nature of South Korean high schools, we separate class-level exposure to mixed-gender (versus single-sex) peers from school-level exposure and potential unobserved changes instigated at school level that accompany the school type change, by comparing adjacent cohorts in switching schools, where one has been exposed to a mixed-gender environment at both school- and class levels while the other had such exposure at school level only. We find that class-level exposure to mixed-gender (versus same-sex) peers has little effect on the attainment for boys, but a significant negative effect on the attainment of girls. Therefore, while for boys, the negative effect of a coed school seems largely driven by exposure to mixed-gender peers at *school-level*, it is *class-level* coed exposure that explains the disadvantage for girls. We should emphasize that this estimated effect is the *net* effect of pupil gender type (single-sex versus coed), which is inclusive of possible endogenous responses to it by e.g., teachers and parents. However, we would argue that for policy purposes, it is this net effect that matters, as any transformation from single-sex to coed pupil type may induce endogenous responses.

Although we focus in this paper on the pupil gender type in specific, our attempt to understand its role in explaining the overall advantage of single-sex schools is closely related to work such as Angrist et al. (2013), Dobbie and Fryer (2013, 2015), and Fryer (2014) who try to understand the roles of specific inputs and practices that characterize high-performing schools. Further research investigating the role of other dimensions of better performing schools will be fruitful, as accumulation of such information will help guide other schools and policy makers in deciding what elements to include (or not) in the package of treatment (called a "school") they offer to students.

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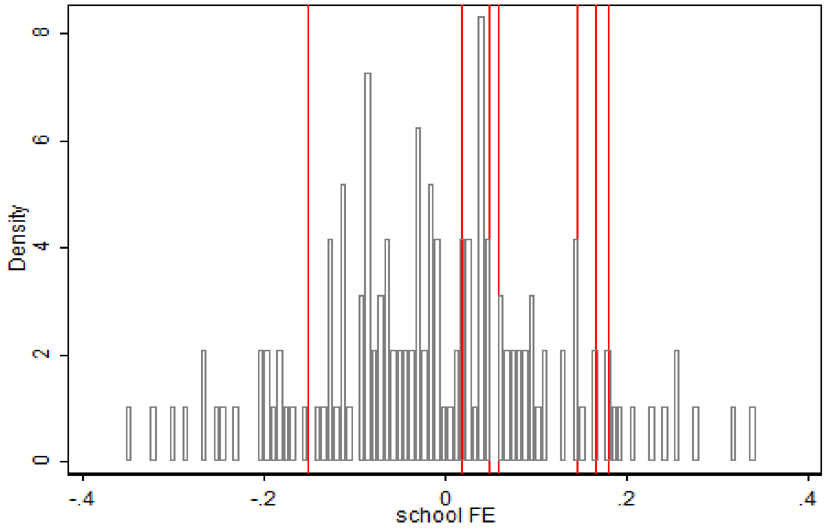
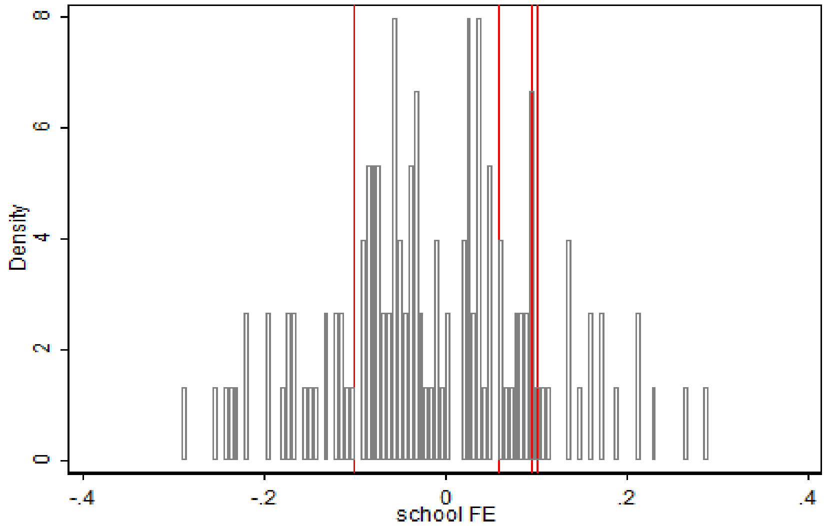
Figure 1: Location of switchers in the distribution of school fixed effects



(a) Boys



(b) Girls

Notes: This figure shows the histogram of school fixed effects (an estimate of school quality). The vertical lines in red indicate the location of switching schools along the distributioin. School fixed effects are estimated using CSAT zscore: Korean as the dependent variable for 1996-2009. In case of switching schools, only pre-switch data are used. Using other subjects as dependent variable does not change the results. All estimates condition on district-year fixed effects.

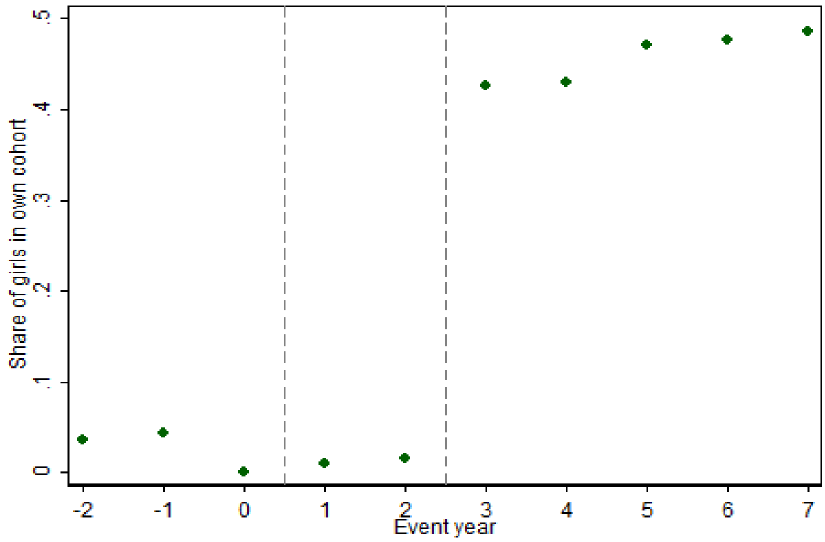
Figure 2: Example of a formerly all-boys school that converts to coed status

Calendar year (CSAT cohort):	t*-3	t*-2	t*-1	t*	t*+1	t*+2	t*+3	t*+4
Grade 12	**Boys**	**Boys**	**Boys**	**Boys**	**Boys**	**Boys** Girls	**Boys** Girls	**Boys** Girls
Grade 11	Boys	Boys	Boys	Boys	Boys Girls	Boys Girls	Boys Girls	Boys Girls
Grade 10	Boys	Boys	Boys	Boys Girls	Boys Girls	Boys Girls	Boys Girls	Boys Girls

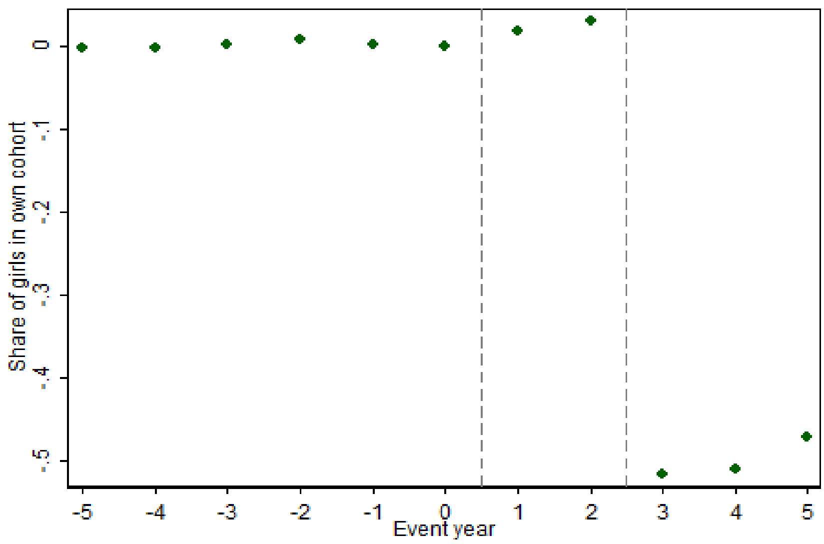
Event year t (from 12th graders in calendar year t)	**-2**	**-1**	**0**	**1**	**2**	**3**	**4**	**5**
Extensive margin:								
Coed exposure at school level	**No**	**No**	**No**	**Yes**	**Yes**	**Yes**	**Yes**	**Yes**
Coed exposure in own cohort for three years	**No**	**No**	**No**	**No**	**No**	**Yes**	**Yes**	**Yes**
Intensive margin:								
Years of coed exposure at school level	0	0	0	1	2	3	3	3
Years of coed exposure in own cohort within school	0	0	0	0	0	3	3	3

Notes: This school starts to admit girls for the incoming class beginning in year t*.
CSAT exams are taken at the end of the 12th grade. Therefore, 12th graders in year t are also the CSAT cohort t.
The upper panel shows the snapshot of the pupil gender composition at the grade and school level in different calendar years.
The lower panel shows the treatment status of different CSAT cohorts (different 12th graders) based on three years of high school attendance.
Event year is normalized to zero for the last CSAT cohort (12th graders) who were exposed to neither school- nor class-level coed environment during their three years of high school attendance.
Arrows in the upper panel indicates the grade progression of the cohorts corresponding to event years 0, 1, 2, and 3 (CSAT cohorts t*-1, t*, t*+1, t*+1), respectively.

Figure 3: Share of Girls in Own Cohort at Switching Schools (Relative to Non-switching Schools)



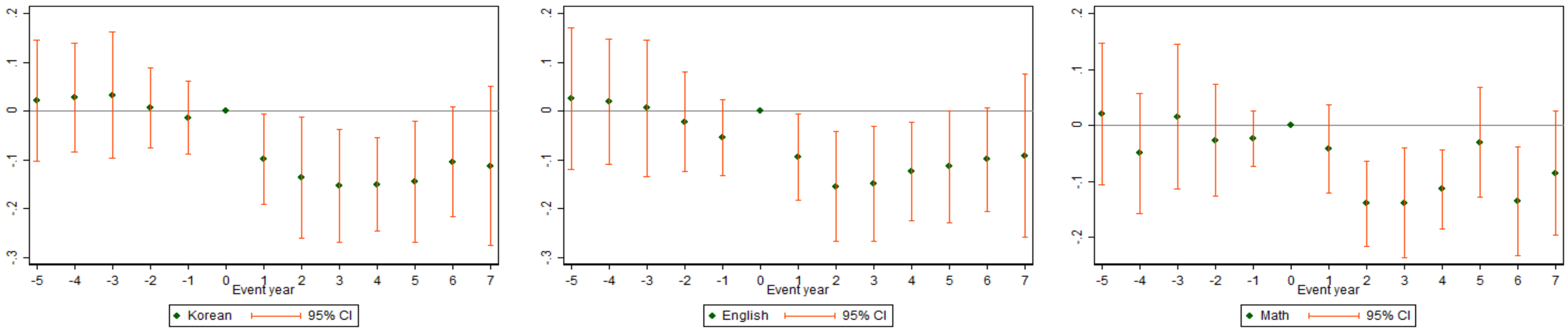
(a) Boys



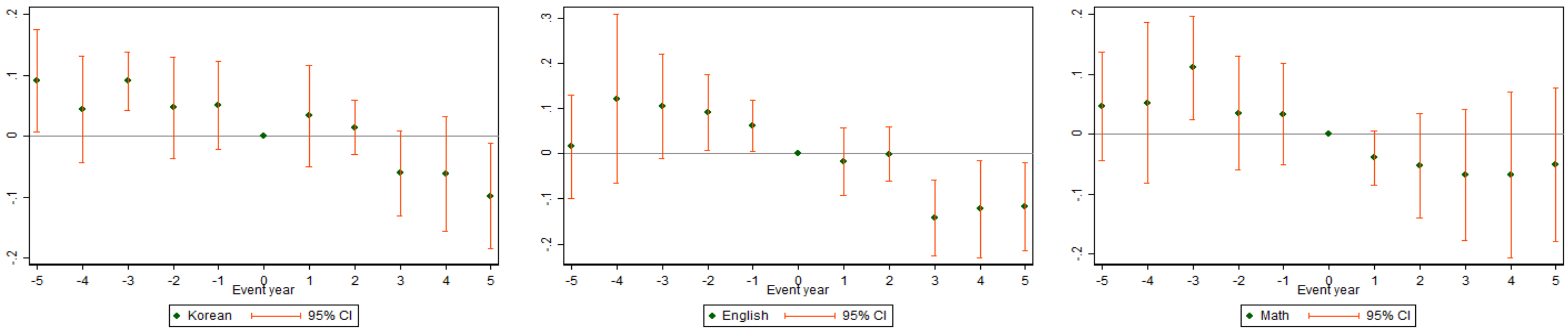
(b) Girls

Notes: This figure plots the share of girls in own cohort by event years. Coefficient for event year 0 is normalized at zero. School FE and district-year FE are controlled for. The two virtical lines show the first cohorts who were exposed to school- and class-level coed environment, respectively.

Figure 4: Event study of school type change from single-sex to coed: CSAT scores



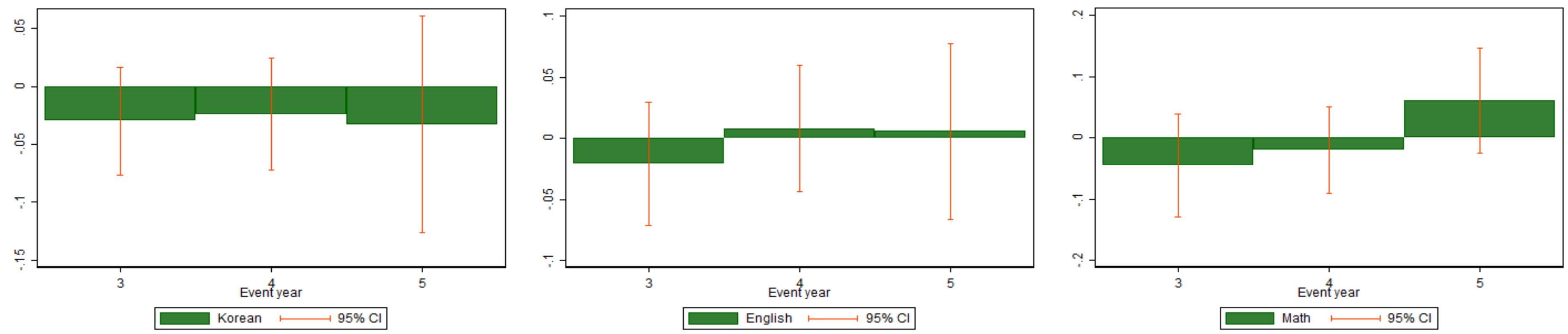
(a) Boys



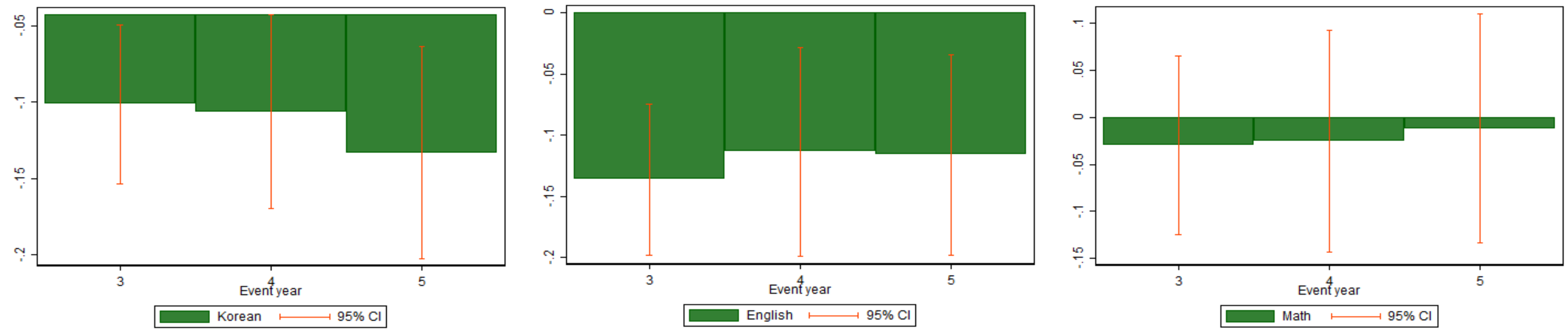
(b) Girls

Notes: This figure plots the coefficients from estimating equation 4 using the CSAT scores in different subjects as the dependent variable. Coefficient for event year 0 is normalized at zero. All regressions include school FE and district-year FE. Standard errors are clustered by school.

Figure 5: Are the estimates driven by novelty effects?



(a) Boys



(b) Girls

Notes: This figure presents the estimates of a variant of equation 3, which allows different coefficients for event years 3, 4, and 5, respectively. 95% confidence intervals are displayed along with the diff-in-diff coefficients. Estimation includes cohorts corresponding to event years years 1-5 at switching schools and their counterparts at non-switching schools. Standard errros are clustered by school.

Table 1A: School-level characteristics by school type, 1996-2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All boys school always	All girls school always	Coed school always	All boys school that switches to coed			All girls school that switches to coed		
				all	pre	post	all	pre	post
					A. 1999-2009				
Share of girls in own cohort	0.00	1.00	0.48	0.30	0.00	0.34	0.82	1.00	0.66
	(0.00)	(0.00)	(0.08)	(0.21)	(0.00)	(0.19)	(0.25)	(0.00)	(0.24)
Share of girls in school	0.00	1.00	0.48	0.34	0.00	0.39	0.77	1.00	0.58
	(0.00)	(0.00)	(0.07)	(0.17)	(0.00)	(0.12)	(0.23)	(0.00)	(0.13)
Share of female teachers	0.16	0.51	0.44	0.41	0.22	0.44	0.46	0.49	0.44
	(0.13)	(0.11)	(0.13)	(0.17)	(0.06)	(0.16)	(0.12)	(0.14)	(0.09)
Share of female administrators	0.21	0.27	0.34	0.34	0.32	0.34	0.10	0.08	0.13
	(0.15)	(0.17)	(0.16)	(0.20)	(0.18)	(0.20)	(0.08)	(0.08)	(0.07)
Obs	738	639	548	77	10	67	44	20	24
					B. 1996-2009				
Year (CSAT cohort)	2003	2003	2003	2003	1998	2005	2003	2000	2006
	(4.04)	(4.02)	(4.05)	(4.05)	(1.49)	(2.86)	(4.07)	(2.50)	(1.93)
Private	0.84	0.83	0.26	0.29	0.32	0.27	1.00	1.00	1.00
	(0.37)	(0.38)	(0.44)	(0.45)	(0.48)	(0.45)	(0.00)	(0.00)	(0.00)
Recently (1997-2007) established	0.00	0.07	0.23	0.00	0.00	0.00	0.25	0.31	0.17
	(0.03)	(0.26)	(0.42)	(0.00)	(0.00)	(0.00)	(0.44)	(0.47)	(0.38)
Class size	40.77	42.12	40.54	40.54	48.93	36.66	43.07	48.97	35.21
	(7.52)	(8.52)	(9.04)	(7.20)	(2.08)	(5.09)	(8.86)	(6.88)	(3.34)
Pupil-teacher ratio	20.34	21.05	19.03	19.03	22.88	17.24	21.84	24.82	17.87
	(3.92)	(4.68)	(4.51)	(3.30)	(1.26)	(2.26)	(4.46)	(3.61)	(1.10)
Pupil-administrator ratio	241.28	246.89	234.22	245.66	238.61	248.92	222.81	231.41	211.35
	(62.87)	(81.02)	(54.95)	(34.53)	(27.5)	(37.06)	(30.32)	(31.73)	(24.54)
School size (total enrollment)	1647	1583	1506	1624	1847	1520	1468	1584	1314
	(413.85)	(454.88)	(386.59)	(245.45)	(178.78)	(199.27)	(257.22)	(275.15)	(113.14)
Obs	939	801	661	98	31	67	56	32	24
Number of schools	68	61	64		7			4	

Notes: Mean is reported with standard deviations in parentheses. Data come from the 1996-2009 issues of the Seoul Education Statistics Annual. School characteristics are measured as of a CSAT cohort's final year of high school. School characteristics that require information broken out by gender are available for 1999 onwards only (Panel A). All other characteristics are available for 1996-2009 (Panel B). Recently established indicates whether a school is established between 1997 and 2007 (produced the first CSAT cohort between 1999 and 2009). For switching schools, post indicates the school's coed (versus single-sex) status as of a CSAT cohort’s final year of high school.

Table 1B: Student achievement by school type on CSAT 1996-2009

		(1)	(2)	(3)	(4)	(5)	(6)
		Single-sex school always	Coed school always	All boys school that switches to coed		All girls school that switches to coed	
				pre	post	pre	post
				A. Boys			
zscore: Korean	Mean	-0.097	-0.135	-0.104	-0.277	n/a	-0.132
	SD	1.048	1.041	0.988	1.046		1.014
	Obs	464936	155117	18853	19390		2674
zscore: English	Mean	-0.064	-0.114	-0.144	-0.277	n/a	-0.108
	SD	1.037	1.036	0.985	0.997		1.004
	Obs	464166	154834	18841	19338		2662
zscore: Math	Mean	-0.019	-0.045	-0.058	-0.185	n/a	0.005
	SD	1.043	1.032	1.001	0.981		0.976
	Obs	456161	150474	18850	18711		2365
				B. Girls			
zscore: Korean	Mean	0.166	0.135	n/a	-0.030	0.219	0.124
	SD	0.890	0.912		0.926	0.845	0.901
	Obs	386461	139351		9191	16796	5550
zscore: English	Mean	0.128	0.098	n/a	-0.099	0.227	0.094
	SD	0.927	0.954		0.911	0.870	0.923
	Obs	386074	139135		9163	16788	5537
zscore: Math	Mean	0.061	0.040	n/a	-0.157	0.108	0.016
	SD	0.940	0.961		0.899	0.913	0.925
	Obs	371225	133015		8483	16735	4821

Notes: Based on administrative data on the College Scholastic Aptitude Test (CSAT) for 1996-2009 excluding 2007 (for which score data are not available). Scores are standardized to have a mean of 0 and standard deviation of 1 for each CSAT cohort and by subject. A vast majority of the students take all three subjects. However, from 2004 onward, the math section of the CSAT was no longer mandatory for admission to some colleges. Therefore, the number of observations in math is generally smaller than that for Korean and English in the data. For our empirical analysis, we use all observations available in each subject. However, interpretation of math scores is subject to this caveat.

Table 2: Between-school estimates: The composite effects of attendance at a coed (versus single-sex) school

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent var.: CSAT score in					
	Korean	English	Math	Korean	English	Math
	Boys			**Girls**		
	A. 1996-2009					
Coed (versus single-sex) school	-0.054**	-0.081***	-0.058**	-0.042**	-0.056*	-0.049*
	(0.022)	(0.027)	(0.023)	(0.020)	(0.030)	(0.025)
Observations	660,970	659,841	646,561	557,349	556,697	534,279
Number of clusters	143	143	143	136	136	136
R-squared	0.024	0.047	0.035	0.016	0.036	0.030
	B. 1999-2009					
Coed (versus single-sex) school	-0.074***	-0.096***	-0.063**	-0.051**	-0.069**	-0.060**
	(0.025)	(0.031)	(0.026)	(0.022)	(0.033)	(0.029)
Observations	481,122	480,039	466,732	420,346	419,706	397,278
Number of clusters	143	143	143	135	135	135
R-squared	0.027	0.050	0.035	0.019	0.040	0.033
School FE	No	No	No	No	No	No
District*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimates of equation 1 using the CSAT scores as the dependent variable. Panel A uses the full sample and Panel B restricts attention to the periods (1999-2009) for which we have the full set of time-varying school-level observables. Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: The role of observable school characteristics in explaining the advantage of single-sex schools

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
X =	None	Private (versus public)	Recently establish ed	Share of female teachers	Share of female admin.	Class size	Pupil-teacher ratios	Pupil-admin. Ratios	Log school size	All
Boys										
A. CSAT score: Korean										
Coed (versus single-sex) school	-0.074***	-0.057*	-0.072***	0.006	-0.059**	-0.076***	-0.060**	-0.072***	-0.063**	0.031
	(0.025)	(0.030)	(0.027)	(0.031)	(0.026)	(0.024)	(0.024)	(0.025)	(0.025)	(0.034)
X		0.034	-0.006	-0.325***	-0.121*	0.547	0.016***	0.024	0.129*	
		(0.036)	(0.036)	(0.092)	(0.066)	(0.363)	(0.005)	(0.019)	(0.069)	
B. CSAT score: English										
Coed (versus single-sex) school	-0.096***	-0.078**	-0.092***	-0.003	-0.079**	-0.100***	-0.080**	-0.095***	-0.085***	0.030
	(0.031)	(0.036)	(0.033)	(0.037)	(0.032)	(0.031)	(0.031)	(0.031)	(0.031)	(0.042)
X		0.038	-0.022	-0.384***	-0.146*	0.701	0.020***	0.021	0.140*	
		(0.042)	(0.043)	(0.114)	(0.079)	(0.437)	(0.007)	(0.023)	(0.081)	
C. CSAT score: Math										
Coed (versus single-sex) school	-0.063**	-0.049	-0.066**	0.020	-0.046*	-0.066**	-0.050*	-0.062**	-0.052**	0.042
	(0.026)	(0.031)	(0.028)	(0.031)	(0.027)	(0.026)	(0.026)	(0.026)	(0.026)	(0.036)
X		0.030	0.012	-0.342***	-0.145**	0.621	0.016***	0.021	0.131*	
		(0.036)	(0.037)	(0.099)	(0.069)	(0.381)	(0.006)	(0.020)	(0.068)	
Girls										
D. CSAT score: Korean										
Coed (versus single-sex) school	-0.051**	-0.037	-0.053**	-0.050**	-0.052**	-0.041*	-0.028	-0.051**	-0.048**	-0.014
	(0.022)	(0.029)	(0.023)	(0.023)	(0.023)	(0.022)	(0.024)	(0.023)	(0.023)	(0.034)
X		0.033	0.012	0.010	0.003	0.999***	0.019***	0.000	0.075	
		(0.033)	(0.025)	(0.075)	(0.053)	(0.286)	(0.005)	(0.013)	(0.059)	
E. CSAT score: English										
Coed (versus single-sex) school	-0.069**	-0.053	-0.067**	-0.070**	-0.068**	-0.055*	-0.036	-0.069**	-0.064*	-0.028
	(0.033)	(0.040)	(0.033)	(0.035)	(0.033)	(0.032)	(0.034)	(0.033)	(0.034)	(0.048)
X		0.035	-0.010	-0.011	-0.013	1.279***	0.026***	-0.002	0.085	
		(0.047)	(0.035)	(0.108)	(0.078)	(0.381)	(0.006)	(0.017)	(0.084)	
F. CSAT score: Math										
Coed (versus single-sex) school	-0.060**	-0.046	-0.060**	-0.065**	-0.057*	-0.048*	-0.032	-0.059**	-0.055*	-0.031
	(0.029)	(0.037)	(0.030)	(0.030)	(0.029)	(0.029)	(0.031)	(0.029)	(0.029)	(0.044)
X		0.031	0.001	-0.057	-0.044	1.105***	0.022***	0.003	0.097	
		(0.043)	(0.034)	(0.098)	(0.067)	(0.348)	(0.006)	(0.014)	(0.073)	
School FE	No	No	No	No	No	No	No	No	No	No
District*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimates of equation 1 using the CSAT scores as the dependent variable. Sample is restricted to the periods (1999-2009) for which we have the full set of time-varying school-level observables. Column 1 has no additional controls. Columns 2-9 include one school characteristic at a time. Column 10 includes all school characteristics in the same regression (The full coefficients on the school characteristics for column 10 are reported in Appendix Table A.1). Class size and pupil-administrator ratios are multiplied by 100. Each column represents a different estimate. The estimates in Panels A, B, and C are based on 481,122, 480,039, and 466,732 observations, respectively, with 143 clusters in each estimation. The estimates in Panels D, E, and F are based on 420,346, 419,706, and 397,278 observations, respectively, with 136 clusters in each estimation. Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: The effect of converting both school- and class level environment from single-sex to coed

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent var.: CSAT score in					
	Korean		English		Math	
	Boys					
	A. No controls					
Switcher*Post	-0.111**	-0.166**	-0.090*	-0.153**	-0.084**	-0.135**
	(0.049)	(0.067)	(0.051)	(0.076)	(0.039)	-0.059
	B. With controls for time-varying school inputs					
Switcher*Post	-0.112**	-0.168**	-0.095*	-0.159**	-0.078*	-0.129**
	(0.048)	(0.066)	(0.052)	(0.077)	(0.041)	(0.062)
Observations	470,658	470,658	469,611	469,611	456,401	456,401
Number of clusters	143	143	143	143	143	143
	Girls					
	C. No controls					
Switcher*Post	-0.109***	-0.060	-0.192***	-0.167***	-0.105***	-0.073
	(0.036)	(0.044)	(0.037)	(0.054)	(0.035)	(0.092)
	D. With controls for time-varying school inputs					
Switcher*Post	-0.095**	-0.049	-0.180***	-0.160***	-0.097***	-0.070
	(0.038)	(0.041)	(0.041)	(0.054)	(0.037)	(0.093)
Observations	410,898	410,898	410,288	410,288	388,720	388,720
Number of clusters	135	135	135	135	135	135
Switcher*Linear Trend		Yes		Yes		Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
District*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the estimates of equation 2. It excludes the two transition cohorts (event years 1 and 2) who had a partial exposure to coed school environment while having same-sex peers in own cohort for three years. Panels A and C have no additional controls. Panels B and D condition on all time-varying school-level observables: share of female teachers, class size, pupil-teacher ratios, log of school size, pupil-administrator ratios, and percentage of female administrators. Switcher is a dummy indicating whether a school ever changes its type from single-sex to coeducational (there are no changes in the opposite direction). Post indicates exposure to both school- and class-level coed (versus single-sex) environment for three years. Columns 2, 4, and 6 allows for linear trend for switching schools. Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Diff-in-diff estimates of class level exposure to mixed-gender (versus same-sex) peers

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent var.: CSAT score in					
	Korean		English		Math	
	Boys					
	A. No controls					
Switcher*CSPost	-0.021	-0.053**	0.005	-0.027	-0.012	-0.023
	(0.026)	(0.027)	(0.024)	(0.029)	(0.035)	(0.035)
	B. Controls for time-varying school inputs					
Switcher*CSPost	-0.024	-0.055*	0.002	-0.030	-0.009	-0.020
	(0.026)	(0.029)	(0.025)	(0.032)	(0.036)	(0.036)
Observations	471,925	471,925	470,870	470,870	457,658	457,658
Clusters	143	143	143	143	143	143
	Girls					
	C. No controls					
Switcher*CSPost	-0.108***	-0.081**	-0.125***	-0.154***	-0.026	-0.039
	(0.026)	(0.036)	(0.034)	(0.041)	(0.047)	(0.066)
	D. Controls for time-varying school inputs					
Switcher*CSPost	-0.098***	-0.071**	-0.119***	-0.149***	-0.022	-0.035
	(0.028)	(0.036)	(0.036)	(0.042)	(0.050)	(0.068)
Observations	403,948	403,948	403,343	403,343	381,509	381,509
Clusters	135	135	135	135	135	135
Switcher*Linear Trend		Yes		Yes		Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimates of equation 3. Sample is restricted to event years 1, 2, 3, ... (those who were exposed to school-level coed environment) at switching schools and their counterparts at non-switching schools. Columns 2, 4, and 6 allows for linear trend for switching schools. Panels A and C have no additional controls. Panels B and D condition on all time-varying school-level observables: share of female teachers, class size, pupil-teacher ratios, log of school size, pupil-administrator ratios, and percentage of female administrators. Switcher is a dummy indicating whether a school ever changes its type from single-sex to coeducational (there are no changes in the opposite direction). CSPost indicates exposure to mixed-gender (versus single-sex) peers for three years. CSPost=0 for event years 1 and 2. CSPost=1 for event years 3 or later. Robust standard errors clustered by school are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

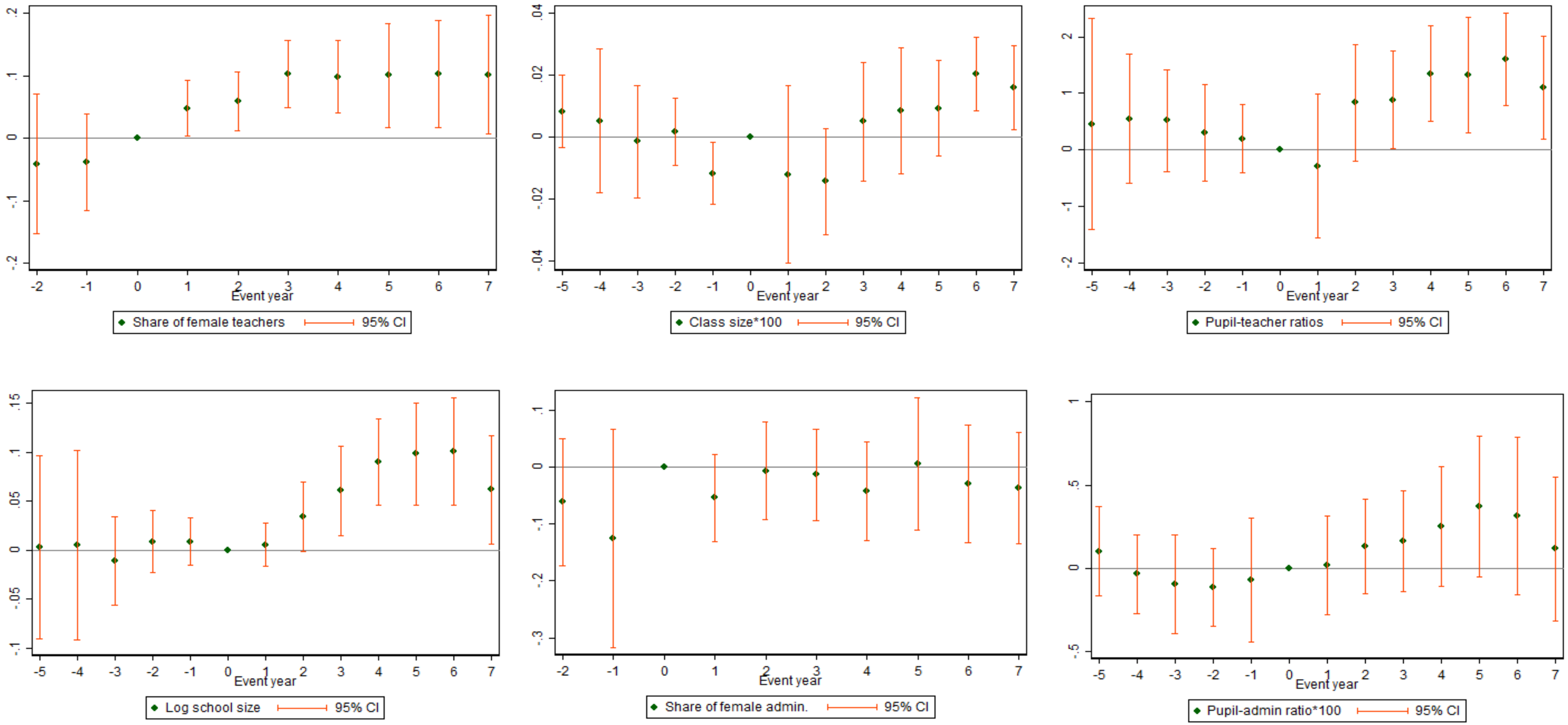
Table 6: Student turnover by treatment status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Boys				**Girls**			
Dependent var.:	Turnover		Exam taking		Turnover		Exam taking	
Mean of Dependent var:	-0.030		0.921		-0.020		0.937	
Switcher*CSPost	0.001	0.022	-0.008	0.016	0.007	-0.009	0.018	0.026
	(0.007)	(0.014)	(0.009)	(0.016)	(0.016)	(0.027)	(0.011)	(0.020)
Switcher*Linear Trend		Yes		Yes		Yes		Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,126	1,126	1,126	1,126	1,036	1,036	1,036	1,036
R-squared	0.477	0.479	0.537	0.539	0.370	0.370	0.395	0.395

Notes: This table reports the estimates of equation 3 using turnover and exam taking as the dependent variable, respectively. The unit of observation is school-cohort. Turnover measures the number of enrollment in the 12th grade in a given school divided by the number of enrollment in the 10th grade two years prior to that, to capture the turnover rates for the same cohort in that school. Exam taking measures the number of CSAT takers divided by the number of enrollment in the 12th grade in the same year, to capture the share of currently enrolled students who take the test. Sample is restricted to event years 1, 2, 3, ... (those who were exposed to school-level coed environment) at switching schools and their counterparts at non-switching schools. Switcher is a dummy indicating whether a school ever changes its type from single-sex to coeducational (there are no changes in the opposite direction). CSPost indicates exposure to mixed-gender (versus single-sex) peers for three years. CSPost=0 for event years 1 and 2. CSPost=1 for event years 3 or later. Robust standard errors clustered by school are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

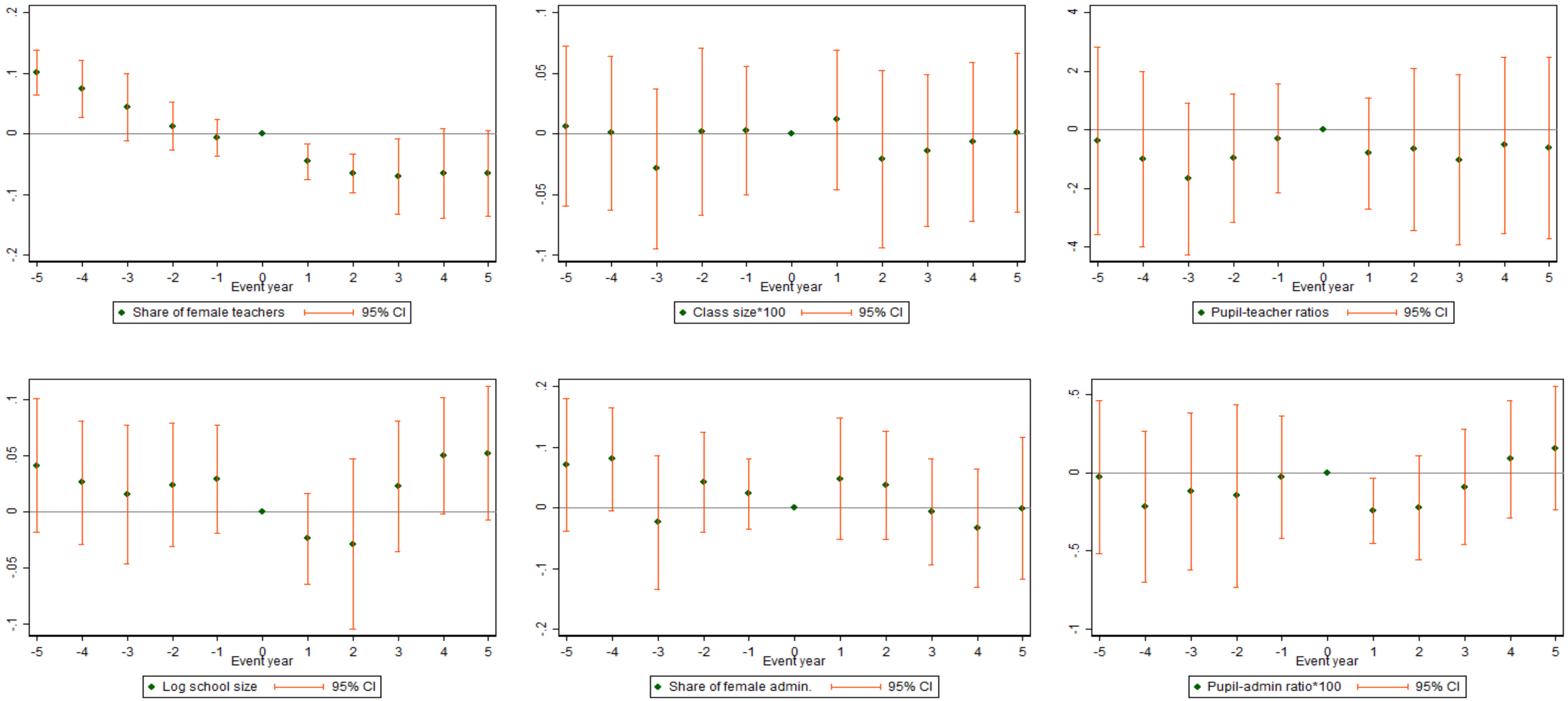
Appendix

Figure A.1: Event study of school type change from single-sex to coed: Time-varying school-level observables - Boys



Notes: This figure plots the coefficients from estimating equation 4 using time-varing school characteristics as the dependent variable. Coefficient for event year 0 is normalized at zero. All regressions include school FE and district-year FE. Standard errors are clustered by school.

Figure A.2: Event study of school type change from single-sex to coed: Time-varying school-level observables - Girls



Notes: This figure plots the coefficients from estimating equation 4 using time-varing school characteristics as the dependent variable. Coefficient for event year 0 is normalized at zero. All regressions include school FE and district-year FE. Standard errors are clustered by school.

Table A.1: The role of observable school characteristics in explaining the advantage of single-sex schools

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent var.: CSAT score in											
	Korean		English		Math		Korean		English		Math	
	A. Boys						B. Girls					
Coed (versus single-sex) school	-0.074***	0.031	-0.096***	0.030	-0.063**	0.042	-0.051**	-0.014	-0.069**	-0.028	-0.060**	-0.031
	(0.025)	(0.034)	(0.031)	(0.042)	(0.026)	(0.036)	(0.022)	(0.034)	(0.033)	(0.048)	(0.029)	(0.044)
Private (versus public)		-0.081**		-0.099**		-0.090**		0.010		-0.009		-0.015
		(0.040)		(0.049)		(0.040)		(0.039)		(0.055)		(0.049)
Recently established		-0.031		-0.052		-0.013		0.014		-0.008		0.001
		(0.035)		(0.042)		(0.037)		(0.024)		(0.033)		(0.032)
Share of female teachers		-0.453***		-0.533***		-0.481***		0.098		0.080		0.017
		(0.125)		(0.166)		(0.140)		(0.093)		(0.135)		(0.120)
Share of female admin.		-0.045		-0.060		-0.069		0.014		0.007		-0.029
		(0.063)		(0.076)		(0.067)		(0.050)		(0.075)		(0.065)
Class size*100		-0.190		-0.206		-0.095		0.542*		0.505		0.507
		(0.361)		(0.445)		(0.386)		(0.322)		(0.425)		(0.416)
Pupil-teacher ratios		0.013**		0.016**		0.011*		0.015***		0.024***		0.019**
		(0.005)		(0.007)		(0.006)		(0.005)		(0.007)		(0.007)
Pupil-admin ratios		0.002		-0.005		-0.003		-0.007		-0.009		-0.003
		(0.018)		(0.022)		(0.019)		(0.008)		(0.011)		(0.010)
Log school size		0.111		0.131		0.122		0.055		0.061		0.077
		(0.075)		(0.090)		(0.077)		(0.052)		(0.074)		(0.067)
School FE	No	No	No	No	No	No	No	No	No	No	No	No
District*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	481,122	481,122	480,039	480,039	466,732	466,732	420,346	420,346	419,706	419,706	397,278	397,278
R-squared	0.027	0.029	0.050	0.053	0.035	0.038	0.019	0.021	0.040	0.043	0.033	0.035

Notes: This table reports the estimates of equation 1 using the CSAT scores as the dependent variable. Sample is restricted to the periods (1999-2009) for which we have the full set of time-varying school-level observables. Odd numbered columns have no additional controls. Even numbered columns include all school characteristics in the same regression. Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.2: The role of observable school characteristics in explaining the advantage of single-sex schools: Exclude switching schools from sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent var.: CSAT score in											
	Korean		English		Math		Korean		English		Math	
	A. Boys						B. Girls					
Coed (versus single-sex) school	-0.075***	0.034	-0.099***	0.029	-0.067**	0.036	-0.045*	-0.008	-0.062*	-0.025	-0.050	-0.018
	(0.027)	(0.039)	(0.034)	(0.049)	(0.029)	(0.041)	(0.024)	(0.038)	(0.036)	(0.054)	(0.031)	(0.049)
Private (versus public)		-0.097**		-0.116**		-0.111**		0.009		-0.015		-0.014
		(0.044)		(0.053)		(0.043)		(0.041)		(0.059)		(0.052)
Recently established		-0.030		-0.047		-0.008		-0.004		-0.032		-0.024
		(0.040)		(0.048)		(0.042)		(0.025)		(0.033)		(0.033)
Share of female teachers		-0.486***		-0.561***		-0.505***		0.073		0.029		-0.003
		(0.136)		(0.181)		(0.152)		(0.100)		(0.146)		(0.133)
Share of female admin.		-0.034		-0.051		-0.063		0.031		0.039		-0.013
		(0.069)		(0.084)		(0.074)		(0.052)		(0.077)		(0.068)
Class size*100		-0.161		-0.170		-0.043		0.561*		0.503		0.559
		(0.370)		(0.459)		(0.398)		(0.335)		(0.445)		(0.432)
Pupil-teacher ratios		0.012**		0.016**		0.011*		0.015***		0.023***		0.018**
		(0.005)		(0.007)		(0.006)		(0.005)		(0.007)		(0.007)
Pupil-admin ratios		0.006		-0.003		-0.000		-0.007		-0.010		-0.003
		(0.018)		(0.022)		(0.019)		(0.008)		(0.011)		(0.011)
Log school size		0.114		0.129		0.119		0.057		0.074		0.080
		(0.076)		(0.091)		(0.078)		(0.053)		(0.076)		(0.070)
School FE	No	No	No	No	No	No	No	No	No	No	No	No
District*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	452,828	452,828	451,813	451,813	439,426	439,426	395,321	395,321	394,730	394,730	373,751	373,751
R-squared	0.026	0.029	0.050	0.054	0.036	0.038	0.019	0.021	0.041	0.044	0.033	0.035

Notes: This table reports the estimates of equation 1 using the CSAT scores as the dependent variable. Sample is restricted to the periods (1999-2009) for which we have the full set of time-varying school-level observables. Schools that switch from single-sex to coed over time are excluded from sample so that only cross-sectional variation is used to estimate the total effects. Odd numbered columns have no additional controls. Even numbered columns include all school characteristics in the same regression. Robust standard errors clustered by school in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.3A: Excluding switching schools one by one - Boys

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent var.: CSAT score in					
	Korean		English		Math	
			Exclude switching school 1			
Switcher*CSPost	-0.013	-0.048	0.008	-0.037	-0.020	-0.037
	(0.030)	(0.030)	(0.028)	(0.031)	(0.040)	(0.039)
Observations	636,660	636,660	635,571	635,571	622,485	622,485
			Exclude switching school 2			
Switcher*CSPost	-0.002	-0.055*	0.018	-0.026	0.013	0.007
	(0.025)	(0.029)	(0.026)	(0.031)	(0.033)	(0.027)
Observations	637,049	637,049	635,955	635,955	622,835	622,835
			Exclude switching school 3			
Switcher*CSPost	-0.041**	-0.057*	-0.016	-0.025	-0.031	-0.009
	(0.021)	(0.031)	(0.018)	(0.033)	(0.036)	(0.037)
Observations	636,661	636,661	635,579	635,579	622,504	622,504
			Exclude switching school 4			
Switcher*CSPost	-0.029	-0.065***	-0.000	-0.032	-0.025	-0.032
	(0.027)	(0.025)	(0.026)	(0.029)	(0.036)	(0.038)
Observations	637,017	637,017	635,928	635,928	622,809	622,809
			Exclude switching school 5			
Switcher*CSPost	-0.012	-0.065**	0.018	-0.041	0.012	-0.034
	(0.029)	(0.027)	(0.026)	(0.030)	(0.034)	(0.039)
Observations	636,750	636,750	635,659	635,659	622,470	622,470
			Exclude switching school 6			
Switcher*CSPost	-0.012	-0.045	0.012	-0.030	-0.008	-0.028
	(0.030)	(0.029)	(0.028)	(0.032)	(0.041)	(0.041)
Observations	636,488	636,488	635,401	635,401	622,327	622,327
			Exclude switching school 7			
Switcher*CSPost	-0.023	-0.045	0.003	-0.009	-0.020	-0.029
	(0.029)	(0.027)	(0.027)	(0.023)	(0.038)	(0.038)
Observations	637,002	637,002	635,911	635,911	622,772	622,772
Switcher*Linear Trend		Yes		Yes		Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table re-estimates our main specification (equation 3, Panel A of Table 5) while excluding one policy switcher at a time to ensure that one particular school is not driving our main findings. Sample is restricted to event years 1, 2, 3, ... (those who were exposed to school-level coed environment) at switching schools and their counterparts at non-switching schools. Columns 2, 4, and 6 allows for linear trend for switching schools. Panels A and C have no additional controls. Panels B and D condition on all time-varying school-level observables: share of female teachers, class size, pupil-teacher ratios, log of school size, pupil-administrator ratios, and percentage of female administrators. Switcher is a dummy indicating whether a school ever changes its type from single-sex to coeducational (there are no changes in the opposite direction). CSPost indicates exposure to mixed-gender (versus single-sex) peers for three years. CSPost=0 for event years 1 and 2. CSPost=1 for event years 3 or later. Robust standard errors clustered by school are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.3B: Excluding switching schools one by one - Girls

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent var.: CSAT score in					
	Korean		English		Math	
	Exclude switching school 1					
Switcher*CSPost	-0.132***	-0.103***	-0.141***	-0.165***	-0.038	-0.058
	(0.014)	(0.032)	(0.035)	(0.045)	(0.060)	(0.073)
Observations	533,198	533,198	532,582	532,582	510,902	510,902
	Exclude switching school 2					
Switcher*CSPost	-0.095***	-0.072*	-0.097***	-0.130***	0.009	-0.015
	(0.026)	(0.037)	(0.024)	(0.032)	(0.048)	(0.070)
Observations	533,273	533,273	532,658	532,658	510,957	510,957
	Exclude switching school 3					
Switcher*CSPost	-0.100***	-0.073*	-0.139***	-0.159***	-0.070*	-0.069
	(0.031)	(0.038)	(0.038)	(0.045)	(0.036)	(0.066)
Observations	533,645	533,645	533,034	533,034	511,298	511,298
	Exclude switching school 4					
Switcher*CSPost	-0.102***	-0.078*	-0.124***	-0.178***	-0.004	-0.025
	(0.037)	(0.045)	(0.047)	(0.047)	(0.062)	(0.080)
Observations	533,138	533,138	532,522	532,522	510,955	510,955
Switcher*Linear Trend		Yes		Yes		Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table re-estimates our main specification (equation 3, Panel A of Table 5) while excluding one policy switcher at a time to ensure that one particular school is not driving our main findings. Sample is restricted to event years 1, 2, 3, ... (those who were exposed to school-level coed environment) at switching schools and their counterparts at non-switching schools. Columns 2, 4, and 6 allows for linear trend for switching schools. Panels A and C have no additional controls. Panels B and D condition on all time-varying school-level observables: share of female teachers, class size, pupil-teacher ratios, log of school size, pupil-administrator ratios, and percentage of female administrators. Switcher is a dummy indicating whether a school ever changes its type from single-sex to coeducational (there are no changes in the opposite direction). CSPost indicates exposure to mixed-gender (versus single-sex) peers for three years. CSPost=0 for event years 1 and 2. CSPost=1 for event years 3 or later. Robust standard errors clustered by school are in parentheses. *** p<0.01, ** p<0.05, * p<0.1