Single-Sex Primary Schools and Student Achievement: Evidence from Admission Lotteries*

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Students at single-sex schools typically perform above national average in test scores. However, whether same-gender schooling is an appropriate intervention strategy to boost academic achievement is still an open question. I leverage randomized assignment of 4 and 5 years old children to schools in Malta to estimate the causal effect of single-sex schooling on national standardized test scores at the end of primary school. To alleviate concerns of endogenous school inputs, I compare students within the same school sector, for which coeducational and single-sex schools looks alike in many important dimensions except for the gender composition of the student population. I find that attending a single-sex school produces large and significant score gains in language (English and Maltese) and Math tests. Subgroup analysis shows that single-sex schools benefit both girls and boys, and that gains in attainment are not driven by students with the strongest preferences for single-sex schools. JEL codes: C26, D47, I21, I28, J16

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1 Introduction

Single-sex schooling used to be the predominant mode of education and is still prevalent in many English-speaking and Muslim-majority countries. Moreover, it has recently spread across the US following the revision of Title IX, a federal legislation that broke the historical link between private and single-gender education by allowing school districts to provide boys-only and girls-only schools and classrooms.[1] Yet, there is little causal evidence that single-sex schools are more effective in increasing test scores than their coeducational (coed) counterparts. Understanding if same-gender learning environments enhance quality of education is especially important in the actual context in which (a) school choice policies are becoming a common feature of educational systems worldwide, and the improvement of academic outcomes for participants is a necessary condition for these policies to be efficiency-enhancing (Deming et al., 2014; Cullen, Jacob, and Levitt, 2006)[2] and; (b) there is an increasing interest in understanding the scope of school inputs, and homogeneous gender environments in particular, in reducing the gender gap in math (Fryer and Levitt, 2010), non-cognitive achievement (Bertrand and Pan, 2013) and career choices (Schneeweis and Zweimüller, 2012; Park, Behrman, and Choi, 2018).

The rationale for gender segregation in formal education is based on the belief that single-sex environments respect gender differences in learning better than coed settings. Boys and girls seem to have dissimilar paths in their neurological development and socialization styles and teachers, by facing same-gender classrooms, may be better at tailoring instructions according to each gender needs.[3] Additionally, students seem to be less disruptive in same-gender classes (Francis, 2000; Lavy and Schlosser, 2011) and less prone to conform gender stereotypes salient in coed settings (Wilder and Powell, 1989; Kessels and Hannover, 2010).

Studies that compare the academic achievement of students enrolled in single-sex and coed schools show, consistently, that the first group outperform the second one (Malacova, 2007; Lee and Bryk, 1986). However, it is a challenge to disentangle if this evidence is due to the effect of having same-sex peers or due to the fact that students who choose to attend a single-sex school differ in important unobserved ways from those that choose a coed school. Moreover, even when it is possible to control for the sorting of students, single-sex schools may differ in other unobservable dimensions from the coed counterpart, beyond

[1] In 2006, changes in Title IX and amendments to the No Child Left Behind Act allowed government schools in the US experimenting with single-sex education. As of the 2011-2012 school year, around 390 public coed schools offered single-sex classrooms, while 116 were transform to single-sex schools entirely (NASSPE, 2012).

[2] School choice policies depart from traditional school assignment by catchment area and have been implemented in a variety of ways, from *open enrollment* policies to school vouchers.

[3] According to neuroscience research, cerebral volume peaks at 14.5 years for boys and 11.5 years for girls (Lenroot and Giedd, 2006). At early ages, girls show higher ability to focus than boys while the opposite is true for activity levels. There is also a well documented gap in willingness to compete by Niederle and Vesterlund (2007) and Dohmen et al. (2011). See Booth and Patrick (2012) and Lee, Niederle, and Kang (2014) for gender differences in tournament participation conditional on attending a same-sex schools.

classroom gender composition. By comparing the performance of (randomly allocated) students at dissimilar schools one is at risk of accounting for a significant single-sex effect that is actually due to other channels in which these school types differ.

In this paper, I leverage admission lotteries to identify the causal effect of single-sex education on the academic achievement of elementary school students. The Secretariat of Catholic Education, the central office of catholic education in Malta, offer the most sought-after (coed and single-sex) schools in the island. In order to allocate primary school slots to students, the Secretariat uses a centralized clearinghouse based on students preferences, school priorities and random lottery numbers. I use lottery outcomes as an instrument for single-sex schooling in order to deal with non random sorting and non-compliance. Using rich administrative data, I provide evidence that coed and single-sex schools run by the Secretariat are similar in a variety of school level inputs (class size; teacher gender, experience and qualifications; and curricula among others). By comparing students at schools that are indistinguishable in many important dimensions, I disentangle the same-gender effect from the single-sex school effect.

I first show lottery-based estimates for the single-sex school effect, which has in the reference group coed students from both state and catholic schools. Although this parameter may not necessarily measure the pure effect of having same gender peers, it is the measure that parents care the most when deciding which school type is a good match for their kids. It is also informative about the probable size of the true causal effect of same gender peers. The results of these *naive* estimates suggest a positive effect on standardized English and Maltese language test scores, but no effect on math. In order to isolate the same-gender peers effect, I use the fact that single-sex and coed catholic are similar in many observable school characteristics. I consider a specification that accounts for the differences that catholic schools may have from state schools per se, and compute the effect that single-sex school have above and beyond the catholic effect. This estimate yield positive and significant single-sex school effect on achievement in all subject areas, though the effects are larger for the language subjects than for math. One of the most interesting findings from this study is the fact that these results holds for both, girls-only and boys-only schools. Using parents' rank-order list of schools, I then explore heterogeneous effect by estimating a mixed rank-ordered logit model to identify how the effects change conditional on the weight parents place at different schools characteristics. In contrast with previous studies, single-sex school effect is not driven by students with the strongest preferences for single-sex schools.

Besides solving the problem of non-random sorting of students across school types, the Maltese setting offers additional advantages. First, selective attrition is not a concern. Students moving out of district pose a problem to many lottery-based studies (and different assumptions produce conflicting estimates), while I work with a centralized lottery and admin data for the whole Malta island. Second, single-sex schools are a rather homogeneous group within the Maltese school system, so assertion is not built by averaging the effect of many heterogeneous schools. Third, although lotteries are not necessarily externally valid (those who choose to participate may not necessarily come from a random process and therefore may not be representative of the general student population), extrapolation of the results is less challenging if the amount of participants in the admission lotteries is high. Catholic schools in Malta have a minimum of 5 candidates per slot. Moreover, catholic schools in Malta seems to serve students that would otherwise attend their state neighborhood school. Lastly, the single-sex schools analyzed here are particularly relevant for the country accountability mandate as these schools are state-funded and part of the mainstream education system.

This paper makes a contribution to two strands of the literature. To the literature on determinants of school achievement, I convincingly show, by exploiting random variation, that having same gender-peers at elementary level improves student academic achievement. Well-identified causal effect estimates of having same-gender peers on academic outcomes comes from researchers leveraging quasi-experimental assignment variation in Trinidad y Tobago (Jackson, 2012), South Korea (Park, Behrman, and Choi, 2013; Lee et al., 2014; Choi, Moon, and Ridder, 2014; and Dustmann, Ku, and Kwak, 2018), and Switzerland (Eisenkopf et al., 2015). Overall, these studies show only moderate differences between the typical students at single-sex and coed schools once the comparison is restricted to students at schools that seem to differs in their gender composition only. Both Jackson (2012) and Choi, Moon, and Ridder (2014) also show that their estimates mask considerable heterogeneity. In the former, single-sex schooling improves test scores only for girls with strong preferences for single-sex schools. In the latter, when estimating the effect for each districts, which is the unit at which randomization occurs in Seoul, only half shows a positive and significant effect, suggesting endogenous sorting of individuals to districts. Moreover, a common feature of these studies is that they measure the effect of single-sex schooling at high school ages.[4] This is the first paper, to the best of my knowledge, that leverage experimental variation to estimate the effect of single-sex education at childhood age. This is important since gender interactions seems to change considerably by age and, therefore, results found at higher level of education may not apply for students at elementary schools. In addition, given that childhood is a period in which school inputs are most effective in improving students cognitive development (Chetty et al., 2011; Heckman, Pinto, and Savelyev, 2013), and where the foundation of gender gaps laid, then it is imperative to inform the debate on single-sex education about the effect of having same-gender peers at elementary levels.

I also make a contribution to the literature that uses lotteries in a school context. Admission lotteries have been used to study the impact of attending high performing schools through vouchers (Rouse, 1998; Angrist et al., 2002; Abdulkadiroğlu, Pathak, and Walters, 2018) or by school choice policies (Cullen, Jacob, and Levitt, 2006; Deming et al., 2014). There is also a large number of studies focused on the "No Excuses" charter school effect

[4] See Booth, Cardona-Sosa, and Nolen (2018) and Oosterbeek and Ewijk (2014) for evidence of gender composition within classes at tertiary education.

(Hoxby and Murarka, 2008; Angrist et al., 2010; 2012; Dobbie and Fryer, 2015).[5] Most of these studies use school-specific lotteries for a small share of slots after distance-based priority slots are secured for local students. In this paper, I leverage centralized admission lotteries for schools that serve 30 percent of the student population in the country and where the majority of slots are subject to random rationing. There is also no distance priority in the assignment mechanism, and thus school classes are geographically diverse.

The organization of the remainder of the paper is as follows. Section 2 provides an overview of the institutional background, the admission lottery system and describes the data and the sample used in this analysis. Familiarity with the institutional setting will permit a clearer interpretation of the identification strategy. Section 3 outlines the standard lottery-based estimation framework and the alternative causal strategies that consider the idiosyncratic nature of noncompliance in this setting. Section 4 presents the main estimates of single-sex school graduation on academic outcomes for different specifications of the *risk set* accounting for reapplication patterns and parent preferences. Section 5 explores heterogeneous effects while the last section, Section 6, concludes and outlines an agenda for future work.

2 Institutional Setting and Data Description

2.1 Malta Education System

Malta, a high-income and service-based country, became a British colony in 1813 and gain independence on September 1964. English is one of the two official languages, together with Maltese. Due to its colonial inheritance, Malta education and examination systems closely follow the British model. Compulsory education covers primary (ages 5-11) and secondary (ages 12-16) education and students take national and externally graded exams at the end of each stage. Kindergarten is not compulsory, but attended by almost all 3 and 4-years-old.[6] The school year runs from mid/late September to early/mid June and there are three mainstream education providers: the Government (state schools), the Church (catholic schools) and the Independent sector (private schools).

Interestingly, catholic schools in Malta use a centralized lottery to ration seats. Since the nineties, Malta introduced several measures and educational reforms in order to democratize access and increase parental school choices.[7] As a consequence, the Catholic sector became embedded in the mainstream public school system. They ceased charging tuition fees, implemented the National Curriculum and the National Minimum Conditions Regulations

[5]This reference list is not exhaustive. For a detailed summary of the lottery-based charter school research see Chabrier, Cohodes, and Oreopoulos (2016).

[6]Participation rates in 2014 were 95.4 and 97.7 percent for kindergarten I and kindergarten II, respectively (OECD Stats EU-SILC, 2014). Note that entry to each education level in Malta is on a birth-year basis, so enrollment in kindergarten I is determined by the calendar year a child turns 3, 4 for kindergarten II, and so on.

[7]See Cutajar (2007) for an overview of the educational reforms in Malta, specially after independence.

(which establish standards of hygiene, safety and classroom dimensions and amenities), and became funded by the national government.[8] More importantly, after the agreement, catholic schools were not allowed to select students on faith grounds or any other trait. Given the fact that catholic schools were highly sought-after institutions, the Secretariat for Catholic Education decided to use lotteries in order to deal with students admission.[9]

In this context, independent schools are an expensive alternative and thus it is reasonably to think that catholic schools are attended by students that otherwise would attend traditional state schools, predominantly in their neighborhood.[10] As of 2015, the distribution of students across primary schools was 56 percent state, 31 percent catholic and 13 percent private. In terms of spatial distribution, state schools are spread across the island while the catholic ones are highly concentrated in the Northern and Southern harbors. This can be seen in **Figure 1**, which maps the primary school locations by school sector. Another distinct characteristic of catholic schools is that they can be either coed (4) or single-sex schools (10 girls-only and 7 boys-only), while all state primary schools in Malta are coeducational.[11,12] When comparing academic achievement of students across school types, single-sex schools stand out. Difference in performance are shown in **Figure 2**, which plots estimates of school quality coming from a regression of a standardized composite test score at the end of primary school on a full set of year and school fixed effects. While students at coed schools achieve below the national average, those at single-sex schools are disproportionately on the right side of the distribution.

The lottery application process is simple. In January each year, parents submit an application on a paper form they pick up from the Secretariat of Catholic Education or any catholic school (see template in Appendix **Figure A1**).[13] They need to provide basic demographic information (detailed further below) and an unrestricted rank-order list (ROL) of schools.[14] There is no requirement for participating in the lottery, that is, parents do not

[8]The government fully funded the budgeted salaries of the staff and gives additional 10 percent to cover other operational expenses. Catholic schools do have the right to ask for voluntary donations and parents pay for school supplies.

[9]The island of Gozo, the second-largest island of the Maltese archipelago with 8 percent of the Maltese population, implemented a separated admission lottery for the 4 catholic primary schools in the island. Gozitan lotteries are not part of the sample used in this paper.

[10]Average tuition fee in private schools was around 3600 euros annually (14 percent of the average household disposable income) by the 2014-2015 school year according to Malta Today (2015). Student admission at state primary schools is determined by town of residence (locality). Principals may accept students coming from a different town if the parents request it, though in conversations with the staff at the Ministry of Labor and Education indicated that these are exceptional cases.

[11]Three of the boys-only schools opened in 2011 with rolling-up in Year 1 and Year 4. In most of the specifications, these schools do not contribute to the lottery sample, specially because the admission lottery slightly changed since then and did not request parents to submit a rank-order list of schools anymore.

[12]At secondary education, single-sex schools were the norm until recently. Since 2014, a few state-run secondary schools are gradually transitioning to mixed-gender schools.

[13]Data collection for this paper involved the scanning and digitization of the individual application forms. The preservation of lottery data was not a priority after the admission process was completed and prevent me to get information before 2008.

[14]There are no limits on the length of the ROL, which means that parents can rank as many schools as they like from the ones available for a particular intake grade and year.

need to satisfy any criteria to make their child a candidate, and it is costless.[15] Lotteries are held around April or May and there are different admission lotteries for each traditional intake grade.[16]

Matching mechanism The Secretariat for Catholic Education follows a centralized single-offer allocation mechanism in which single random lottery numbers act as a tie-breaking variable by placing students in a queue, and then processes students in that order. Assignment to schools proceeds as follows: the first student in the queue obtains her most preferred school as stated in the application form, the subsequent student obtains her top choice among schools with slots remaining, and so on until no seat remains in any school. Although boys and girls are placed in the same queue, capacities by school are set by gender beforehand. Obviously there is no decision of gender in single-sex schools and coeducational school split slots evenly. This means that, even though there is a unique lottery draw for all applicants, the assignment mechanism can be though as two separated assignment mechanism, one for each gender.

In the mechanism design literature, this scheme is referred to as a random serial dictatorship (RSD) and is the easiest mechanism to implement in an allocation problem. RSD is strategy-proof. By only using the preference over schools information when it is the applicant's turn to make the choice, the best strategy for an applicant is to report truthfully. This mechanism also has the property of being fair (equal treatment of equals), since each student has the same chance to appear in each position in the queue (Pathak, 2011).

School Priorities Schools also ration their seats using priorities. Importantly, priorities are not school-specific (i.e., do not vary across schools). The priority groups are arranged in lexicographic order based on the following traits: applicants with already-enrolled siblings; children from church-run homes; children of employees; special cases (low-income family, single-mother); and children with special educational needs (SEN). Due to national regulations, the number of SEN students is capped at two slots per classroom, so a separate lottery is held for them. Based on preferences, slots at each school are first allocated to students belonging to these priority groups, the rest of the vacancies are filled by the single lottery number as described above.

Admission lotteries are competitive. Every year, above 1000 children are entered in the lottery for each intake grade and, on average, less than 30 percent receive an offer. The number of birth per year in the Maltese island is around 3400, which means that around 30 percent of the cohort population participates in the admission lotteries. Given that

[15] There was no charge for applying up to 2010. Administrative cost were introduced in 2011 of about 10 euros by that time.

[16] Places at higher grades are only made available when a student leaves, which is a very infrequent event. These slots are allocated at the discretion of the principals.

admission lotteries do not necessarily draw applicants from the general student population, this fact is important for the external validity of the results. There is no reason to assume that those who sign up for a catholic school slots are coming from a random process. Thus evaluating lottery winners and losers may reveal little about the effect of schools on non-participant, although extrapolation of results is less of a challenge if the amount of participants is quite high as in this context.

In this setting, all schools face a high demand. Almost 50 percent of the parents rank all feasible schools and among those that rank a fewer number of schools, the average is to rate about 60 percent of the schools. A detailed distribution of parents across shares of school ranked is shown in **Figure 3**). The ROL of schools also reveal that parents prefer single-sex over coed schools, and this hold independently of the applicant's gender. **Figure 4** shows average ranking per school for girls and boys separately. Note that the lower the value, the more attractive is the school. Preferences play a central role in lottery-based studies at isolating the random components of the data-generating mechanism. The empirical strategy in Section 3 will discuss this further.

2.2 Data and Sample Selection

I combine data from three individual-level datasets: (a) the application forms, which contain the student full name, gender, date of birth, parent's name, father or mother ID, home address, sibling relation, special educational need status and the preference order of schools; (b) administrative records with school capacities, the student priority grouping, lottery numbers and lottery outcomes (i.e., identifying which applicants were the successful candidates and to which school they were matched); (c) the End of Primary Benchmark (Benchmark), a standardized national examination at age 11, when Year 6 students finish mandatory primary education. The Benchmark is administered every June and assesses English, Maltese and Math skills.

Test scores were standardized by subject and year to have mean zero and unit variance among all test-takers. Thus, the resulting compiled data consists of lottery participants and their academic performance on standardized tests by the end of primary school. Recall that the centralized admission mechanism uses a mix of school priorities and lottery numbers to ration seats, and that students at priority groups have guaranteed access (except SEN). Consequently, I limit the regression sample to students in the marginal priority group, that is, applicants for which lottery number alone determine whether they get an offer or not. Lotteries for SEN students are part of the analysis sample, though I exclude degenerate lotteries (lotteries with no losers).[17]

I use all the lotteries held for KG2 and Y1, which are the traditional intake grades

[17]SEN lotteries can be small, specially for girls. With the exception of one of the lotteries for this group, there is no variation in the instrument as all the girls got an offer. Thus, they are omitted from the lottery analysis.

of single-sex schools for student cohorts 2005 and 2006.[18] The sample should also be restricted to applicants to the relevant sector, i.e., those that apply to single-sex school by ranking them anywhere in the application form, and exclude those than only rank coed schools. This is of little concern since, among the randomized applicants, only 10 parents have exclusively chosen coed schools in their submitted ROL of schools. For cohort 2005, I also use the lottery held in Y4 to rolling-up students in three new boys-only schools. Although the sample of lotteries is bigger than the one used in this study, the focus is on available lotteries through time for the two cohorts under analysis. The main reason for this is that reapplication rates in this setting are consistently high and tracking the full application patterns of student may be important for identification if, for example, first-time applicants are systematically different from second or third-time applicants. I will come back to this point when defining risk sets in the next section.

The overall match rate of students at marginal priority group is 82 percent. Several reasons explain not achieving a perfect match: (i) students may have moved outside Malta, (ii) there are 3 catholic schools that do not participate in the End of Primary Benchmark and, (iii) the application data per lottery vary in term of key variables for matching.[19] When the student ID number was not available the match keys used were: (a) surname, name and date of birth; (b) surname name and mother name; (c) surname, name and father name. The match differential for lottery winners and losers is about -10.6 percent, so the probability of being matched is lower if the applicant is a lottery winner. This is consistent with (ii), so when applicants who received an offer from any of the three non-participating schools are excluded, the coefficient falls to 0.002 (SE = 0.015), indicating that admission lottery winners are not more likely to be matched than losers.[20] Additional details on the match rates per grade and year of the lottery are available in Appendix **Table B1**.

The evidence on the mechanisms behind single-sex education is based on survey data from the Progress in International Reading Literacy Study (PIRLS) 2016. PIRLS is an international assessment conducted by the International Association for the Evaluation of Educational Achievement (IEA) and designed to measure trends in reading comprehension skills among 10-year old students. The survey includes besides some students and teachers background data, information about the home literacy environment, the school curriculum and curriculum implementation and the instructional practices. The target population in

[18] Different schools differ in the school grade they start admitting students. For example, all coed catholic schools start serving student since KG1, while single-sex school do it from KG2 or Y1. However, this does not mean that coed school participate exclusively in KG1 lottery. Due to class size regulations, class sizes are limited to 15 and 20 for KG1 and KG2, respectively, and to 30 students for Y1 to Y6. Consequently, coed schools participate in each grade lottery, topping up classrooms (and single-sex school with traditional intake grade in KG2 also participates in Y1 lotteries following the same logic).

[19] For lotteries held before 2011 it was not possible to get digital file formats and data was recovered from archived documentation. By that time, the preservation of records was not a priority after the school admission process was completed and having incomplete data for some lotteries is more the result of bad luck when the data collection started in 2016 than to a selective matching.

[20] Notice that these students would not be part of the regression sample anyway and that being a participating school is a fact that is unlikely to be related to whether a student is a lottery winner or loser.

Malta consists of students enrolled in Year 5 of compulsory primary education in 2016, a year before the Benchmark examination.[21] I link the PIRLS data to the lottery data by date of birth and school.[22]

2.3 Applicant Characteristics and Balance

I use data from the application records to construct a set of predetermined variables to test the validity of the randomization and to examine treatment effect heterogeneity. I geocoded student home addresses to compute travel time and distance to neighborhood school. I also use locality information to compute the average social benefit expenditures in the student's neighborhood. I classify student's name into a biblical name category and student's surname into a popular surname category.[23] If a child's biblical name reveals deep religious beliefs, a way to check that catholic school admission lotteries are truly randomized is by checking that there no disproportionate number of applicants with Christian names getting a slot offer. Mother age (or father, depending on reporting) can be recovered using the fact that the last two digits of a person ID in Malta correspond to the year of birth.

In order to understand how representative lottery participants are, **Table 2** reports descriptive statistics for the children population (3-5 years old) and for the pool of lottery applicants at baseline.[24] Comparing the first three columns, applicants do not seem to differ in terms of demographic characteristics from the student general student population in Malta. They are equally likely to born in the same quarter of birth, have the most common surnames or a typical biblical name. Given the spacial concentration of catholic schools in the island, it is reasonable to expect that these schools serve students who are disproportionately located in surrounding neighborhoods. Notably, there are no appreciable differences in the regional distribution of lottery applicants and the whole student population, indicating that those who applied may live anywhere in the Malta island. A closer look at the distribution of applicants per locality can be seen in Appendix **Figure A2**. This map shows the share of all children ages 3-5 in 2010 in each locality, who participate in the admission lotteries in the same year.

I formally test for the quality of the lotteries by regressing different predetermined variables on a dummy variable indicating whether the applicant won the lottery, where

[21] PIRLS assesses students in their fourth year of schooling, which corresponds to the fourth grade in most countries. In Malta, however, PIRLS is administered to students in their fifth grade. This is because the statutory school starting age is five and so Year 5 test better match the assessment to the achievement level of students. The sample comprised almost the whole population of 10-year-olds.

[22] Access to the restricted use items was subject to approval by the IEA Amsterdam.

[23] Traditional biblical names were identified using reports from the Population and Tourism Statistics Unit at the National Statistic Office that compile, each year, a ranking of popular names for babies registered at the Public Registry. Typical biblical names are Elena, Eliza, Catherine, Maria and Anna for girls. For boys the list includes Luke, Matthew, Jacob, Zachary, John and Isaac. The 2011 Population Census in Malta revealed that 25 percent of the Maltese population share 10 surnames: Borg, Camilleri, Vella, Farrugia, Zammit, Galea, Micallef, Grech, Attard, Spiteri and Azzopardi.

[24] Studies on Charter schools or Magnet programs show that these schools serve a targeted group of students which are, generally speaking, coming from more disadvantaged family backgrounds.

winning the lottery means that the student is offered a slot in a single-sex school. I include controls for lottery fixed effect (interaction of year and grade of application, SEN status and gender) in order to make use of the within-lottery randomness only. Point estimates and standard errors of this test are reported in the last column of **Table 2**. Additionally, F-statistics evaluating the joint hypothesis that all differences are jointly non significant are reported in the last row of the table.[25] Lottery winners and losers are similar on the range of predetermined observable characteristics. Note that differences are not only statistically insignificant but also substantively small in magnitude. Given the balance on observables that lotteries achieve, I can rule out concerns that lottery winners are losers are systematically different on unobservable dimensions, like parental involvement and motivation.

Typically, winners receive an offer of a slot the following days after the admission lottery is held. If an offer is declined, the slot opens up for subsequent candidates further down the waiting list. Actually, some applicants may be offered a slot close to (or at) the beginning of the school year if some students fail to show up. Accordingly, one may classify lottery winners as students that received an *initial offer* (immediately after the lottery day) or include also those that received offers made latter -*ever offer*.[26] All the results presented in the main sections of this study are based on the *initial offer* instrument because this is the information available for all the lotteries. Results based on the *ever offer* instrument will be shown in the appendix section.

3 Empirical Framework

3.1 Identification Strategy

I am interested in the effect of single-sex school graduation on student achievement at Year 6, the final year of primary school. If single-sex schooling is determined by random lottery numbers and we have full compliance with lottery outcomes, one could estimate the treatment effect by Ordinary Least Squares (OLS) using the following baseline formulation:

$$y_i = \beta_1 SS_i + \beta_2' R_i + \beta_3' X_i + \phi_{t(i)} + \epsilon_i, \tag{1}$$

[25]Balance regressions were also performed over the matched sample, for the single-sex offer and for the catholic offer. The results, reported in Appendix **Table B2**, are similar to those in Table 2. I also estimated covariate balance over students who got admission to single-sex schools through priority (i.e., whose lottery numbers had no impact on admission) and those entering single-sex schools because they were lotteried-in. If student admitted inside and outside of lotteries are different in ways related to student outcomes, the inferences of this study to that population will be limited. The results of this exercise, reported in Appendix **Table B3**, show that with a few exceptions, the differences are small and statistically insignificant.

[26]Although both classifications would yield valid instruments one can view a *ever offer* as inferior to an *initial offer* if some parents that were lottery losers initially quickly find alternative plans and reject a late offer while those not being able to find alternatives would accept the offer made latter on Hoxby and Rockoff (2005).

where y_i is the test score of student i and the treatment, denoted by SS_i, is an indicator equal to 1 if the student graduated from a single-sex primary school, and 0 if not. The inclusion of a set of dummies R_i, defined in detail below, are often referred to as *risk sets* (Abdulkadiroğlu et al., 2011) and indicate that the lottery allocation of applicants to schools is valid only after conditioning on a vector of individual characteristics. The vector X_i represents a set of applicants' baseline covariates that, although not necessary for identification, are included to increase precision. The term $\phi_{t(i)}$ are time fixed effects that capture shocks in the test year that may affect all students, while ϵ_i is the error term. The parameter of interest, β_1, indicates whether single-sex school graduates have systematically higher or lower test scores than coeducational school graduates. Alternatively, given the fact that students may have access to single-sex school at different intake grades, it is possible to model the effect of single-sex schooling on test scores as a function of the length of exposure to treatment. In such a case, the variable SS_i is defined as years spent in single-sex schools as of the test date and β_1 can be interpreted as the causal response to each year spent in a single-sex school. I report estimates based on both treatment specifications, but for simplicity I assume that treatment is binary in the rest of the section.

In practice, administrative assignment may fail to conform fully to a randomized design as some students may not comply with the treatment that the lottery assigns to them. For example, students who are offered a single-sex school slot may decide to deviate from assignment and attend their neighborhood state school. Analogously, students not offered a slot may manage to get admission by the discretion of the principals or gaining sibling priority when a sibling wins a lottery. This would potentially cause the treatment and the error term to be correlated, and thus OLS estimates of β_1 in equation (1) would be biased. I therefore use a two-stage least square (2SLS) strategy for estimating the effect of graduating at a single-sex school using lottery outcomes as instruments. Identification of causal effects rests upon a conditional independence assumption, stating that a vector of lottery offers Z_i is excluded from the causal equation once we condition on the full set of risk sets, and an exclusion restriction, asserting that winning a lottery only affects test scores through single-sex schooling and nothing else. The approach so far follows the standard lottery-based estimator. However, we need to depart from the pure lottery instrument to accommodate reapplications and parental preferences.

Parents of kids at traditional intake grades in Malta seems to be persistent in attempting to get a place at a single-sex school more than once. Actually, around 70 percent of rejected first-time applicants reapply a second time. The fact that students may participate in subsequent centralized lotteries make identification less straightforward as application patterns may be correlated with potential outcomes. For example, an applicant may lost the first time she applies in kindergarten, reapply the following year to Y1 admission lottery and get a slot offer in that instance. There are sound reasons to assume that parental characteristics of this child may differ from those that only apply to kindergarten lottery. Note that the probability of receiving an offer for a single-sex school is also higher for the first type of applicants, introducing an obvious endogeneity concern. However, while it is not random in which application pattern a student is, the lottery outcome is exogenous conditional on that lottery strata.[27] Thus, controlling for application pattern is necessary for the validity of lottery outcomes as instruments (Dale and Krueger, 2002).[28]

The first-stage equation of the problem at hand has the following general formulation:

$$SS_i = \alpha_1' Z_i + \alpha_2' R_i + \alpha_3' X_i + \phi_{t(i)} + \eta_i, \tag{2}$$

where Z_i is a set of instruments based on the outcomes of J lotteries in which the student participated and R_i are the risk sets. I specifically consider:

$$\alpha_1' Z_i = \sum_{j=1}^{J} \alpha_{1j} Z_{ij}, \tag{2.1}$$

$$\alpha_2' R_i = \sum_{j=1}^{J} \alpha_{2j} L_{ij}. \tag{2.2}$$

The expression in (2.1) is the instrumental variability spanned by the vector of J possible lottery offers, with each dummy Z_{ij} being an indicator of receiving an offer of a slot at any single-sex school in lottery j. Recall that lotteries are run separately for SEN status and gender so the subindex j denotes the interaction between the grade and year in which a student apply, the special needs status and the gender variable. Equation (2.1) accounts for the fact that the probability of graduation at a single-sex school depends on a bundle of lotteries in which the individual participated rather than a single lottery. This equation is general enough to embed a number of specifications which can be defined by imposing alternative restrictions on the parameters α_{1j}. For example, we can focus on the number of offers received or having at least one offer of a single-sex slot, etc.

The parameter α_1 is the effect of the lottery offers on the probability of single-sex school graduation, and the instrumental variable estimate, defined by equations (1) and (2), is the local average treatment effect (LATE) on student achievement for those who comply with the lottery assignment (i.e., school graduation was determined by the lottery outcome) (Angrist, Imbens, and Rubin, 1996).

The expression in (2.2) defines the risk set controls. In their simplest specification, L_{ij}

[27] I empirically investigate the presence of observable differences in parental characteristics across applicants with different application patterns. In Appendix **Figure A3** I report these estimates. Although I don't find strong differences, I remain agnostic about the possibility that unobserved characteristics can be significantly different and report results with students' application pattern adjustment.

[28] Studies in general do not deal with reapplications or use the first lottery an individual participates for the estimation of the treatment effect (Angrist et al., 2010; Ketel et al., 2016). When looking at first time applicants, the group of compliers includes only those who got treatment by winning the first lottery. Given that each lottery a student participates is a valid instrument for single-sex schooling, I leverage here all the lotteries each cohort may participate. Results using first time application result are shown in Appendix **Table B4**.

is a dummy variable that takes value 1 if individual i participates in lottery j. Inclusion of these controls follows from the fact that school assignment is random within lotteries but not necessarily between them (i.e., a student's decision to participate in a certain lottery is non-random). Note that the *risk sets* can be defined in a more general way by grouping students depending on the "bundle" of lottery applications, i.e., the application pattern.[29]

Like reapplication, preferences over schools are also far from being randomly assigned. Wining-losing probabilities are not independent of the revealed preferences and one would like to compare parents who submitted a similar ranking of preferred schools. In practice, there are different ways of leveraging lottery assignments independent of potential outcomes. One may focus on offers at student's first-choice school (Abdulkadiroğlu, Hu, and Pathak, 2013; Deming, 2011; Deming et al., 2014; Hastings, Kane, and Staiger, 2009) or condition on the full set of schools ranked (Pop-Eleches and Urquiola, 2013; Lucas and Mbiti, 2014). Note that the last case is only possible when the number of observations is large relative to the number of schools. However, when the number of schools is large, there is an obvious dimensionality constraint given by the extreme large number of school-rank combinations one may find in the data. To deal with this issue, one can impose ad hoc solutions of the type conditioning to the three top choices or, alternatively, control for the simulated conditional probability of getting admission (referred as propensity score in the rest of the paper) as proposed by Abdulkadiroğlu et al. (2017).[30] This method has two advantages, on the one hand it reduces the dimensionality problem and, on the other hand, it maximizes the number of individuals that contributes to the estimation of the average treatment effect. Intuitively, the propensity score can be calculated by drawing lottery numbers and running the allocation mechanism many times (while keeping constant applicants preferences and school priorities and slots), and computing the resulting average assignment rates. I follow their analytic formula to generate for each applicant a probability of receiving an offer from a single-sex school.

In order to account for preferences, I refine the risk set controls in the following way:

$$\alpha_2' R_i = \sum_{s=1}^{S} \sum_{j=1}^{J} \alpha_{2sj} L_{ij} \times P_{ijs}, \tag{2.3}$$

where s indexes schools (or at a more aggregate level, the school type) and P_{ijs} is a dummy variable taking value 1 if school s is the first choice in the rank-order list of schools of applicant i in lottery j. Alternatively, P_{ijs} can be replaced by an estimation of the propensity score for single-sex school as explained above.

[29] In such a case, equation (2.2) can be redefined as a full set of lottery fixed effect interactions: $\times_{j=1}^{J} \alpha_{2j} L_{ij}$, which accounts for the specific set of lotteries in which the students participated.

[30] Abdulkadiroğlu et al. (2017) evaluate the efficiency gains obtained by controlling for the propensity score relative to, for example, schools ranked first using Denver centralized school assignment. Moreover, they show that this is applicable to any centralized mechanisms satisfying the "Equal Treatment of Equals" (ETE) property.

3.2 Disentangling the Effect of Same-gender Peers

Note that the instrumental variable estimate, defined by equations (1) and (2), contrasts single-sex schooling with a mix of coeducational schools from the State and the Catholic sectors. Although this estimate provides relevant information for policy analysis and can be a relevant criterion for parents, β_1 may not necessarily reflects the effect of having same-gender versus mixed-gender peers. The main confounder behind that interpretation is the fact that the estimation also bundles the effect of attending a catholic school.

There are a number of dimensions in which catholic schools differ from the state-run coed schools. The former are privately managed, have more discretion to select teachers, school text and pedagogical approaches, and may extend activities beyond the core curriculum. In addition, due to the fact that there is one state primary school per locality, the student population at some of those schools may be relatively low, potentially leading to greater individualized instruction, while catholic schools operate under full capacity.[31,32] On the contrary, for historical and institutional reasons, the Catholic sector comprises a more homogeneous set of schools. Although I can not rule out the hypothesis that catholic coed and single-sex schools differ in unobservable dimensions, I can test for significant differences in a set of observed characteristics. I investigate this in **Table 3** which reports summary statistics for single-sex and coed schools within the catholic sector. Panel A presents the average characteristics of the school staff; Panel B for the teachers only; while Panel C shows statistics for school location (relative to the distance to the capital city Valletta), instruction time, and school and class sizes. Statistics are reported separately for the pool of single-sex schools (column 2) and disaggregated for girls-only and boys-only schools (columns 3-4, respectively). Columns 5-8 shows that differences between coed and single-sex schools are small and non-significant at 95% confidence. For example, as teaching at primary school level is predominantly a female profession, the typical teacher at any of these school types is female. This rules out the student-teacher gender (mis)match effect widely explored in the educational literature.[33] Also, other teaching staff characteristics, the instruction time allocated to the three main subjects (English, Maltese and Math) and the class sizes are strongly balanced across school types. Single-sex and coeducational catholic schools do not seems to differ in terms of location neither which may trigger a differential access to resources. [SEGUIR DESCRIBIENDO LAS TABLAS]

The evidence in **Table 3** that single-sex and coed catholic schools are balanced in observable characteristics is consistent with the idea that, leveraging on random

[31]The average class size at state primary schools is 17 students, well below the established regulatory threshold of 30.

[32]Surprisingly, the religious instruction seems not to be the overriding factor that differentiate these two school sectors. Roman Catholic religion is taught in all state-run schools and for the same amount of hours per week as in the Catholic schools. Parents do have the right to opt-out but very few choose to do it (less than 2% during the 2008-2009 scholar year and around 6% during 2015-2016).

[33]See Bettinger and Long (2005); Dee (2007); Antecol, Eren, and Ozbeklik (2015) and Lim and Meer (2017).

allocation of applicants, differences in student performances are mainly attributable to the same-gender composition of peers rather than other characteristics of the school. I therefore estimate the following equation:

$$y_i = \delta_1 SS_i + \delta_2 CATHOLIC_i + \delta_3' R_i + \delta_4' X_i + \phi_{t(i)} + \varepsilon_i, \tag{3}$$

where $CATHOLIC_i$ is a dummy variable taking one if the student graduated from a catholic school (single sex or coed). Variable SS_i and $CATHOLIC_i$ are instrumented with lottery outcomes as discussed above in this section.[34] In this specification, we can interpret δ_1 as the causal effect of graduating from a single-sex school conditional on attending catholic education (or the impact of moving a student from a coed catholic classroom with equal share of girls and boys students to a single-sex school). Similarly, δ_2 is interpreted as the causal effect of graduating from a coed catholic school relative to a coed state schools (recall that all state schools are coeducational at elementary level).

4 Lottery-based Estimates

To ease exposition, I present results using the stacked version of the dataset (so each student has one observation for each lottery she participated). Main results are also shown for different definition of risk set controls. Panel A controls for the whole student's application pattern, which is the full set of interactions of lottery fixed effects. Lottery fixed effects are generated by the interaction of four variables indicating the grade and year of application, SEN status and gender. Panels B to D account for parental preferences in addition to the lottery participation, this corresponds to different definitions of P_{ijs} in (2.3). Panel B incorporates the first-choice (most preferred school) in the risk set by interacting the preferred school identity with the student application pattern. In panels C, the full application pattern is interacted with a dummy variable identifying if the first-choice is a single-sex school. The last panel, Panel D, incorporates the propensity score, which shape the probability of assignment to single-sex schools taking all features of student preferences and school priorities into account. Point estimates are sensible to these definitions, which is reasonable as selection is not accounted for in the same way across risk sets. Due to data availability before 2010 and changes in the application process after 2011, preferences are observed for a subset of lotteries. For this reason, Panels B to D leverage offer variation only from the set of lotteries where preferences are observed and may differ from results shown in the first panel.[35]

[34] Note that receiving an offer attend to a catholic schools is also exogenous after conditioning for lottery participation patterns and preferences, therefore, it is straightforward to extend the 2SLS strategy to this setting.

[35] As each lottery is exogenous conditional on controlling for the corresponding risk set, using a subset of lotteries remains valid for identification.

Single-sex Schooling I first examine outcomes estimated by equations (1) and (2) above. **Table 4** presents estimates on the effect of winning the lottery on single-sex school graduation. Each cell of the table corresponds to a separate regression, with columns referring to alternative risk set controls. First stage results show that, on average, applicants who received a single-sex offer are about 50 percentage points more likely to graduate from a single-sex elementary schools than those who were not offered a single-sex spot. With perfect compliance of the treatment and no loss to follow-up, the first stage should be equal to one. In practice, however, some lottery winners never attend single-sex schools while some lottery losers end up getting access through waiting list, principal discretion or gaining sibling priority.

Table 5 displays reduced-form and 2SLS estimates corresponding to the first-stage estimates in **Table 4**. Columns 1-3 report estimates for English, columns 4-6 for Maltese while columns 7-9 for Math scores. On average, receiving a single-sex slot offer boosted language scores but there is no significant effect on math at standard levels. Columns labeled "Reduced Form" reports these results. Estimates do vary depending on the risk sets definition, though the general pattern is that improvements in test score are between 0.10 to 0.15 standard deviations (hereafter, σ) for English and between 0.07σ-0.010σ for Maltese. The point estimates for Math are consistently below of those for the language subjects (of about 0.04σ on average) and none of them is statistically significant.

Without demographic controls, the two-stage least squares (2SLS) estimates in Panel A are about 0.20σ for English and 0.15σ for Maltese. These results are reported in the middle column labeled "2SLS". When the risk set definition includes parent preferences of some form, the effects are higher. For example, take Panel D, which shows results when comparing applicants with the same lottery pattern and propensity score. The estimated effects are 0.30σ and 0.20σ for English and Maltese, respectively. The addition of controls for demographic characteristics (quarter of birth and locality fixed effects plus indicator of having a biblical name) reduces the point estimates a bit although the general pattern described so far is reasonably robust. Note the results in columns (3), (6) and (9) comes from a very demanding specification of the data that includes student's locality fixed effects.

In sum, results show that there is a real gain on English and Maltese of single-sex schooling that can not be attributed to student selection.

Same-gender Peers While estimates shown so far causally identify academic gains from single-sex schooling, this may not necessarily reflect the effect of having same-gender peers alone. Many factors may confound the relationship between same-sex peers and achievement. For example, single-sex schools may attract better teacher or put more emphasis in the core curriculum than state coed schools. If any of these factors affect test scores, one is at risk of attributing the positive estimated effects to the gender composition of classrooms while this may merely be due to single-sex schools being better schools per se.

In order to test is same-gender channel, I consider results from equation (3). The idea is to remove the effect of attending a catholic school from that of attending a single-sex school, relying on the fact that most observable characteristics among single-sex and coed schools within the catholic sector are strongly balanced as shown in **Table 3**. This evidence allows me to rule out that the effect of single-sex education is driven by other key inputs in the education production function (like differences in teachers' gender and experience, class size, different curriculum design) and, at the same time, reduces the probability that unobservable characteristics of the single-sex schools, other than the gender-peer composition, are the main mediators of the effect. Naturally, the validity of this assumption can not be directly tested without random assignment of the single-sex status across schools (keeping constant any other school characteristic), so it remains a suggestive result in this paper.

Results of this specification are shown in **Table 6**. Interestingly, for all measures of school performance including math, the effect of attending a single-sex school, above and beyond the effect of catholic school, is positive and significant. Intention to treat effects are reported in columns labeled "Reduced Form" while the rest of the columns show two-stage least square estimates, with and without baseline demographic controls. Based on the results shown in panels A, applicants who won a single-sex lottery scored around 0.26σ higher on English and 0.24σ higher in Maltese. The reduced-form Math results is about 0.15σ and statistically significant. The 2SLS estimates of the effect of graduating from a single-sex school show gains on the order of 0.33σ for English, 0.30σ for Maltese and 0.19σ for Math. The addition of controls for baseline demographic characteristics does little to these estimates. The estimated effects when including preference controls (panels B to D) are two to three times bigger than those described for the first panel.

These results also reveal that students that received a coed offer performed worse in all of the core subjects. The point estimates for *Catholic* in the reduced-form are all consistently negative (and significant in most of the cases). The 2SLS estimates tells the same story, implying that coed graduates at catholic schools perform worse than those at state schools. This is a puzzling result and hard to reconcile with the fact that catholic schools are highly demanded every year. One possible explanation could be related to the smaller and more manageable class sizes at state schools. So, same-gender are better than mixed-gender classrooms but the difference is mediated by the class size, being less pronounced if the size of the class is smaller. Another possible explanation could be that parents with a lucky ticket for a catholic place relax after securing a slot, while parents of applicants that did not manage to get any highly sought-after seat compensate by putting more effort (i.e., parental involvement and school quality are substitute in the education production function). This aforementioned hypothesis has been suggested by Cullen, Jacob, and Levitt (2006) when evaluating Chicago Public Schools and finding that winning a lottery to a high-valued added school does not produce any test score gains.

5 Response Heterogeneity

Prior work on high school students had found that single-sex schooling has positive effect on the academic achievement of girls, while results are more mixed for boys. I assess in this section whether the findings presented so far are driven by female students attending girls-only schools. I also explore treatment effect heterogeneity along two other pre-determined dimensions: preferences over single-sex schools and quality of the outside option (i.e neighborhood school). One important advantage of the maltese assignment mechanism is that it is strategy-proof, so applicants' rank-order list of schools measure their true preferences. Assuming that parents choose schools based on their private benefits, the treatment effect for students with strong preferences for single-sex schools may be much larger that those that place a low weight on having same-gender environments. On the other hand, the fact that lottery participants seem to come from anywhere in the maltese island, motivates an analysis of response heterogeneity based on the quality of the alternative option, the neighborhood school.

Gender In contrast to the substantially more favorable impact of same-gender peers for girls, I find similar impacts for girls and boys attending single-sex schools. **Figure 7** shows 2SLS estimates for each test score controlling for application pattern and baseline demographic characteristics. Girls at girls-only schools score about 0.40 standard deviations in English more than students as coed catholic schools. The point estimates for boys at boys-only schools is lower but substantial, at 0.28 standard deviations. This differences by gender revert for Maltese subject, being more favorable for boys than for girls at single-sex schools (0.37 versus 0.29 standard deviations, respectively). The estimated effects for Math are not significant at standard levels. Findings on language subjects are not sensible to the risk set specification (shown in Appendix **Table B5**). On Math test score, there is much less clear pattern and ones I include controls for preferences (of any type), it seems that boys benefits more than girls of single-sex schooling, though both effects are high.

Weight on Single-sex When submitting the application, parents need to rank schools in order of preference from their first to their last choice. They do not have limitations in the number of schools they can rank and, on average, they rank 60 percent of the schools in their choice set (see Subsection 2.1). Multiple-ranked responses are superior to single (first) choice because in the later case it is difficult to disentangle whether the stated choice is the result of a strong preference for some particular aspects of the choice or due to an unusual error term. On the contrary, with rank-order lists one can gather more information because the same individual provides multiple outcomes by removing the prior chosen school from the subsequent choice set. If parents systematically choose schools who share a common attribute, one can infer that there is a strong preference for that attribute.[36]

[36]Empirical studies on students' heterogeneous preferences over school attributes based on a demand

I use a mixed rank-ordered logit model on parents ranking of schools data to infer the intensity of preferences over schools' attributes (McFadden and Train, 2000).[37] This method allows me to estimate, for each applicant i, the weight ($\hat{\theta}_i$) their parents place on the single-sex attribute when choosing schools. Appendix **Figure A4** summarizes the distribution of parents over the estimated parameter. Almost all parents have a positive value, though the figure reveals significant heterogeneity. The fact that this parameter depends only on baseline data and is independent of the lottery outcome, motivates the following second stage equation:

$$y_i = \gamma_1 SS_i + \gamma_2 (SS_i \hat{\theta}_i) + \gamma_3 CATHOLIC_i + \gamma_4' R_i + \gamma_5' X_i + \phi_{t(i)} + \mu_i. \tag{4}$$

The coefficient γ_2 indicates whether effects are larger or smaller as the weight over single-sex attribute increases. Note that the corresponding first-stage equations add the interaction between $\hat{\theta}_i$ and the instrument for identification. I add controls for the weight parameter, year of test dummy and lottery fixed effects.

Results of this test are shown in **Figure 9**. Although effect are imprecisely estimated they are informative of the heterogeneous treatment effect by preference for single-sex schooling. As expected, effects are weakly increasing with $\hat{\theta}_i$, specially for the language subjects. However, it is worth noting that the highest quartile seems not to drive the results in any of the test scores. Variations in the definition of the risk set controls provide identical results.

Outside option For this test, I infer the quality of each applicant neighborhood school (state-run) using the fixed effect model as the one to construct **Figure 2**. I proceed as before but changing the single-sex school preference parameter for a new parameter that captures the quality of the outside option (neighborhood school) for each applicant. Results are shown in **Figure 10** and, as before, parameter values were categorized in quartiles. In this case the pattern is much lest clear. All point estimates are positive across subjects, but it does not seem that students that have better outside options benefit more from having same-gender peers than those with worse neighborhood options.

system approach can be found in Hastings, Kane, and Staiger (2009) and Abdulkadiroğlu, Agarwal, and Pathak (2017).

[37]This mixed logit can approximate a random utility model of parental choices of the form: Assume $U_{is} = \beta_i' X_{is} + \epsilon_{is}$ is the expected utility for student i of attending school s, and that X_{is} represents a vector of attributes describing s (share of teachers with a bachelor degree, average wage, average teacher tenure, school proximity, being single-sex or a coed school). If parents choose among all possible schools in the choice set S_i based on whether it delivers the highest utility, then he probability of choosing school s in the first choice is given by $Prob(c_i^1 = s) = Prob(U_{is} - U_{ik} > 0) \, \forall k \neq s$. It is assumed that the following choices are made in a similar manner, except for the fact that the choice set excludes schools already chosen, until a preference order is obtained over all schools. Given this assumptions, the probability of parent i having a particular ranking of alternative schools is modeled by a rank-ordered logit as the product of of best choices. For example, for the case of three alternative schools we have $Pr_i(ranking\ s_a, s_b, s_c) = Pr(s_a\, 1st\, best) * Pr(s_b\, 2nd\, best)$. Parameters were estimated by maximum likelihood estimation (MLE) following Lancsar, Fiebig, and Hole (2017).

6 The Lasting Effects of Primary Single-Sex Schooling on Curriculum Track

It is a well established fact that there is a substantial gender difference in major choices (OECD, 2016). Women are typically underrepresented in science and math-intensive fields while men are underrepresented in health and humanities majors. University student choices are a reflection of what happens in secondary school when students choose the curriculum track (Xuereb, 2001). Thus, despite a recent increase in the intake of math courses by female students, a gendered course-taking pattern is still persistent (Bank, 2011).

A number of studies have looked at the effect of single-sex high schools on this matter (Jackson, 2012; Park, Behrman, and Choi, 2018). In theory, single-sex schools should reduce the gap in the gendered course taking, especially for girls, because the lack of competition with boys for the teacher's attention and the less gender-salient environment (Riordan, 1990). But beside the evidence that students at single sex high schools show higher confidence levels of their academic skills on gender-atypical subjects (academic self-concept) than their counterpart in coeducational schools (Sax and Kevin M., 2011; Sullivan, 2009), there is no evidence that female students take more science courses (Jackson, 2012) in high school or that they are more likely to join a university with a STEM college major (Park, Behrman, and Choi, 2018).

One possible explanation for the results describe above is that interventions at adolescent age, such as single-sex school environment, come too late. Gender stereotypes are internalized early in life through teachers and parents ("The Role of Parents and Teachers in the Development of Gender-Related Math Attitudes"; Alan, Ertac, and Mumcu, 2018; Carlana, 2019) and traditional norms and beliefs about gender maybe should be challenged before they get too entrenched.

In this section, I address the question of whether the uptake of subjects at high schools differs according to the type of primary schools attended. In Malta, students choose the curriculum track at the age of thirteen, which covers the last 3 years of secondary education. Besides a foreign language, they have to choose two elective subjects among a set of academic and vocational courses that supplement the national core curriculum. Elective subjects cover a wide range of fields from home economics or hospitality, which are prevalently female courses, to chemistry or physical education, which are prevalently male courses. In practice, this curriculum track largely affects the afterwards choice of college major (Calleja, 2008).[38]

I collected individual administrative data on the choice of elective subjects at Catholic secondary schools and linked students with their lottery participation for primary schools. A number of characteristics of the catholic high-school education guarantee that students

[38] For instance, admission to medicine school requires A-levels in biology or chemistry, and students are strongly encouraged to take these elective courses if they aim to enroll in any medicine-related fields at university.

face a similar decision among the set of elective subjects independently of whether they took single-sex or coed primary education. First, all schools offer the same three science courses (physics, chemistry and biology). Second, it is possible to observe in all cases a wide number of topics that span very similar fields. Finally, conditional on gender, the available subjects are quite similar irrespective of the type of primary school education. Appendix C describes in more details this setting and the transition from primary to secondary school within the catholic sector.

The effect of single-sex primary on curriculum track is documented in **Table 7**. Specifically, the table shows 2SLS estimates of the effects of single-sex school on the probability of taking at least one science subject (column 1); the probability that both subject options taken are science (columns 2); the probability of taking at least one prevalently female subject (column 3); and the probability of taking at least one vocational subject (column 4). These estimates follow the same lottery specification as presented in equation X, fully interacted with the student gender.[39] Results reveal, for example, that girls that attended a girls-only school are 17% more likely to choose two science subjects compared to girls coming from coeducation catholic schools. Similar point estimates for girls, but on the opposite direction, applies for the probability of taking at least one prevalently female subject, though imprecisely estimated. For boys, the only statistically significant effect is on the probability of choosing at least one vocational subject (column 4). The effect is negative indicating that boys from boys-only primary schools are 15% less likely to choose at least one vocational subject compared to their counterpart coming from coeducational catholic schools.[40]

7 Mechanisms of Single-Sex Schools

The estimated results show a substantial impact of single-sex education on the academic achievement of boys and girls. In this section, I use student and teacher survey data to establish the mechanisms that led to such effects. The educational literature points out that the single-sex school effect operates through direct and indirect channels. The first one refers to a gender peer interaction effect. The indirect channels refer to the gender alignment in the teachers pedagogical practices, discipline methods, as well as the ability-level to which the class is pitched (Jackson, 2019). The survey data do not allow me to fully explore and measure all the mediating factors behind the positive effects of single-sex education, it provides, however, important insights.

Teachers Survey I evaluate teacher responses to school type using PIRLS teacher

[39]In practice, the risk set includes the lottery and application pattern fixed effects. It does not control for preferences because these were recovered for a subset of lotteries, while the data collected at catholic secondary schools covers more applicants for many different lotteries. This omission is of little empirical consequence as was shown before.

[40]OLS estimates of this specification are available in Appendix **Table C2**.

questionnaires. Although very wide on the topic being investigated, the survey only covers Year 5 teachers in 2016 and not the whole school staff, so the results of this analysis should be taken with some caution as small sample sizes may lead to rather imprecise estimates. See **Appendix D** for a detailed description of the survey questions used to construct the outcomes examined in this section. All measures are standardized to have mean zero and standard deviation of one. The estimates presented here are based on the main specification, so all comparisons are made among teachers in Catholic schools. I split the single-sex school variable into two dummy variables, a girls-only and a boys-only school dummy, so estimates are deviations from teachers' answers at coeducational school with an equal share of male and female students.[41]

I first analyze answers to questions about teacher satisfaction and the quality of teacher-student relations. I combined 5 items to measure teacher satisfaction ("I am content with my profession as a teacher", "I find my work full of meaning and purpose", "I am enthusiastic about my job", "My work inspires me" and "I am proud of the work I do"), while the quality of the relationship is based on the answers to the item "The students are respectful of the teachers". Results are shown in Panel A of **Table 8**. The first column report estimates of the effect of girls-only school on teacher responses while the second column reports estimates of boys-only school. While there seem to be no differences in the level of teacher satisfaction with the teaching profession among teacher in single-sex and coeducational catholic schools (first row), the effect on the quality of the relationship between students and teachers are positive and significant for teachers at girls-only schools. The estimates for boys-only schools are also positive, but not statistically different from zero.

Next, I explore classroom disruption and school discipline. The first dimension is captured by the teachers report about students' general behavior ("The students behave in an orderly manner",) or if they report that disruptive students limits the teaching. School discipline is measured by two dummy variables related to the teacher agreement with the following statements: "This school has clear rules about student conduct" and "This school's rules are enforced in a fair and consistent manner". Panel B of **Table 8** shows the results for these dimensions. Consistent with the hypothesis that boys are more disruptive than girls (Lavy and Schlosser, 2011), teachers in girls-only schools are less likely to report that students misbehave while the opposite is true for teachers at boys-only schools, though imprecisely estimated. Again, this is in comparison to teachers at coeducational schools where gender-typical characteristics are mixed.[42] The survey does not allow to differentiate

[41]If we restrict the sample to teachers working at schools that do participate in the Benchmark examination, the estimates produce similar results as the ones describe here (Appendix **Table D1**).

[42]School principals were also surveyed about the quality of the teacher-student relationship, classroom environment and school emphasis on academic success. Estimates using the principals sample confirm the finding described in this section using the teacher survey. There is, however, more detailed questions related to the quality of inter-student relationships. Principal in single-sex schools are more likely to report that intimidation or verbal abuse or physical fights among students is a problem in their school, specially at boys-only schools. Estimates are not statistically significant and overall, the level of reported classroom

if there are different discipline methods across schools, but teachers at single-sex schools are more likely to report that the school employ and enforce clear rules of conduct. This suggestive evidence goes in line with the results of Lee et al. (2014), which report that teachers in boys-only schools employ stricter discipline and teaching methods.

In Panel C of **Table 8** I explore the survey questions related to the teacher instructional practices. While teachers at girls-only schools are more likely to report that they use individualized methods of teaching compared to their counterpart at coed schools, teachers a boys-only schools are less likely to do so, statistically significant at the 10 percent level. Interestingly, in both single-sex school types, teacher reporting of alignment of pedagogical practices (engaging students' interests, use multiple perspectives or link content to prior knowledge) is lower compared to teachers in coed schools. Results for the reported use of ability grouping go in the same direction, negative and not statistically significant for both single-sex school types. However, instructional practices in both girls-only and boys-only schools seem to be more teacher-guided, as teachers are less likely to leave students to work independently on an assigned plan or goal.

Complementary to school environment and teachers' instructional practices is that of the effect of single-sex school on the school emphasis on academic success. These estimates are reported in Panel D. Curriculum expertise is a index derived from how teachers characterized understandings of the school's curricular goals and their degree of success in implementing the school's curriculum. Teachers at girls-only schools reported significantly lower curriculum expertise while there is no such evidence for teachers at boys-only schools. Teachers were also surveyed about the students' desire to do well in school and their ability to reach academic goals. Estimates are small and insignificant indicating that teachers' perception of students' performance are comparable across school types.

Students Survey I am able to match students surveyed in PIRLS with the lottery data.

8 Conclusion

This paper provides evidence that boys and girls benefited from single-sex education. Despite the importance of this issue for the educational policy, it has proven difficult to researchers to inform the debate due to the challenge of dealing with student sorting into school types and with unobserved potentially confounder factors of the schools that decide to serve students of the same gender or of a mix of genders. This study provides rigorous evidence of the impact of single-sex education by leveraging admission lotteries and, to my knowledge, the first one to address it at elementary level.

The point estimates suggest that average score of a student at a single-sex school is about 0.35 and 0.20 standard deviations higher than that of a student at a coeducational

disturbance is lower that that of coed catholic schools. See Appendix **Table D2**.

school for the language and math subjects, respectively. The results are not confounded by differences in the teachers' quality, instruction time or class sizes, which are equally distributed across school types within the Catholic sector. Moreover, I find that single-sex education has a positive and significant effect on academic achievement of girls and of boys. The sizes of the estimates effects are similar for both genders.

There are two main limitations in this paper. One is the lack of evidence on the mechanism behind the single-sex effect. We are not sure whether the findings are the result of a direct gender-peer effect, a change in teacher behavior while instructing or the combination of both.

Last, while the results suggest that gender segregation improves students test scores, it is also possible that it may affect, in a negative way, the development of some noncognitive skills. Although there is no empirical evidence on this fact, and it is out to the scope of this paper to provide evidence on that hypothesis, further evidence on this matter may have important implications when considering the cost and benefits of single-sex learning environments.

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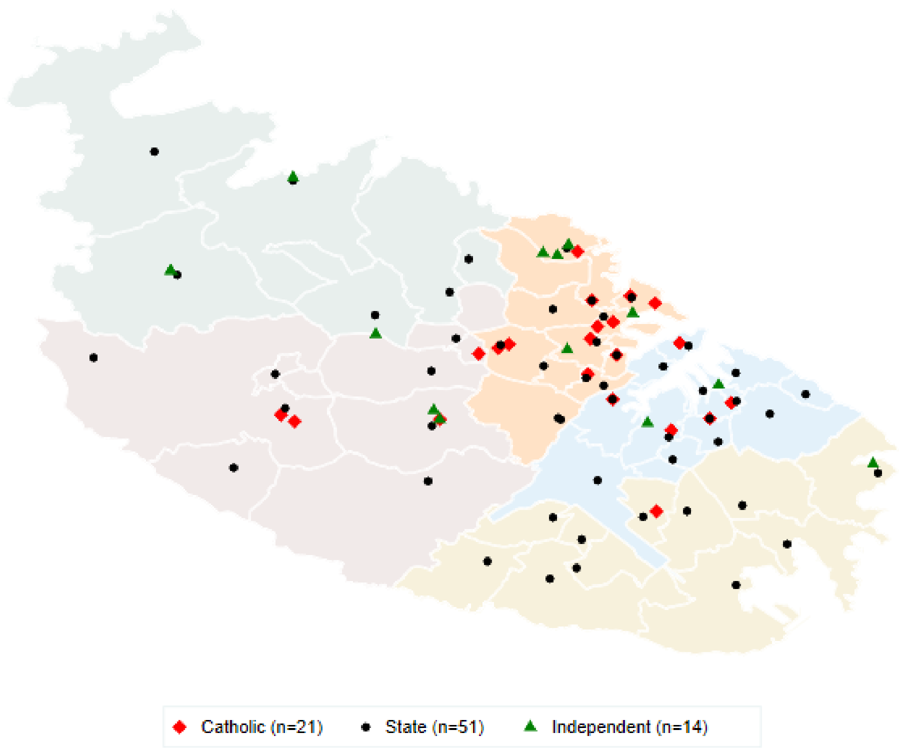
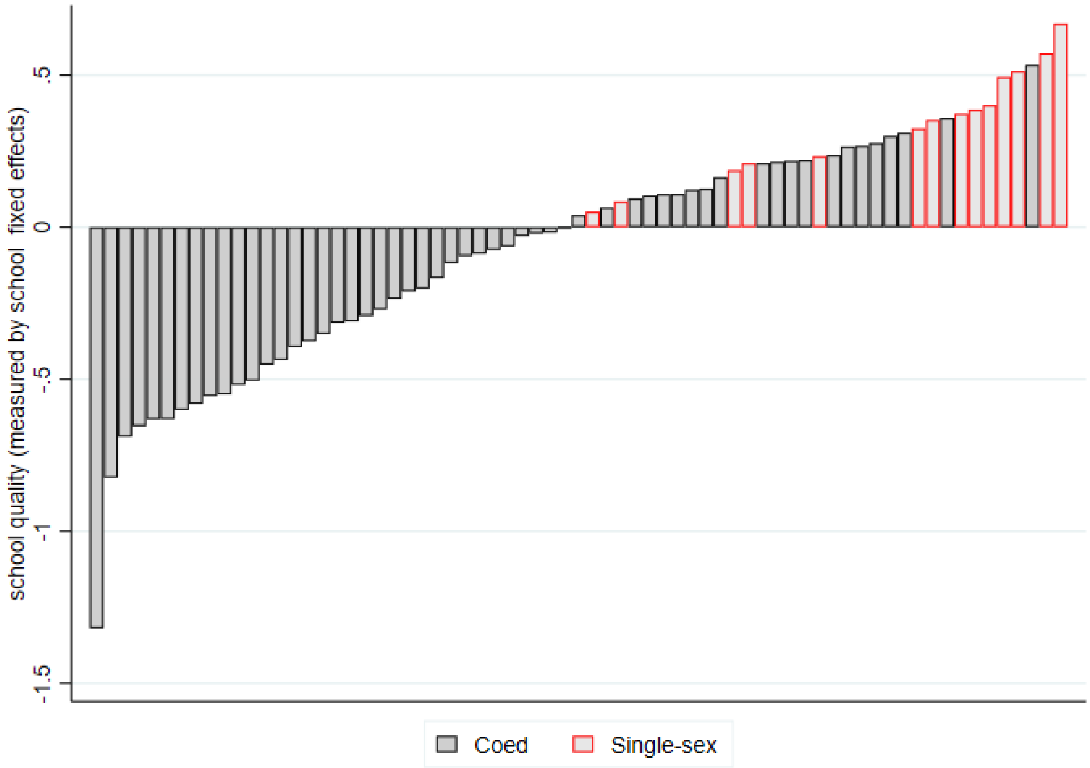
Figure 1. Location of Primary Schools in Malta



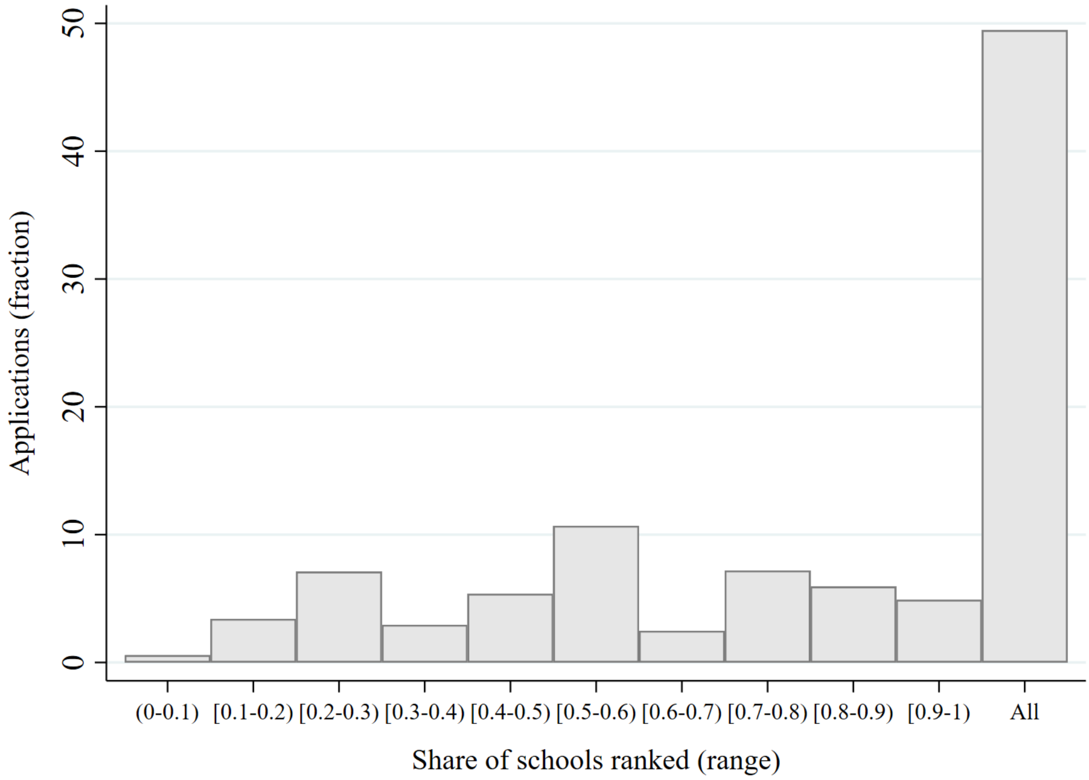
Notes: Malta island is subdivided in 5 districts and 54 localities. The upper three districts, from left to right, are the Northern District, the Western District and the Northern Harbor District that together form the North Western Region. The other two districts, the South Eastern District and the Southern Harbor District form the South Eastern Region. The total number of schools per sector are reported in parentheses.

Figure 2. School Quality



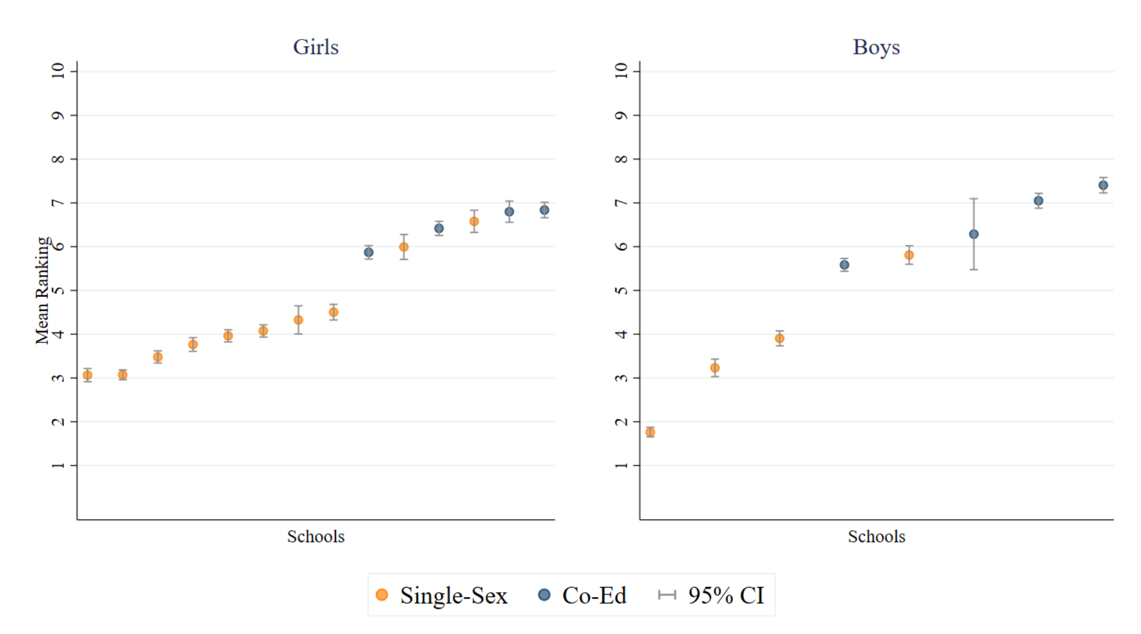
Notes: Each bar represents a school. The y-axis shows estimates results from a OLS regression of students composite score at Year 6 (end of primary school) on the full set of year and school fixed effect dummies. The composite score is the average of the scores in English, Maltese and Math and was standardized to have mean zero and unit variance by year. Sample includes all test-takers from state and catholic schools. Source: End of Primary Benchmark 2014-2017.

Figure 3. Applicants Distribution According to the Share of School Ranked



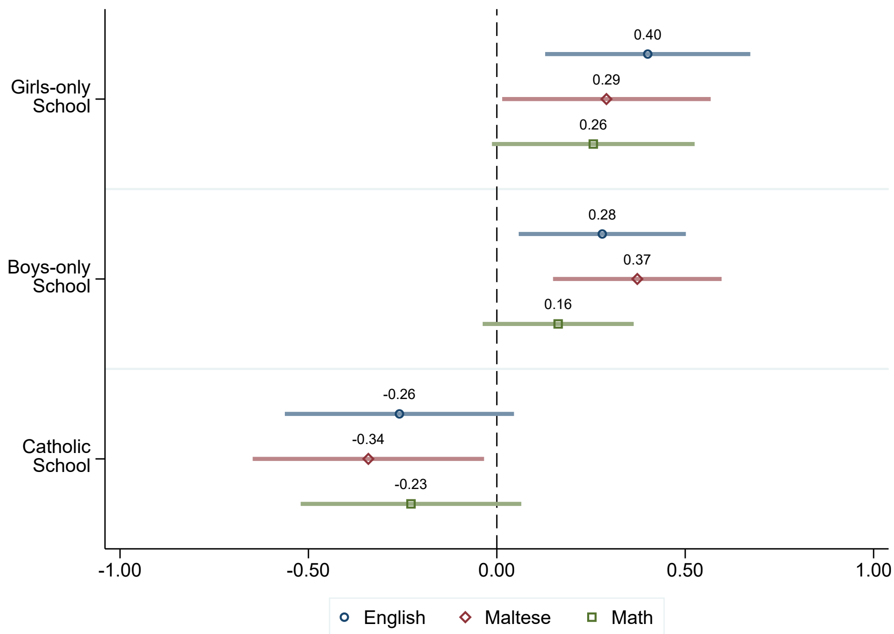
Notes: Each bar represents the fraction of applicants whose share of school ranked falls within the specified range.

Figure 4. Catholic Schools Ranking



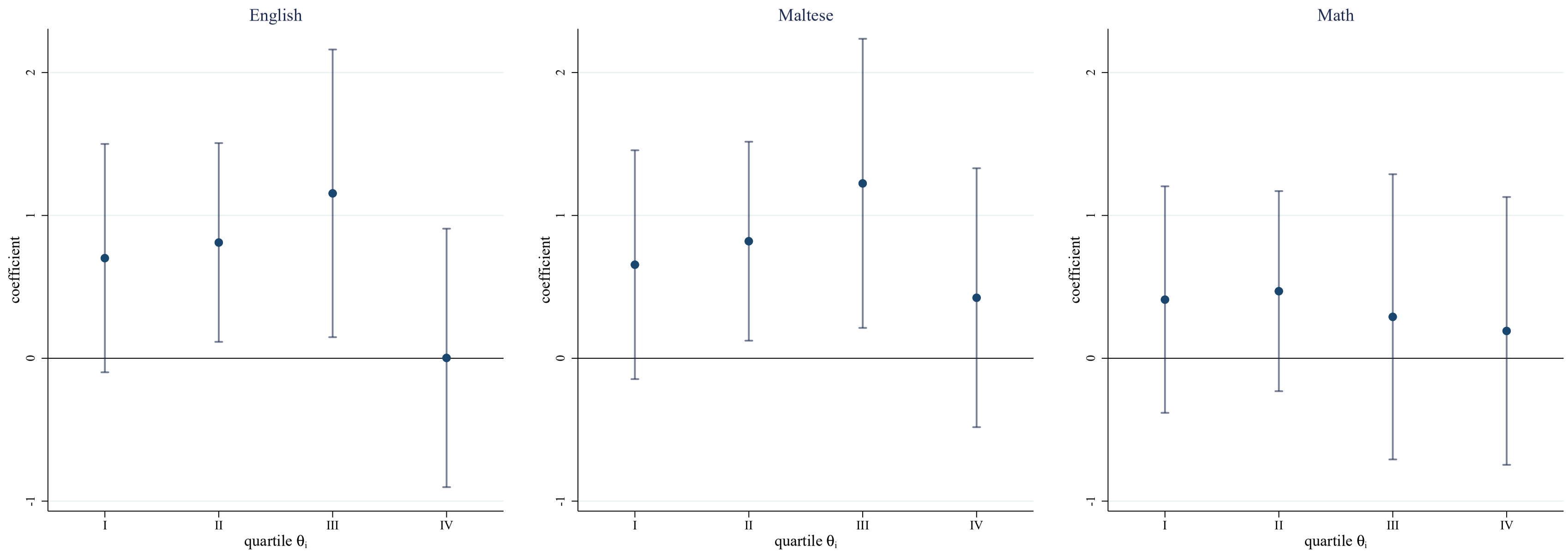
Notes: Each dot represents a school. The y-axes shows the average rank order per school based on the submitted rank-order lists (ROLs). Vertical bars represent 95% confidence intervals. As girls and boys have different number of choices at every lottery (year-grade of application), the ranking was transformed in order to have the same 1 to 10 absolute scale.

Figure 7. 2SLS results by School Type



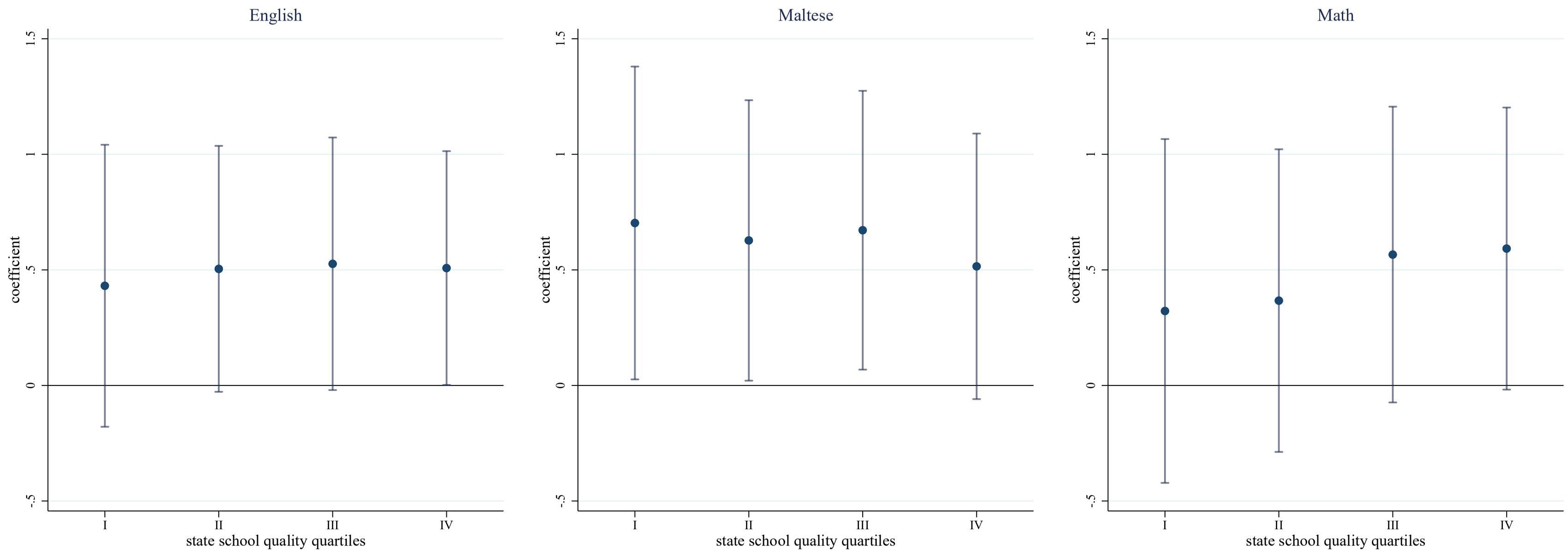
Notes: This figure shows heterogeneous treatment effects by school type for English, Maltese and Math test scores. Each figure shows the estimated coefficient (and the 95 % confidence intervals) on SS_i^g, SS_i^b and CS_i on test scores from the 2SLS model described in equation (4). Regression controls for baseline demographic characteristics, year of exam fixed effect and application pattern.

Figure 9. Heterogeneity in Treatment Effect by Weight placed on Single-sex Attribute



Notes: This figure shows heterogeneous treatment effects by parent's single-sex preference intensity for English, Maltese and Math test scores. Each figure shows the estimated coefficient (and the 95 % confidence intervals) on SS_i interacted with dummies for the quartile of single-sex weight ($\hat{\theta}_i$) on test scores from the 2SLS model described in equation (4). Regression controls for baseline demographic characteristics, quartiles of the single-sex weight, year of exam fixed effect and lottery fixed effect.

Figure 10. Heterogeneity in Treatment Effect by Quality of Neighborhood School



Notes: This figure shows heterogeneous treatment effects by state school quality for English, Maltese and Math test scores. Each figure shows the estimated coefficient (and the 95 % confidence intervals) on SS_i interacted with dummies for the quartile of a measure of state-run school quality on test scores from the 2SLS model described in equation (4). Regression controls for baseline demographic characteristics, quartiles of state school quality, year of exam fixed effect and lottery fixed effect.

Table 1. Applicants and Single-sex School offers per Lottery

Cohort	Lottery Year (1)	Total Applicants (2)	Randomized Applicants (3)	Percent offered a seat (4)
Panel A: Lottery Grade KG2				
Cohort 2005	2009	1091	901	16.3
Cohort 2006	2010	1182	1023	17.1
Panel B: Lottery Grade Y1				
Cohort 2005	2010	979	834	34.9
Cohort 2006	2011	952	716	58.7
Panel C: Lottery Grade Y4				
Cohort 2005	2013	292	264	67.0

Notes: Column 1 shows the calendar year the lottery was held. Column 2 shows the number of applicants for catholic schools slots. Column 3 shows the number of randomized applicants (i.e those in the marginal priority group). Column 4 shows the share of randomized applicants who got a single-sex offer.

Table 2. Descriptive Statistics and Balance Regression

	Child Population (1)	Lottery Losers (2)	Lottery Winners (3)	Balance Regression (4)
Born Quarter 1	0.235	0.244	0.261	0.019
				(0.019)
Born Quarter 2	0.221	0.259	0.239	-0.015
				(0.019)
Born Quarter 3	0.259	0.241	0.239	-0.001
				(0.018)
Born Quarter 4	0.283	0.253	0.259	-0.002
				(0.019)
Biblical Name		0.100	0.099	-0.012
				(0.013)
Popular Surname	0.243	0.243	0.259	0.009
				(0.019)
Mother Age		33.6	34.5	-0.192
				(0.238)
Father Age		35.9	37.1	0.173
				(0.260)
Distance Local School		1.22	1.31	-0.019
				(0.117)
High Quality Local School		0.431	0.436	-0.003
				(0.022)
Southern Harbour	0.195	0.147	0.144	-0.007
				(0.016)
Northern Harbour	0.297	0.325	0.303	-0.009
				(0.020)
South Eastern	0.182	0.152	0.178	0.029
				(0.016)
Western	0.148	0.193	0.197	-0.012
				(0.017)
Northern	0.176	0.181	0.176	0.000
				(0.016)
Social Assistance		4123	4109	-17.808
				(32.368)
Observations		1624	1213	
F-test				55.301
p-value				0.463

Notes: Column 1 reports mean values for children aged 3-5 from the National Statistic Office and the National Obstetric Information System. Columns 2 and 3 report mean values for applicants according to their admission lottery status. Column 4 reports the coefficient (and standard deviation) from a regression of the variable indicated in each row on a dummy variable equal to one if the applicant won the admission lottery and cero otherwise. Regression includes lottery fixed effects. Columns 2-4 sample is restricted to randomized applicants in non-degenerate lotteries. Popular Surname is an indicator variable equal to one if the applicant's surname is one of the 10 most common surnames as identified in the 2011 Population Census: Borg, Camilleri, Vella, Farrugia, Zammit, Galea, Micallef, Grech, Attard and Spiteri. Social Assistance is the SA expenditure per beneficiary (in euros) in the locality in which the applicant resides. The F-statistics jointly test balance for all baseline covariates. The associated p-value indicates a failure to reject the null. An asterisk indicates statistically significant differences between lottery winners and losers at the 5% level.

Table 3. Coeducational and Single-sex Catholic School Characteristics

	Catholic Schools		Difference
	Coeducational	Single Sex	(2)-(1)
	(1)	(2)	(3)
Panel A: School staff			
Female (%)	94.7	91.6	-3.127
Age (years)	40.4	41.1	0.706
Bachelor (%)	50.7	60.2	9.479
Teaching Experience (years)	10.6	10.9	0.258
Tenure (years)	8.4	6.0	-2.381
Distance to School (miles)	2.4	2.4	0.046
Wage (log)	9.6	9.5	-0.076
Observations	171	585	
F-stat			10.351
p-value			(0.170)
Panel B: Teachers			
Female (%)	96.7	96.5	-0.187
Age (years)	36.8	37.2	0.446
Bachelor (%)	83.6	85.0	1.393
Teaching Experience (years)	10.5	10.1	-0.405
Tenure (years)	8.2	8.3	0.107
Distance to School (miles)	2.6	2.3	-0.256
Wage (log)	9.8	9.8	-0.008
Observations	61	202	
F-stat			2.529
p-value			(0.925)
Panel C: School Characteristics			
Instruction Time (hours per week)			
English	5.75	5.84	0.086
Maltese	4.97	4.75	-0.224
Math	5.53	5.62	0.093
PE	1.06	1.26	0.192
Religion	2.42	2.56	0.142
Other	1.60	1.15	-0.456
School Size (m2)	4673.3	4032.3	-641.0
Distance to Valletta (miles)	1.92	2.53	0.616
Class Size	25.7	25.4	-0.394

Notes: Columns 1 and 2 report mean values for staff at each school type, as specified in the column headings. Column 3 reports the difference in means across groups of the variable indicated in each row. The F-statistics, reported at the bottom of the panels, jointly test balance for all baseline covariates. Asterisk indicates statistically significant differences between groups at the 5% level. Source: Administrative school data for the school year 2010.

Table 4. First Stage Estimates

	Risk Set Controls			
	Application Pattern	Application Pattern & First-Choice	Application Pattern & Single-sex First-Choice	Application Pattern & Propensity Score
	(1)	(2)	(3)	(4)
Z^{ss}	0.498***	0.516***	0.529***	0.519***
	(0.017)	(0.035)	(0.033)	(0.035)
N	2599	1286	1286	1265

Notes: This table reports first-stage estimates. The instrument is an indicator equal to one if the applicant received an *initial* single-sex offer (and zero otherwise). The dependent variable is graduating from a single-sex school. All models include year of test dummies. Columns differ in the way the risk set controls are defined. Asterisks indicate statistically significant at the 10% (*), 5% (**) or 1% (***) level.

Table 5. Lottery Results: Effect of Single Sex Schooling

	English			Maltese			Math		
	No Baseline Controls		Baseline Controls	No Baseline Controls		Baseline Controls	No Baseline Controls		Baseline Controls
	Reduced Form	2SLS	2SLS	Reduced Form	2SLS	2SLS	Reduced Form	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				A. Application Pattern					
Single-sex	0.103***	0.207***	0.185***	0.077***	0.155***	0.145**	0.042	0.084	0.071
	(0.028)	(0.055)	(0.060)	(0.029)	(0.057)	(0.061)	(0.030)	(0.060)	(0.065)
N	2599	2599	2599	2596	2596	2596	2564	2564	2564
				B. Application Pattern & First-Choice					
Single-sex	0.114*	0.220**	0.184	0.055	0.106	0.049	0.039	0.075	0.051
	(0.062)	(0.112)	(0.118)	(0.062)	(0.114)	(0.116)	(0.068)	(0.124)	(0.127)
N	1286	1286	1286	1284	1284	1284	1268	1268	1268
				C. Application Pattern & Single-sex First-Choice					
Single-sex	0.130**	0.245**	0.198*	0.091	0.172	0.091	0.042	0.079	0.042
	(0.059)	(0.108)	(0.113)	(0.062)	(0.115)	(0.114)	(0.065)	(0.121)	(0.125)
N	1286	1286	1286	1284	1284	1284	1268	1268	1268
				D. Application Pattern & Propensity Score					
Single-sex	0.156***	0.301***	0.256**	0.107*	0.206*	0.125	0.068	0.131	0.096
	(0.059)	(0.109)	(0.113)	(0.063)	(0.118)	(0.117)	(0.067)	(0.124)	(0.127)
N	1265	1265	1265	1263	1263	1263	1248	1248	1248

Notes: Columns report reduced-form and 2SLS estimates of single-sex school graduation on English, Maltese and Math scores. The instruments are indicators equal to one if the applicant received an *initial* single-sex and catholic school offer (and zero otherwise). Columns (3), (6) and (9) reports 2SLS estimates from specifications that add quarter of birth dummies, and indicator of having a biblical name and locality fixed effects. All models include year of test dummies. Panels differ in the way the risk set controls are defined. Asterisks indicate statistically significant at the 10% (*), 5% (**) or 1% (***) level.

Table 6. Lottery Results: Effect of Same-Gender Peers

	English			Maltese			Math		
	No Baseline Controls		Baseline Controls	No Baseline Controls		Baseline Controls	No Baseline Controls		Baseline Controls
	Reduced Form	2SLS	2SLS	Reduced Form	2SLS	2SLS	Reduced Form	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				A. Application Pattern					
Single-sex	0.261***	0.337***	0.320***	0.237***	0.309***	0.346***	0.148**	0.194**	0.194**
	(0.083)	(0.109)	(0.110)	(0.083)	(0.110)	(0.112)	(0.075)	(0.098)	(0.099)
Catholic	-0.180**	-0.229	-0.244	-0.179**	-0.262*	-0.351**	-0.121*	-0.190	-0.216
	(0.077)	(0.153)	(0.154)	(0.078)	(0.153)	(0.155)	(0.071)	(0.144)	(0.149)
N	2590	2590	2590	2587	2587	2587	2555	2555	2555
				B. Application Pattern & First-Choice					
Single-sex	0.481**	0.875**	0.941***	0.419*	0.742**	0.739**	0.285	0.519	0.580
	(0.240)	(0.388)	(0.358)	(0.227)	(0.372)	(0.334)	(0.224)	(0.362)	(0.355)
Catholic	-0.375	-0.772*	-0.900**	-0.373*	-0.752*	-0.821**	-0.248	-0.519	-0.625
	(0.236)	(0.434)	(0.397)	(0.219)	(0.409)	(0.367)	(0.220)	(0.402)	(0.401)
N	1277	1277	1277	1284	1281	1275	1259	1259	1259
				C. Application Pattern & Single-sex First-Choice					
Single-sex	0.461**	0.832***	0.913***	0.527***	0.754**	0.915***	0.360*	0.647**	0.697**
	(0.205)	(0.321)	(0.300)	(0.203)	(0.326)	(0.297)	(0.194)	(0.317)	(0.305)
Catholic	-0.340*	-0.710*	-0.858**	-0.450**	-0.728*	-0.990***	-0.326*	-0.678*	-0.778**
	(0.203)	(0.368)	(0.338)	(0.198)	(0.413)	(0.330)	(0.190)	(0.354)	(0.343)
N	1277	1277	1277	1275	1281	1275	1259	1259	1259
				D. Application Pattern & Propensity Score					
Single-sex	0.396**	0.616**	0.638**	0.539**	0.794***	0.742***	0.374*	0.567**	0.554**
	(0.200)	(0.271)	(0.252)	(0.219)	(0.299)	(0.268)	(0.191)	(0.257)	(0.249)
Catholic	-0.251	-0.393	-0.475*	-0.431**	-0.801**	-0.717**	-0.296	-0.481*	-0.519*
	(0.199)	(0.305)	(0.281)	(0.217)	(0.390)	(0.297)	(0.189)	(0.286)	(0.281)
N	1275	1275	1275	1273	1279	1273	1257	1257	1257

Notes: Columns report reduced-form and 2SLS estimates of single-sex school graduation (beyond the catholic school effect) on English, Maltese and Math scores. The instruments are indicators equal to one if the applicant received an *initial* single-sex and catholic school offer (and zero otherwise). Columns (3), (6) and (9) reports 2SLS estimates from specifications that add quarter of birth dummies, and indicator of having a biblical name and locality fixed effects. All models include year of test dummies. Panels differ in the way the risk set controls are defined. Asterisks indicate statistically significant at the 10% (*), 5% (**) or 1% (***) level.

Table 7. Subject Options and Primary School Attended

	At least one science subject (1)	Two science subjects (2)	At least one female subject (3)	At least one vocational subject (4)
Girls-only Primary	-0.147	0.176*	-0.170	-0.291
	(0.197)	(0.106)	(0.223)	(0.211)
Boys-only Primary	-0.111	-0.059	-0.078	-0.156**
	(0.071)	(0.060)	(0.076)	(0.074)
Observations	1022	1022	1022	1022

Notes: Table reports 2SLS estimates of the effect of single-sex primary schooling on the outcome variable shown in the column header. Columns 3 and 4 add a control for the number of prevalently female and vocational subjects offered, respectively. Regressions include lottery fixed effects and application pattern. Sample includes all students at Catholic secondary schools that participated in the primary admission lotteries. Robust standard errors, clustered at individual level, are reported in parenthesis. Significance levels are indicated by * < .1, ** < .05, *** < .01.

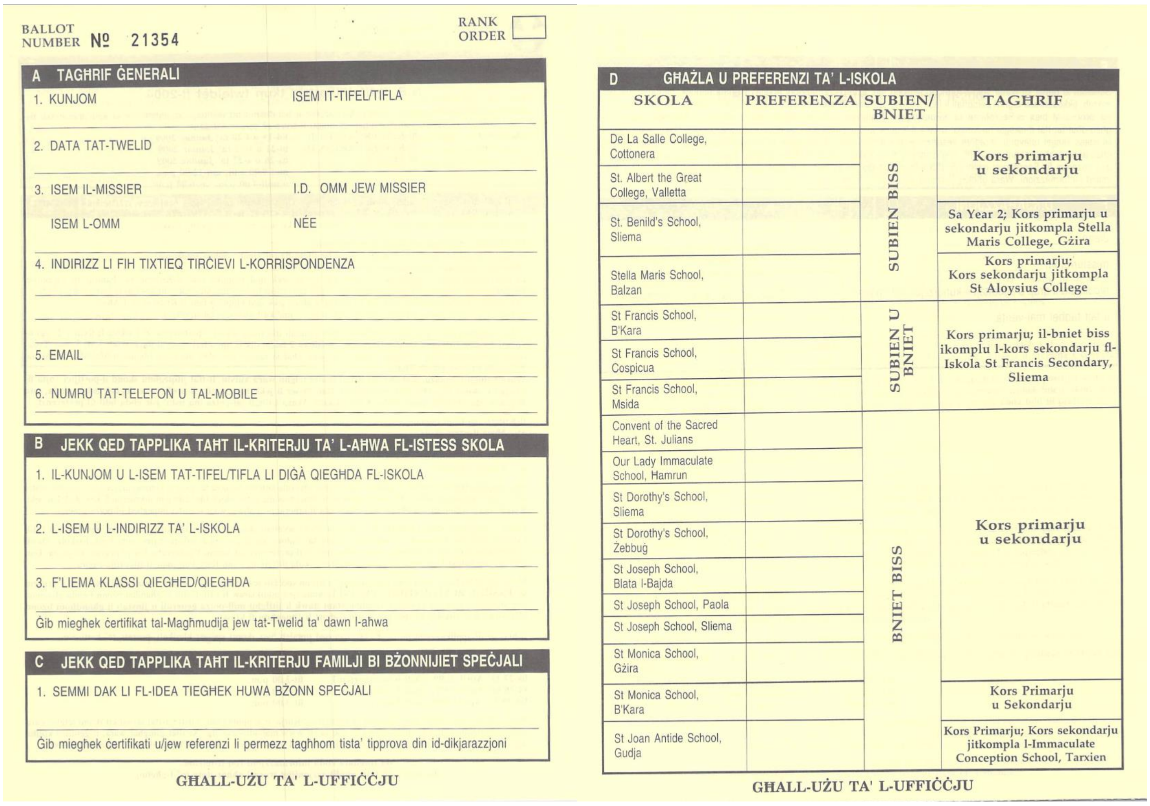
Table 8. Effect of Single-sex School on Teachers Inputs

	Girls-only Schools (1)	Boys-only Schools (2)
Panel A: Teacher Satisfaction & Quality of the Teacher-Student Relationship		
Teacher satisfaction	-0.271	-0.326
	(0.232)	(0.211)
Students are respectful of the teachers	0.664*	0.254
	(0.284)	(0.264)
Panel B: Classroom Environment & School Discipline		
Student misbehavior	-0.640*	0.123
	(0.269)	(0.346)
School rules are clear and enforced	0.583*	0.372
	(0.284)	(0.227)
Panel C: Instructional Practices		
Individualized instruction & feedback	0.300	-0.657*
	(0.498)	(0.387)
Aligned pedagogical practices	-0.080	-0.596
	(0.451)	(0.426)
Guided instruction	0.801*	0.935*
	(0.349)	(0.390)
Ability grouping	-0.790	-0.803
	(0.571)	(0.578)
Panel D: Self- and Student Assessment		
Curriculum expertise	-0.912*	0.137
	(0.371)	(0.269)
Students' performance	0.086	0.026
	(0.590)	(0.590)

Notes: Table reports OLS estimates of the effect of single-sex school on the outcome variable shown in each row. Regressions include a dummy variable equal to one if the school belong to the Catholic sector and 0 if it belongs to the State sector. As such, all comparison are made among 165 teachers at single-sex and coeducational schools. Sample includes teachers of Year 5 students enrolled in Catholic and State schools in 2016 that participated in the PIRLS survey. Robust standard errors, clustered at school level, are reported in parenthesis. Significance levels are indicated by * < .1, ** < .05, *** < .01. See Appendix D for details on the definition of the outcome in each row.

A *Catholic Schools Lottery Data*

Figure A1. Application Form



BALLOT NUMBER № 21354

RANK ORDER

A TAGHRIF ĠENERALI

1. KUNJOM — ISEM IT-TIFEL/TIFLA
2. DATA TAT-TWELID
3. ISEM IL-MISSIER — I.D. OMM JEW MISSIER
   ISEM L-OMM — NÉE
4. INDIRIZZ LI FIH TIXTIEQ TIRĊIEVI L-KORRISPONDENZA
5. EMAIL
6. NUMRU TAT-TELEFON U TAL-MOBILE

B JEKK QED TAPPLIKA TAĦT IL-KRITERJU TA' L-AĦWA FL-ISTESS SKOLA

1. IL-KUNJOM U L-ISEM TAT-TIFEL/TIFLA LI DIĠÀ QIEGĦDA FL-ISKOLA
2. L-ISEM U L-INDIRIZZ TA' L-ISKOLA
3. F'LIEMA KLASSI QIEGĦED/QIEGĦDA

Ġib mieghek ċertifikat tal-Maghmudija jew tat-Twelid ta' dawn l-aħwa

C JEKK QED TAPPLIKA TAĦT IL-KRITERJU FAMILJI BI BŻONNIJIET SPEĊJALI

1. SEMMI DAK LI FL-IDEA TIEGĦEK HUWA BŻONN SPEĊJALI

Ġib mieghek ċertifikati u/jew referenzi li permezz tagħhom tista' tipprova din id-dikjarazzjoni

GĦALL-UŻU TA' L-UFFIĊĊJU

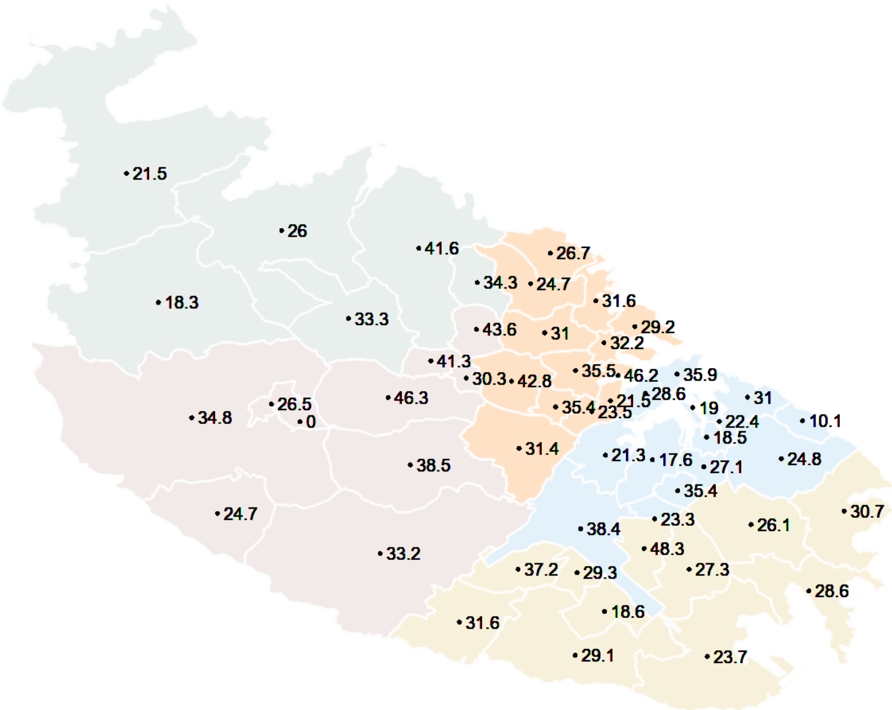
D GĦAŻLA U PREFERENZI TA' L-ISKOLA

SKOLA	PREFERENZA	SUBIEN/ BNIET	TAGĦRIF
De La Salle College, Cottonera		SUBIEN BISS	Kors primarju u sekondarju
St. Albert the Great College, Valletta		SUBIEN BISS	Kors primarju u sekondarju
St. Benild's School, Sliema		SUBIEN BISS	Sa Year 2; Kors primarju u sekondarju jitkompla Stella Maris College, Gżira
Stella Maris School, Balzan		SUBIEN BISS	Kors primarju; Kors sekondarju jitkompla St Aloysius College
St Francis School, B'Kara		SUBIEN U BNIET	Kors primarju; il-bniet biss ikomplu l-kors sekondarju fl-Iskola St Francis Secondary, Sliema
St Francis School, Cospicua		SUBIEN U BNIET	Kors primarju; il-bniet biss ikomplu l-kors sekondarju fl-Iskola St Francis Secondary, Sliema
St Francis School, Msida		SUBIEN U BNIET	Kors primarju; il-bniet biss ikomplu l-kors sekondarju fl-Iskola St Francis Secondary, Sliema
Convent of the Sacred Heart, St. Julians		BNIET BISS	Kors primarju u sekondarju
Our Lady Immaculate School, Hamrun		BNIET BISS	Kors primarju u sekondarju
St Dorothy's School, Sliema		BNIET BISS	Kors primarju u sekondarju
St Dorothy's School, Żebbuġ		BNIET BISS	Kors primarju u sekondarju
St Joseph School, Blata l-Bajda		BNIET BISS	Kors primarju u sekondarju
St Joseph School, Paola		BNIET BISS	Kors primarju u sekondarju
St Joseph School, Sliema		BNIET BISS	Kors primarju u sekondarju
St Monica School, Gżira		BNIET BISS	Kors primarju u sekondarju
St Monica School, B'Kara		BNIET BISS	Kors Primarju u Sekondarju
St Joan Antide School, Gudja		BNIET BISS	Kors Primarju; Kors sekondarju jitkompla l-Immaculate Conception School, Tarxien

GĦALL-UŻU TA' L-UFFIĊĊJU

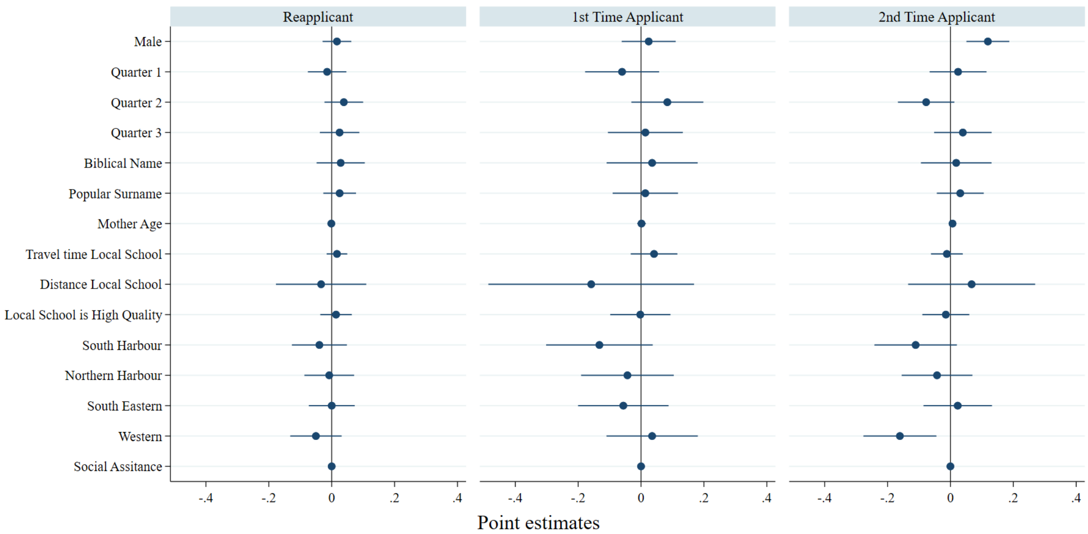
Notes: Application form used to admit student at Catholic schools during scholastic year 2010-2011. Source: Secretariat for Catholic Education.

Figure A2. Lottery Participants by Locality of Residence



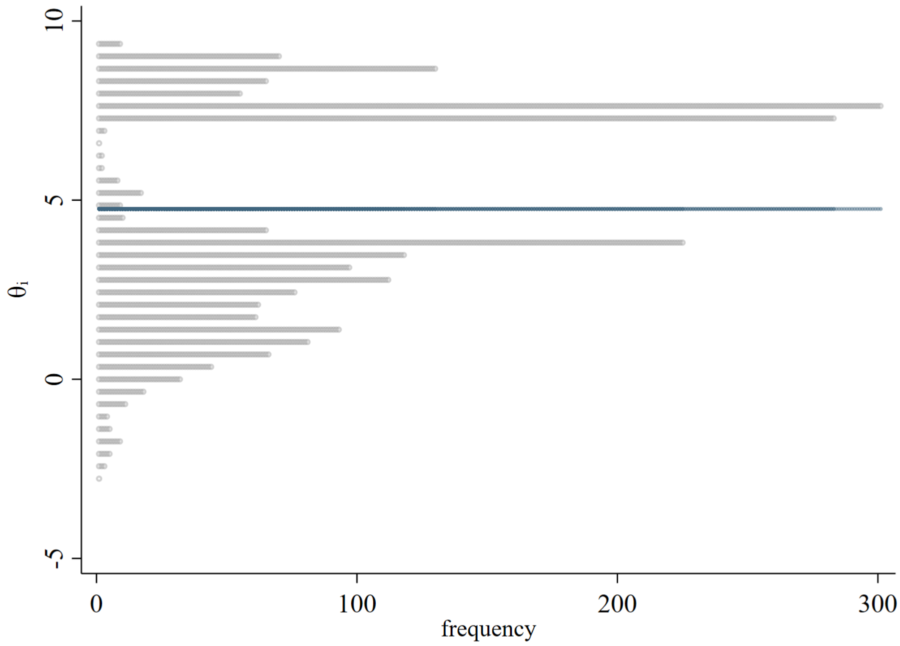
Notes: Each number represents the fraction of the child population (3 to 5 years old) living in each local authority unit by 2010 that participated in catholic schools admission lotteries the same year.

Figure A3. Determinants of Reapplication



Notes: Each subgraph represents the results of a separate regression of applicants' baseline characteristics on reapplication. The first outcome is defined as a dummy variable equal to one if the applicant has ever reapply and zero otherwise. The second outcome is defined as a dummy variable equal to one if the applicant has reapply once and zero if the applicant never reapplied. The last outcome is defined as a dummy variable equal to one if the applicant has reapply twice.

Figure A4. Applicants Weight over Single-Sex Attribute



Notes: This figure shows the distribution of applicants over $\hat{\theta}_i$, the weight parents place on single-sex school attribute. $\hat{\theta}_i$ was estimated using a mixed rank-ordered logit model over the complete ranking of schools data. The model includes teachers' average characteristics (age, wage, tenure and bachelor degree), a single-sex indicator and distance (measure as the distance between the applicant address to each ranked school). The blue line indicates the sample mean value.

B Matching Rate

Table B1. Matching Rate between Lottery Applicants and Exam Takers

Lottery Year	Total Applicants	Randomized Applicants	Match Rate	Balance Regression: All Lotteries	Balance Regression: Benchmark Participating Schools
	(1)	(2)	(3)	(4)	(5)
Panel A: Lottery Grade KG2					
2009	1091	901	0.765		
2010	1182	1023	0.774		
Panel B: Lottery Grade Y1					
2010	979	834	0.788		
2011	952	716	0.777		
Panel C: Lottery Grade Y4					
2013	292	264	0.981		
2009-2013				-0.106	-0.002
				(0.016)	(0.015)

Notes: Column 1 shows the number of candidates to get admission into catholic schools. Column 2 shows the number of randomized applicants (i.e those in the marginal priority group). Column 3 shows the match rates from randomized applicants to scores data at the end of primary school (Benchmark). Column 4 reports the coefficient (and standard deviation) from a regression of the match indicator variable on a dummy variable equal to one if the applicant received an offer (Z_{ij}), using all the lotteries. Column 5 reports the coefficient of the same specification as in column 4 but excluding those applicants who received an offer from a non-benchmark participating school. Regressions include controls for application year, grade and SEN-lottery.

Table B2. Descriptive Statistics and Balance Regression

	Single-sex Offer			Catholic Offer		
	Lottery Losers	Lottery Winners	Balance Regression	Lottery Losers	Lottery Winners	Balance Regression
	(1)	(2)	(3)	(4)	(5)	(6)
Born Quarter 1	0.241	0.268	0.033	0.244	0.258	0.019
			(0.021)			(0.022)
Born Quarter 2	0.261	0.229	-0.023	0.263	0.230	-0.028
			(0.022)			(0.023)
Born Quarter 3	0.243	0.238	-0.004	0.242	0.240	-0.003
			(0.021)			(0.021)
Born Quarter 4	0.254	0.263	-0.005	0.249	0.269	0.012
			(0.021)			(0.022)
Biblical Name	0.095	0.099	-0.003	0.093	0.101	-0.002
			(0.015)			(0.016)
Popular Surname	0.251	0.252	-0.010	0.247	0.258	0.003
			(0.021)			(0.022)
Mother Age	33.6	34.6	0.094	33.4	34.6	0.155
			(0.252)			(0.265)
Father Age	35.9	37.3	0.317	35.8	37.3	0.499
			(0.271)			(0.277)
Distance Local School	1.30	1.29	-0.079	1.26	1.34	-0.009
			(0.139)			(0.140)
High Quality Local School	0.283	0.307	0.018	0.278	0.310	0.029
			(0.023)			(0.023)
Southern Harbour	0.150	0.139	-0.017	0.149	0.143	-0.011
			(0.018)			(0.019)
Northern Harbour	0.316	0.286	-0.024	0.322	0.282	-0.036
			(0.022)			(0.023)
South Eastern	0.163	0.171	-0.002	0.153	0.184	0.030
			(0.019)			(0.019)
Western	0.189	0.205	0.011	0.187	0.205	0.011
			(0.020)			(0.020)
Northern	0.179	0.197	0.033	0.187	0.184	0.005
			(0.018)			(0.019)
Social Assistance	4118	4096	-24.729	4122	4093	-41.468
			(36.437)			(38.041)
F-test			48.547			63.058
p-value			0.683			0.240

Notes: Columns 1-2 (4-5) report mean values for applicants according to admission lottery status. Column 3 (6) reports the coefficient (and standard deviation) from a regression of the variable indicated in each row on a dummy variable equal to one if the applicant won the admission lottery and zero otherwise. Regression includes lottery fixed effects. Sample is restricted to randomized applicants, in non-degenerate lotteries, that were matched to Benchmark. The F-statistics jointly test balance for all baseline covariates. Asterisk indicates statistically significant differences at the 5% level.

Table B3. Characteristics of Non-randomized Applicants and Single-sex Schools Lottery Winner

	Non-randomized Applicants (1)	Lottery Winners (2)	Balance Regression (3)
Born Quarter 1	0.229	0.261	0.028
			(0.022)
Born Quarter 2	0.255	0.239	-0.013
			(0.023)
Born Quarter 3	0.251	0.239	-0.013
			(0.022)
Born Quarter 4	0.264	0.259	-0.002
			(0.023)
Biblical Name	0.083	0.099	0.011
			(0.015)
Popular Surname	0.255	0.259	-0.004
			(0.023)
Mother Age	34.8	34.5	-0.867*
			(0.294)
Father Age	37.3	37.1	-0.710*
			(0.310)
Distance Local School	1.27	1.31	0.032
			(0.148)
High Quality Local School	0.264	0.304	0.039
			(0.023)
Southern Harbour	0.160	0.144	-0.020
			(0.019)
Northern Harbour	0.308	0.303	0.005
			(0.024)
South Eastern	0.155	0.178	0.023
			(0.019)
Western	0.197	0.197	-0.008
			(0.021)
Northern	0.177	0.176	0.000
			(0.019)
Social Assistance	4150	4109	-47.952
			(38.343)
Observations	541	1213	
F-test			13.707
p-value			0.471

Notes: Columns 1 and 2 report mean values for non-randomized students and single-sex slot winners, respectively. Column 3 reports the coefficient (and standard deviation) from a regression of the variable indicated in each row on a dummy variable equal to one if the students won admission to a single-sex school via random lottery numbers and zero if the student won admission by priority status (non-randomized). Regression includes lottery fixed effects. Columns 1-3 sample is restricted to applicants that born in 2005 and 2006, in lotteries where there is variation in the running variable and with complete baseline characteristics. For detailed description of variables, see notes on Table 2. The F-statistics jointly test balance for all baseline covariates. Asterisk indicates statistically significant differences at the 5% level.

Table B4. Coeducational, Girls-only and Boys-only Catholic School Characteristics

	Catholic Schools			Diff	Diff
	Coed (1)	Girls-only (2)	Boys-only (3)	(2)-(1) (4)	(3)-(1) (5)
Panel A: School staff					
Female (%)	94.7	93.3	88.8	-1.404	-5.898
Age (years)	40.4	42.3	39.4	1.856	-1.042
Bachelor (%)	50.7	64.5	53.8	13.805*	3.073
Teaching Experience (years)	10.6	10.8	11.0	0.144	0.429
Tenure (years)	8.4	5.2	7.3	-3.222	-1.124
Distance to School (miles)	2.4	2.4	2.5	-0.030	0.167
Wage (log)	9.6	9.5	9.5	-0.065	-0.093
Observations	171	361	224		
F-stat				18.536	6.621
p-value				(0.010)	(0.469)
Panel B: Teachers					
Female (%)	96.7	98.5	93.1	1.740	-3.666
Age (years)	36.8	37.9	36.1	1.100	-0.708
Bachelor (%)	83.6	85.9	83.3	2.331	-0.273
Teaching Experience (years)	10.5	10.2	10.1	-0.364	-0.482
Tenure (years)	8.2	8.3	8.3	0.134	0.057
Distance to School (miles)	2.6	2.2	2.5	-0.345	-0.099
Wage (log)	9.8	9.8	9.8	0.001	-0.024
Observations	61	130	72		
F-stat				1.611	6.363
p-value				(0.978)	(0.498)
Panel C: School Characteristics					
Instruction Time (hours per week)					
English	5.75	5.92	5.63	0.175	-0.120
Maltese	4.97	4.65	4.97	-0.317	-0.005
Math	5.53	5.85	5.09	0.321	-0.440
PE	1.06	1.30	1.16	0.233	0.097
Religion	2.42	2.74	2.13	0.327	-0.292
Other	1.60	0.98	1.53	-0.622*	-0.069
School Size (m2)	4673.3	2997.0	6448.0	-1676.3	1774.6
Distance to Valletta (miles)	1.92	2.25	2.93	0.334	1.018
Class Size	25.7	25.3	25.4	-0.458	-0.325

Notes: Columns 1-3 report mean values for staff at each school type, as specified in the column headings. Columns 4-5 reports the difference in means across groups of the variable indicated in each row. The F-statistics, reported at the bottom of the panels, jointly test balance for all baseline covariates. Asterisk indicates statistically significant differences between groups at the 5% level. Source: Administrative school data for the school year 2010.

Appendix Table B4. Lottery Results First Time Applicants: Same-Gender Peers

	English			Maltese			Math		
	No Baseline Controls		Baseline Controls	No Baseline Controls		Baseline Controls	No Baseline Controls		Baseline Controls
	Reduced Form	2SLS	2SLS	Reduced Form	2SLS	2SLS	Reduced Form	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				A. Application Pattern					
Single-sex	0.403***	0.527***	0.488***	0.364***	0.482***	0.504***	0.182*	0.238	0.205
	(0.118)	(0.167)	(0.161)	(0.123)	(0.174)	(0.172)	(0.108)	(0.149)	(0.148)
Catholic	-0.179	-0.227	-0.215	-0.182	-0.251	-0.323	-0.091	-0.122	-0.133
	(0.114)	(0.208)	(0.197)	(0.120)	(0.219)	(0.214)	(0.106)	(0.194)	(0.196)
N	1665	1665	1665	1662	1662	1662	1641	1641	1641
				B. Application Pattern & First-Choice					
Single-sex	0.366*	0.760*	0.788**	0.325	0.662	0.679*	0.230	0.442	0.442
	(0.202)	(0.390)	(0.345)	(0.219)	(0.410)	(0.373)	(0.214)	(0.387)	(0.360)
Catholic	-0.196	-0.507	-0.627*	-0.206	-0.503	-0.596	-0.200	-0.439	-0.510
	(0.194)	(0.414)	(0.358)	(0.205)	(0.427)	(0.387)	(0.206)	(0.408)	(0.380)
N	1277	897	897	1284	895	895	884	884	884
				C. Application Pattern & Single-sex First-Choice					
Single-sex	0.382**	0.784**	0.817***	0.506**	1.012***	0.891***	0.359*	0.678*	0.591*
	(0.170)	(0.311)	(0.299)	(0.208)	(0.373)	(0.341)	(0.194)	(0.349)	(0.327)
Catholic	-0.188	-0.483	-0.607*	-0.331*	-0.783**	-0.750**	-0.325*	-0.690*	-0.681**
	(0.164)	(0.330)	(0.313)	(0.199)	(0.399)	(0.360)	(0.184)	(0.361)	(0.340)
N	897	897	897	895	895	895	884	884	884
				D. Application Pattern & Propensity Score					
Single-sex	0.284*	0.498**	0.443*	0.487**	0.794**	0.612**	0.343*	0.530*	0.385
	(0.160)	(0.243)	(0.227)	(0.237)	(0.342)	(0.305)	(0.201)	(0.288)	(0.261)
Catholic	-0.074	-0.129	-0.173	-0.305	-0.518	-0.438	-0.288	-0.481	-0.430
	(0.157)	(0.257)	(0.235)	(0.232)	(0.370)	(0.328)	(0.193)	(0.298)	(0.275)
N	896	896	896	894	894	894	883	883	883

Notes: Columns report reduced-form and 2SLS estimates of single-sex school graduation on English, Maltese and Math scores using only first time applicantions. The instruments are indicators equal to one if the applicant received an *initial* single-sex and catholic offer (and zero otherwise). Columns (3), (6) and (9) reports 2SLS estimates from specifications that add quarter of birth dummies, and indicator of having a biblical name and locality fixed effects. All models include year of test dummies. Panels differ in the way the risk-set is defined. Asterisks indicate statistically significant at the 10% (*), 5% (**) or 1% (***) level.

Table B5. Effect of Same-Gender Peers by School Type

	English		Maltese		Math	
	No Baseline Controls	Baseline Controls	No Baseline Controls	Baseline Controls	No Baseline Controls	Baseline Controls
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Application Pattern			
Girls-Only	0.436***	0.401***	0.260*	0.291**	0.257*	0.256*
	(0.140)	(0.139)	(0.142)	(0.141)	(0.137)	(0.137)
Boys-only	0.287***	0.280**	0.333***	0.373***	0.163	0.163
	(0.111)	(0.113)	(0.111)	(0.114)	(0.101)	(0.102)
Catholic	-0.250	-0.258*	-0.252	-0.341**	-0.203	-0.228
	(0.155)	(0.155)	(0.156)	(0.157)	(0.146)	(0.149)
N	2590	2590	2587	2587	2555	2555
			B. Application Pattern & First-Choice			
Girls-Only	0.860**	0.954**	0.583	0.588	0.430	0.517
	(0.408)	(0.374)	(0.405)	(0.365)	(0.406)	(0.395)
Boys-only	0.903**	0.917**	1.033***	1.006***	0.695*	0.697*
	(0.402)	(0.382)	(0.399)	(0.372)	(0.359)	(0.357)
Catholic	-0.770*	-0.902**	-0.734*	-0.798**	-0.514	-0.618
	(0.437)	(0.397)	(0.428)	(0.387)	(0.416)	(0.412)
N	1277	1277	1275	1275	1259	1259
			C. Application Pattern & Single-sex First-Choice			
Girls-Only	0.809**	0.881***	0.817**	0.781**	0.577*	0.625*
	(0.334)	(0.311)	(0.356)	(0.324)	(0.350)	(0.334)
Boys-only	0.886**	0.983***	1.184***	1.203***	0.812**	0.859**
	(0.354)	(0.351)	(0.358)	(0.344)	(0.327)	(0.334)
Catholic	-0.711*	-0.859**	-0.925**	-0.993***	-0.685*	-0.783**
	(0.371)	(0.342)	(0.386)	(0.349)	(0.363)	(0.353)
N	1277	1277	1275	1275	1259	1259
			D. Application Pattern & Propensity Score			
Girls-Only	0.574**	0.603**	0.650**	0.591**	0.486*	0.480*
	(0.286)	(0.262)	(0.320)	(0.285)	(0.289)	(0.275)
Boys-only	0.714**	0.722**	1.167***	1.101***	0.768***	0.738**
	(0.315)	(0.312)	(0.327)	(0.312)	(0.283)	(0.291)
Catholic	-0.396	-0.478*	-0.696**	-0.727**	-0.490*	-0.528*
	(0.307)	(0.283)	(0.339)	(0.303)	(0.291)	(0.287)
N	1275	1275	1273	1273	1257	1257

Notes: Columns report 2SLS estimates using the stacked dataset and cluster standard errors at individual level. Panels differ in the way the risk sets are defined. Asterisks indicate statistically significant at the 10% (*), 5% (**) or 1% (***) level.

C Catholic Secondary Schools

C.1 Transition from Primary to Secondary

While there is a combination of single-sex and coeducational schools at primary level, all secondary schools are single-sex within the Catholic sector. The transition from primary to secondary schools occurs in a way that every student is guaranteed a place. For those primary schools that offer secondary education (in a different building and sometime in a different location), students usually stay within the same school. Students from primary schools that do not offer secondary education have to be allocated into (single-sex) secondary schools. A high school admission lottery creates this allocation, which guarantee that students cannot select into different catholic high schools.[43] Thus, secondary schools comprises students coming from both single-sex and coed primary schools.

C.2 Subject Options

Although schools can differ in the set of optional subjects offered, these differences are unlikely a concern for a number of reasons. First, all schools offer the same science courses (physics, chemistry and biology). In boys-only schools, physics is usually mandatory and the other two science courses are available in the option subject set. For girls-only schools, there is usually not such a requirement of physics being mandatory. A typical female student has to choose 3 optional subjects, but the science subjects are displayed in a way that they have to choose at least one of them, with the rest remaining available if they want to take more than one science course. So, for every student in the sample I net out one science subject (physics for boys and the first observed for girls), and use the rest of their choices. Second, in all cases it is possible to observe a wide number of topics that span very similar fields. Finally, conditional on gender, the available subjects are quite similar irrespective of the type of primary school education.

Table C.1 shows that the number of vocational and prevalently female subjects differ slightly according to the type of primary attended. The regression for the number of science was excluded as there is no variability among schools. Science courses comprise biology, chemistry and physics and all secondary schools offer them. Vocational courses include Information Technology, Engineering Technology, Hospitality, Agribusiness, Health and Social Care, Media Literacy Education and Textiles and Fashion.[44] Prevalently female subjects include Home Economics, Health and Social Care, Hospitality, Textile and Fashion and Art/Drama.

[43] As there are usually more slots than candidates from the catholic primary, students coming from other school sectors participate.

[44] Hairdressing and Beauty and Retail should be included in this list but none of the schools offer these subjects.

Table C1. Optional Subject Set and Primary School Attended

	Prevalently Female Subjects		Vocational Subjects	
	Girls	Boys	Girls	Boys
	(1)	(2)	(3)	(4)
Single-Sex Primary	-0.260***	0.098	-0.013	-0.157**
	(0.053)	(0.065)	(0.042)	(0.071)
Mean coed	3.22	2.42	1.36	1.81
Observations	2024	1565	2024	1594

Notes: Table reports OLS estimates of the effect of single-sex primary schooling on the number of prevalently female (columns 1 and 2) and vocational (columns 3 and 4) subjects offered in Catholic secondary schools. The third row reports the average number of subjects offered to students comming from a coeducation primary school. Sample includes all graduate students from Catholic primary schools. Robust standard errors are reported in parenthesis. Significance levels are indicated by * < .1, ** < .05, *** < .01.

Girls that attended coeducational schools end up in Catholic secondary schools that offer, on average, 3.2 prevalently female subjects, as reported in the second row of column 1. Girls coming from single-sex primary schools face a slightly less number of female dominated subjects (-0.260). For boys, the effect goes in the other direction, though it is not statistically significant. In relation to the number of vocational subjects reported in columns 3 and 4, while there is evidence of a balanced number of subjects offered to girls, boys coming from single-sex primary schools face less number of vocational subjects to opt for (-0.157) compared to boys coming from coeducational schools. Given these small differences, the main specification include the number of subjects offered as controls.

Table C2. Subject Options and Primary School Attended

	At least one science subject	Two science subjects	At least one female subject	At least one vocational subject
	(1)	(2)	(3)	(4)
Girls-only Primary	0.022	0.138**	-0.191	-0.360***
	(0.139)	(0.070)	(0.148)	(0.136)
Boys-only Primary	-0.092	-0.073	-0.028	-0.156**
	(0.060)	(0.052)	(0.061)	(0.063)
Observations	1022	1022	1022	1022

Notes: Table reports OLS estimates of the effect of single-sex primary schooling on the outcome variable shown in the column header. Columns 3 and 4 add a control for the number of prevalently female and vocational subjects offered, respectively. Regressions include lottery fixed effects and application pattern. Sample includes all students at Catholic secondary schools that participated in the primary admission lotteries. Robust standard errors, clustered at individual level, are reported in parenthesis. Significance levels are indicated by * < .1, ** < .05, *** < .01.

D Data Appendix Mechanisms

D.1 Teacher Survey

Teacher satisfaction is an index derived from five survey questions which ask teachers to classify how often they feel in a particular way about being a teacher. The statements are "I am content with my profession as a teacher", "I find my work full of meaning and purpose", "I am enthusiastic about my job", "My work inspires me", "I am proud of the work I do". The answer categories are very often, often, sometimes and never or almost never. I sum teacher responses to each of the statements and standardized the index to be mean zero with unit variance. The measure of the quality of the teacher-student relationship is constructed based on teacher agreement to the statement "The students are respectful of the teachers". The answer categories are agree a lot, agree a little, disagree a little and disagree a lot, and the measure is standardized to have a mean of zero and standard deviation of one.

Student misbehaviour represents an index of the extent to which teachers reported that disruptive students limit how they teach the class and their disagreement with the statement "The students behave in an orderly manner". Each variable is standardized before creating the average. The measure for school rules combines agreement with the statements "This school has clear rules about student conduct" and "This school's rules are enforced in a fair and consistent manner".

The measure of individualized instruction and feedback is derived using answer to the statements "I use individualized instruction for reading", "Provide materials that are appropriate for the reading levels of individual students", "Give individualized feedback to each student". In relation to teacher alignment of pedagogical practices, I combine answers to the statements "Provide reading materials that match the students' interests", "Link new content to students' prior knowledge", "Use multiple perspectives (among students and texts) to enrich understanding". Guided instruction and ability grouping are derived from a survey question asking teacher how often they organize students "to work independently on an assigned plan or goal" and "create same-ability groups", respectively.

Teachers' curriculum expertise is based on teacher characterization of their school for the following statements: "Teachers' understanding of the school's curricular goals" and "Teachers' degree of success in implementing the school's curriculum". Last, teachers assessment of students performance is constructed using the statements: "Students' desire to do well in school" and "Students' ability to reach school's academic goals".

Table D1. Effect of Single-sex School on Teachers Inputs
(excluding teachers from non-participating schools)

	Girls-only Schools (1)	Boys-only Schools (2)
Panel A: Teacher Satisfaction & Quality of the Teacher-Student Relationship		
Teacher satisfaction	-0.121	-0.388
	(0.213)	(0.241)
Students are respectful of the teachers	0.604*	0.100
	(0.307)	(0.278)
Panel B: Classroom Environment & School Discipline		
Student misbehavior	-0.661*	0.441
	(0.306)	(0.411)
School rules are clear and enforced	0.666*	0.194
	(0.278)	(0.188)
Panel C: Instructional Practices		
Individualized instruction & feedback	0.338	-0.652
	(0.568)	(0.463)
Aligned pedagogical practices	-0.107	-0.406
	(0.479)	(0.435)
Guided instruction	0.704*	1.083*
	(0.353)	(0.452)
Ability grouping	-0.852	-0.647
	(0.582)	(0.598)
Panel D: Self- and Student Assessment		
Curriculum expertise	-0.849*	0.163
	(0.433)	(0.334)
Students' performance	0.124	0.124
	(0.614)	(0.653)

Notes: Table reports OLS estimates of the effect of single-sex school on the outcome variable shown in each row. Regressions include a dummy variable equal to one if the school belong to the Catholic sector and 0 if it belongs to the State sector. As such, all comparison are made among 154 teachers at single-sex and coeducational schools. Sample includes teachers of Year 5 students enrolled in Catholic and State schools in 2016 that participated in the PIRLS survey and exclude those working at schools that do not participate in the Benchmark examination. Robust standard errors, clustered at school level, are reported in parenthesis. Significance levels are indicated by * $<$ .1, ** $<$ .05, *** $<$ .01.

Table D2. School Principals' Survey

	Girls-only Schools (1)	Boys-only Schools (2)
Panel A: Quality of the Teacher-Student Relationship		
Intimidation or verbal abuse of teachers or staff	-1.143	-9.288
	(0.595)	(0.630)
Panel B: Classroom Environment		
Absenteeism/Late arrival at school	-0.414	-0.118
	(0.587)	(0.622)
Classroom disturbance	-1.173*	-0.926
	(0.587)	(0.622)
Intimidation or verbal abuse among students	0.136	0.486
	(0.600)	(0.635)
Physical fights among students	-0.946	0.193
	(0.579)	(0.613)
Panel C: Teacher and Student Assessment		
Curriculum expertise	0.041	0.029
	(0.598)	(0.634)
Students' performance	0.435	0.650
	(0.592)	(0.627)

Notes: Table reports OLS estimates of the effect of single-sex school on the outcome variable shown in each row. Regressions include a dummy variable equal to one if the school belong to the Catholic sector and 0 if it belongs to the State sector. As such, all comparison are made among 80 school principals at single-sex and coeducational schools. Sample includes school heads of Catholic and State primary schools in 2016 that participated in the PIRLS survey. Robust standard errors, clustered at school level, are reported in parenthesis. Significance levels are indicated by * < .1, ** < .05, *** < .01.