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

Soledad Giardili†

November 22, 2019

JOB MARKET PAPER

[click here for the most recent version]

Abstract

In the last decade, single-sex education has experienced a major revival. However, whether same-gender schooling is an appropriate strategy to boost academic achievement remains an open question. I leverage randomized assignment of 4- and 5-year-old children to schools in Malta to estimate the causal effect of single-sex education on short- and medium-run outcomes. To alleviate concerns of endogenous school inputs, I compare students within the same school sector, for which coeducational and single-sex schools are alike in all dimensions except for the gender composition of the student population. I find that attending a single-sex primary school produces large and significant test score gains for both boys and girls at the end of primary school. Furthermore, single-sex schooling in childhood has lasting effects on the choice of curriculum track in secondary school. Students make less gendered subject choices and are less likely to enroll in vocational subjects. Survey evidence suggests that the single-sex school effect is mediated through higher student satisfaction with school, lower levels of classroom disruption and teachers' use of guided instruction.

*I thank my advisors, Erich Battistin and Francesco Fasani, for their support and guidance on this project. I also thanks Marcos Vera-Hernández, Barbara Petrongolo, Jonas Vlachos, Sarah Smith, Marina della Giusta, Mårten Palme, David Strömberg, Marcos Manacorda, Diego Battiston, Ines Helm, Ferenc Szucs, Mitch Downey, Sebastian Axbard, Anna Raute, Mauricio Romero, Arne Hole, Jordi Blanes i Vidal, Kamalini Ramdas as well as seminar and conference participants for helpful conversations and valuable comments. Special thanks are due to Fr Charles Mallia and the team from the Secretariat for Catholic Education and Louis Scerri, Karen Grixti and Carmen Muscat from the Educational Assessment Unit in the Ministry of Labor and Education in Malta for their help with data collection and for providing valuable institutional information. I am also grateful to the Data Protection Office and the National Statistic Office in Malta as well as The International Association for the Evaluation of Educational Achievement (IAE) for their assistance with data access. All results have been reviewed to ensure that no confidential information is disclosed. I thankfully acknowledge financial assistance by a Small Academic Expenses Grant from the Royal Economic Society. All opinions, findings, conclusions and errors are those of the author and do not reflect the views of any other entity.

†School of Economics and Finance, Queen Mary University of London, Mile End Rd, E1 4NS. Email: s.giardili@qmul.ac.uk.

1. Introduction

Among a number of school inputs with the potential to improve students' academic performance, single-sex education has recently attracted increasing attention. This is partly because gender-homogeneous schools and classrooms have been expanded in the United States and in England over the last decade in an attempt to raise the educational achievement of boys and girls.[1]. Furthermore, its relatively low implementation cost—reshuffling students across classrooms or schools—makes this strategy very attractive.

Although the concept is not new, the rationale behind single-sex education is. Proponents of single-sex education argue that gender-homogeneous environments improve student outcomes by tailoring instruction according to gender-specific needs, improving classroom behavior, and making gender stereotypes less salient (e.g., girls being labelled as less talented in math). However, there is little credible empirical evidence that attending single-sex schools improves students' educational outcomes. Moreover, the few studies available are based on high school or university students, and there is no evidence from early school stages, where the aforementioned benefits may be particularly relevant.

This paper fills this gap in the literature by assessing the impact of single-sex education among children of primary school age. The empirical evidence on the effects of single-sex education for adolescents or young adults may not hold for students at early ages. Early and middle childhood are key stages of an individual's physical and cognitive development (Eccles, 1999; Heckman, Pinto, and Savelyev, 2013). During this period, children develop new life habits and social roles, and thus, this is where the foundations of gender gaps is laid (Ruble and Martin, 1998). It is also the period in which school inputs are most effective in improving students' cognitive development (Chetty et al., 2011; Heckman, Pinto, and Savelyev, 2013).

Evaluating single-sex education is challenging due to the lack of detailed data and natural experiments. Typically, students who choose to attend a single-sex school differ in important, unobserved ways from those who choose a coeducational (coed) school. Furthermore, even when it is possible to control for the sorting of students,

[1]In the U.S., the debate on single-sex education returned to the forefront after the amendments to the No Child Left Behind Act in 2006 that allowed publicly funded schools to experiment with single-sex education. By 2014, at least 133 single-sex schools and 794 public coeducational schools were offering single-sex classes (Klein et al., 2018). In England, single-sex education is backed by the government when such change better serve the local community (**UKDE18b**). For example, coeducational schools can implement single-sex classes if boys or girls are doing disproportionately bad in a particular subject or their participation is particularly low (e.g. girls in STEM subjects.)

estimates could be biased due to the fact that single-sex schools may differ in other dimensions from their coed counterparts, beyond classroom gender composition.

I leverage admission lotteries and rich administrative school records in Malta to quantify the short- and medium-run effects of attending a single-sex school on students' academic outcomes. The Maltese setting offers a unique opportunity to disentangle the effect of single-sex schooling. First, admission to Catholic primary schools is based on a centralized assignment mechanism based on lotteries that creates an exogenous allocation of students to single-sex and coed schools. Second, for institutional reasons, these schools are strongly homogeneous in a wide range of important dimensions (e.g., curriculum design, teacher qualifications, class size). I bring to bear unique school-level data to provide empirical evidence on this matter.

I begin by studying the impact of single-sex education on students' test scores on a national standardized exam at the end of primary school, i.e., after 6 or 7 years of schooling. I find large positive effects in all subjects areas, of about 0.70σ for English and Maltese, and 0.55σ for mathematics. To put this in perspective, the annual equivalent of these overall effects is four to five times higher than the "per-pupil" effect found in class size studies (Jepsen, 2015). The effect sizes are also about half the effect size found in lottery-based studies for charter schools in Boston (Abdulkadiroğlu et al., 2011 and Angrist et al., 2012).

Interestingly, I find that single-sex education benefits both boys and girls. Students show better average performance in all subjects, and particularly in those in which they do not traditionally have a comparative advantage. Specifically, girls' performance on math is 0.2σ higher than their performance in English and Maltese, while the boys' performance for the language subjects is twice the effect size for math.

The main effects are not driven by parents with strong preference for single-sex schools. To measure preferences over the single-sex school attribute, I estimate a discrete choice model by exploiting information on the school ranking submitted by the parents when applying to the admission lotteries. My results contrast with those of existing lottery-based studies, which usually find that the effects are concentrated among those assigned to schools ranked high in student preferences.

I also examine whether attending a single-sex primary school affects the choice of subjects taken in secondary school. It is a well-established fact that there is a substantial gender difference in major choices and occupations (OECD, 2016), and this may be explained by the choice of curriculum track in secondary school (Card and Payne, 2017). In theory, single-sex schooling might reduce the intake of gender-typical courses, especially for girls, by making gender less salient, lowering competition (Riordan, 1990), and boosting academic self-concept (Sullivan, 2009; Kessels and Hannover, 2010; Sax, Shapiro, and Kevin, 2011). However, the existing literature on

single-sex education does not find evidence that female high school students take more science courses than their counterparts in coeducational settings (Jackson, 2012) or that they are more likely to join a university with a STEM college major (Park, Behrman, and Choi, 2018). One possible explanation is that interventions in adolescence or later, such as a single-sex school environment, come too late (Cunha and Heckman, 2007; Cunha, Heckman, and Schennach, 2010). Gender stereotypes are internalized early in life through teachers and parents (see, for example, Gunderson et al., 2012; Alan, Ertac, and Mumcu, 2018; Carlana, 2019), and traditional norms and beliefs about gender are presumably less likely to be challenged at these higher educational levels.

All Catholic secondary schools in Malta are single-sex, and at the end of Year 8, the second year of secondary school, students have to choose two elective subjects among a set of academic and vocational courses that supplement the national core curriculum. As in the case of primary schools, admission to secondary schools is orthogonal to individual characteristics. This is because there are only two paths to enter a Catholic secondary school, and both are ultimately determined by a lottery. First, some primary schools have associated secondary schools, in which case admittance is granted. Second, those attending a Catholic primary without continuation or those coming from the state sector are admitted through a lottery system.[2] This characteristic, combined with the fact that the subject choice set offered to students does not differ substantially across schools, allows me to test whether students at single-sex secondary schools differ in their choice of subjects depending on the type of primary school attended. Because all students are exposed to single-sex education after coming from different primary schools, it is possible to regard this exercise as a test of whether single-sex schooling in high school comes too late to affect gendered course-taking patterns and preferences for STEM. The results are consistent with the idea that single-sex education at primary school age renders gender less salient, and thus students' choice of subjects is less gender-typical. Female students who attended a girls-only primary school take more science than female students coming from a coeducational primary school, while boys take less science. The effect on "prevalently female" courses (such as hospitality and health and social care) is negative, although imprecisely estimated. Finally, single-sex education reduces, on average, the likelihood of choosing vocational subjects as electives for both genders. Given that academic courses match university entrance requirements more than vocational courses do, my findings suggest that attending a

[2]The transition from primary to secondary schools within the Catholic sector is discussed in detail in Appendix Section E.

single-sex primary school increases the odds of boys and girls entering university.

Overall, the results suggest that attending a single-sex school increases student learning and also has significant and lasting effects on important determinants of career choice, such as the curriculum track decision. One possible explanation for these findings is that teachers and schools are able to specialize when they serve gender-homogeneous groups. Teachers may adopt pedagogical practices (types of examples used, the ability level to which the class is pitched) and discipline methods that best suit students of a given gender. Another (not mutually exclusive) mechanism behind the single-sex school effect could be related to student responses to the gender composition of the classroom, the direct gender peer effect. The presence of the opposite sex may be a distracting or an intimidating factor affecting student beliefs about their own academic abilities.

I study the mechanisms mediating the single-sex school effect using teacher and student survey data. I find that, compared to teachers in coed schools, teachers in girls- or boys-only schools are significantly more likely to use teacher-guided instructions (i.e., children received instruction with examples and explanations). They are also more likely to report that the school employs and enforces clear rules of conduct. As I do not find significant differences among predetermined teacher characteristics, I interpret these results as teachers reacting differently to different gender composition environments. I then link lottery participants to survey data on students' school experiences, such as academic self-perceptions and bullying. I find that students who were drafted to attended a single-sex school report higher levels of school satisfaction and teacher-student relationship than those who were not.

This paper connects with a broader literature on school gender composition. Some studies exploiting the natural variation in the share of female students in adjacent cohorts find that both male and female students perform better in predominately female classes (e.g., Hoxby, 2000; Lavy and Schlosser, 2011) but that a larger proportion of female peers increases the gender gap in STEM entry (Brenoe and Zoelitz, 2018).[3] Other studies based on randomized experiments are less conclusive (Whitmore, 2005) or find that gender homogeneity in classroom groups, measured by the number of surrounding desk mates of the same gender, is beneficial for both male and female students (Lu and Anderson, 2015). Nevertheless, there may be substantial differences between a learning environment with a high share of female

[3]Interestingly, Lavy and Schlosser (2011) further show that the positive effect is consistent with the "bad apple" theory of Lazear (2001). As girls are less likely to exhibit disruptive behavior, having a higher proportion of girls in the classroom improves discipline and the teacher-student relationship.

students and one with only female students.

This study is most closely related to those evaluating the impact of single-sex education, which has become an active area of research in economics in recent years. Empirical studies exploiting the random or quasi-experimental assignment of students to coed and single-sex high schools (or classes) suggest positive effects of single-sex education for academic performance and educational achievement (Jackson, 2012; Park, Behrman, and Choi, 2013; Lee et al., 2014; Choi, Moon, and Ridder, 2014; Eisenkopf et al., 2015; Dustmann, Ku, and Kwak, 2018; Booth, Cardona-Sosa, and Nolen, 2018; Jackson, 2019).[4] However, despite the increase in academic performance, single-sex schooling has not been found to increase the likelihood that female students choose STEM programs (Jackson, 2012; Park, Behrman, and Choi, 2018). None of these studies evaluates the effect of single-sex schooling in childhood. The empirical evidence on single-sex education at the primary school level is almost non-existent, with the exception of Doris, O'Neill, and Sweetman (2013), who compare 9-years-old students' performance in math across different school types in Ireland, addressing selection bias by controlling for pupil and teacher characteristics. They find suggestive evidence that boys perform better in single-sex schools, but there is no such evidence for girls.

This paper also contributes to the literature that uses lotteries in school contexts. Admission lotteries have been used to study the impact of attending high-performing schools (e.g., Rouse, 1998; Angrist et al., 2002; Abdulkadiroğlu, Pathak, and Walters, 2018) and charter schools (e.g., Hoxby and Murarka, 2008; Angrist et al., 2010; 2012; Dobbie and Fryer, 2015). Many of these studies use school-specific lotteries for a small share of slots after distance-based priority slots are secured for local students. This paper is different in a number of important dimensions. First, I leverage centralized admission lotteries (with no distance priority) for schools that serve 30 percent of the country's student population. Second, I work with lotteries for the whole island of Malta so my analysis is not affected by students moving out of the district, which is a typical problem in many lottery-based studies (where different assumptions produce conflicting estimates). Third, an additional characteristic of the Maltese setting is that students in Catholic schools are comparable to the national student population,

[4]Half of these studies (Park, Behrman, and Choi, 2013; Lee et al., 2014; Choi, Moon, and Ridder, 2014; Dustmann, Ku, and Kwak, 2018) are based on Seoul, South Korea, because students are randomly allocated across high schools within school districts. The details of the assignment rule are kept confidential, and there is suggestive evidence that the student allocation actually follows a distance-based rule (Sohn, 2016). In addition to the potential sorting of parents into school districts, this may also add selection bias through the endogenous sorting of parents into residential neighborhoods within each district.

making the extrapolation of the results less of an issue. This is because at least 40% of the population participates in each lottery and applicants come from everywhere on the island.[5] Finally, the single-sex schools analyzed here are particularly relevant for the country's accountability mandate because these schools are state-funded and part of the mainstream education system.

The remainder of the paper is organized as follows. Section 2 describes in detail the institutional background and the admission lottery system used by Catholic schools. Section 3 outlines the data sources and the sample, while Section 4 provides summary statistics about the lottery applicants and the schools. Section 5 outlines the lottery-based estimation framework and presents the empirical results on end-of-primary-school test scores and the choice of subjects in secondary school. Section 6 provides evidence on the underlying mechanisms, and Section 7 concludes the paper.

2. Institutional Setting

2.1. Maltese Education System

Due to its colonial inheritance, Malta education and examination systems closely follow the British model.[6] Compulsory education covers two levels, primary and secondary education, and students take national and externally graded exams at the end of each stage. The primary school cycle runs from Year 1 to Year 6 (ages 5-11). The secondary school cycle is five years in duration, running from Year 7 to Year 11 (ages 12-16). Bilingualism is considered the basis of the educational system. Maltese and English, the two official languages, are taught at all levels within compulsory education. Although kindergarten is not mandatory, attendance is de facto universal.[7] Entry to each education level is on a birth-year basis, meaning that enrollment in Year 1 is determined by the calendar year in which a child turns 5 years old. The school year runs from late September to mid-June, and there are three mainstream education providers: the government (state schools), church (Catholic

[5]Note that admission lotteries do not necessarily draw applicants from the general student population, and there is no reason to assume that those who sign up for a school slot are coming from a random process. Thus, comparing lottery winners and losers in this and any other lottery-based studies may reveal little about the effect of schools on non-participants. However, extrapolation of the results is less of an issue if the number of participants is quite high, as in the Maltese case.

[6]Malta was a British colony for 150 years, gaining independence in 1964. It is the smallest country in the European Union by both population and area and the most densely populated.

[7]Participation rates in 2014 were 95.4 percent and 97.7 percent for kindergarten I and kindergarten II, respectively (OECD Stats EU-SILC, 2014).

schools) and independent (private schools) sectors.

Most of the students in Malta attend schools either from the state or the Catholic sector. Independent schools are an expensive alternative, so Catholic schools are mainly attended by students that would otherwise attend their local state school.[8] As of 2015, the distribution of students across primary schools was 56% in state-run schools, 31% in Catholic schools and 13% in independent schools. Nearly every town and village in Malta has its own primary state school, while Catholic schools are mainly concentrated in the Northern and Southern Harbour districts (Figure 1).

Another distinct characteristic of the primary schools in the Catholic sector is that they are of different types: 4 schools are coed, 10 are girls-only schools and 7 are boys-only schools. All state and private schools are coeducational.[9] When comparing the quality of schools across types, single-sex schools are noteworthy. Figure 2 plots estimates of a proxy for school quality for single-sex and coed schools from a regression of students' end-of-primary-school test score on a full set of year and school fixed effects. While students at coed schools perform below the national average, those at single-sex schools are disproportionately on the right side of the distribution.

While admission to primary state schools is based on a student's home address,[10] Catholic schools use a centralized lottery to ration seats. Since the 1990s, Malta has introduced several measures and educational reforms to democratize access and increase parental school choices.[11] 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 (which establish standards of hygiene, safety and classroom dimensions and amenities), and became funded by the national government.[12] The agreement also prohibited Catholic schools from selecting students on faith grounds or any other trait, so the Secretariat for Catholic Education (hereafter Secretariat), the central

[8]The average tuition fee in private schools was approximately 3600 euros annually (14 percent of the average household disposable income) in the 2014-2015 school year according to Malta Today (2015).

[9]Until recently, except for the independent sector, the system of secondary education in Malta was single-sex. Since 2014, state-run secondary schools have gradually transitioned to mixed-gender schools, while Catholic schools remain gender-homogeneous.

[10]Principals may accept students from a different town if the parents request it and the resources of the school allow it, although according to conversations with the staff at the Ministry of Labor and Education, these are exceptional cases.

[11]See Cutajar (2007) for an overview of educational reforms in Malta, especially after independence.

[12]The government fully funds the budgeted salaries of the staff and gives an 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.

office of Church schools, adopted a lottery system in order to deal with student admissions.[13]

2.2. Catholic Schools Admission Lottery

In January of each year, parents submit an application on a paper form they obtain from the Secretariat or any Catholic school. In the application, parents need to provide basic demographic information and a ranking of schools. The rank-order list (ROL) of schools is unrestricted, which means that parents can rank as many schools as they wish of those available for a particular intake grade and year. There is no requirement to participate in the lottery, that is, parents do not need to satisfy any criteria to make their child a candidate, and it is costless.[14] Lotteries are held around April/May, and there is one admission lottery per traditional intake grade, which are kindergarten and Year 1.[15]

Different schools differ in the school grade in which they begin admitting students. For example, most coed Catholic schools begin serving students in Kindergarten 1 (KG1), while all single-sex schools do so from Kindergarten 2 (KG2) or Year 1. However, this does not mean that coed schools participate exclusively in the KG1 lottery. Class sizes are capped at 15 and 20 for KG1 and KG2, respectively, and to 30 students for Year 1 onwards. Consequently, coed schools participate in each grade lottery, filling classrooms (and single-sex schools with traditional intake grade in KG2 also participate in Year 1 lotteries following the same logic).

Student matching mechanism.—The Secretariat uses a random serial dictatorship (RSD) mechanism to match students to schools. This is 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. Then, assignment to schools proceeds as follows: the first student in the queue obtains her most preferred school as stated on the application form, the subsequent student obtains her top choice among schools with slots remaining, and so on until no seat remains at any school. Although boys and girls are placed in the same queue, the number of available slots in each school are set by gender in advance. Obviously,

[13]The island of Gozo, the second-largest island of the archipelago with 8 percent of the Maltese population, implements a separate admission lottery for the 4 Catholic primary schools on the island. Gozitan lotteries are not part of the sample used in this paper.

[14]There was no charge for applying up to 2010. Administrative costs of approximately 10 euros were introduced in 2011.

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

there is no decision about the student gender in single-sex schools, and coeducational schools divide slots evenly. This means that although there is a unique lottery draw for all applicants (in a particular year for a particular grade), the assignment mechanism can be understood as two separate assignment mechanisms, one for each gender.

The allocation mechanism was slightly modified in 2011. Instead of having to submit a rank order list of schools with the application, parents must attend a public event where the final allocation of schools is decided. At this public event, parents are sequentially called to select a school following the order determined by their lottery number. After each decision, the corresponding slot is not available for the subsequent candidate. Despite the change in design, the final allocation of students to schools is identical to the one that the mechanism used before 2011 would have generated.[16]

RSD is the easiest mechanism to implement in an allocation problem and is strategy-proof. By only using preferences over school 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) because 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, while the rest of the vacancies are filled by the single lottery number as described above.

3. Data

This section provides an overview of each data source and the criteria used to select the sample. Appendix A presents the survey questions and additional details on

[16]The main difference in terms of data is that, with the change, I cannot observe parents' full sets of preferences over schools.

variable definition.

The first dataset is the lottery data and combines administrative data with information extracted from the lottery application forms.[17] The application form data contain a student's full name, gender, date of birth, parent's name, father or mother's ID, home address, sibling relation, SEN status and the preference order of schools. The administrative data contain records of school capacity, the student priority grouping, lottery numbers and lottery outcomes (i.e., identifying which applicants were the successful candidates and the school to which they were matched).

I exclude from the lottery data applicants who were granted access through priority (e.g., those who secured a slot because they had an older sibling already enrolled). Therefore, I keep only those in the marginal priority group, the applicants for whom lottery numbers alone determine whether they receive an offer (randomized applicants). As applicants with SEN were also randomized, they are part of the analysis sample.[18,19]

I use KG2 and Year 1 lotteries, which are the intake grades of single-sex schools, for the cohort of applicants born in 2005 and 2006. Additionally, I include a Year 4 lottery used to fill three new boys-only schools. This exceptional lottery only affected the 2005 cohort.[20] Admission lotteries are competitive. Every year and for every intake grade, there are approximately 1000 applicants, and on average, one out of four receives a single-sex offer. This can be seen in Table 1, which reports information about the year of the lottery, the number of applicants (total and randomized), and the share that received an offer to attend a single-sex Catholic school.

The lottery data allow me to follow the cohorts born in 2005 and 2006 at every year in which they could participate in an admission lottery. For example, if a child does not receive a lottery offer in KG2 and reapplies the following year to the Year 1 lottery, then I observe lottery participation and lottery outcomes in both instances.

[17]Data collection for this paper involved the scanning and digitization of individual application forms (Appendix Figure A1). The preservation of the lottery data was not a priority after the admission process was completed, which prevents me from obtaining information before 2008 and for the 2009 KG2 lottery.

[18]SEN lotteries are included except for those with no losers (i.e., degenerate lotteries). Lotteries held for students with SEN are small, especially for girls, and except for one, there is no variation in the instrument because all the girls received an offer.

[19]The sample should also be restricted to applicants to the relevant sector, i.e., those who applied to single-sex schools by ranking them anywhere in the application form, and exclude those that only ranked coed schools. This is of little concern here since, among the randomized applicants, only 10 parents exclusively chose coed schools in their submitted ROL of schools.

[20]Three of the seven boys-only schools opened in 2011, with rolling enrollment in Year 1 and Year 4. To quickly fill the schools, lotteries were held for Year 1 and Year 4 for 3 consecutive years starting in 2011. The last Year 4 lottery was held in 2013, and everyone born in 2005 was eligible to apply.

As will be discussed in the next section, observing the full application patterns of students is important for the identification strategy.

I link the lottery data to the End of Primary Benchmark (Benchmark) exam data, a test administered on a national basis by the Department of Curriculum Management. The Benchmark is a standardized examination for Year 6 students at the end of their primary cycle. The assessment measures speaking, listening, reading comprehension and writing skills in English and Maltese and mental and written skills in mathematics. The test scores were standardized by subject and year to have mean zero and unit variance among all test-takers. The overall match rate between randomized students and the Benchmark is 82 percent. Several reasons explain not achieving a perfect match, which are described in detail in Appendix B. I assess the quality of the matching procedure by testing whether the match rate differs by lottery status. The results of this exercise suggest that lottery winners are not more likely to be matched than lottery losers (0.002, SE = 0.015).

I also collected information on students' choice of curriculum track that covers the last three years of secondary education. At the age of thirteen, Year 8 students 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 and hospitality, which are prevalently female courses, to chemistry and physical education, which are prevalently male courses. In practice, this curriculum track largely affects the subsequent choice of college major (Calleja, 2008).[21] A number of characteristics of Catholic secondary education guarantee that students face a similar decision among the set of elective subjects independent of whether they attended a single-sex or coed primary school. 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 E describes in greater detail this setting and the transition from primary to secondary school within the Catholic sector.

Finally, I use 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, in addition to some student and teacher background

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

data, information about the home literacy environment, the school curriculum and curriculum implementation and instructional practices. The target population in Malta consists of students enrolled in Year 5 of compulsory primary education in 2016, a year before the Benchmark examination.[22] The PIRLS sample comprised the whole population of 10-year-olds in 2016, which corresponds to the 2006 cohort in the lottery data. I match the two datasets using students' full names and dates of birth.[23]

4. Applicants and School Characteristics

Applicant Representativeness.—Given the spatial concentration of Catholic schools on the island, it is possible that these schools serve students who are disproportionately located in surrounding neighborhoods. I find that this is not the case as applicants come from everywhere in the island. To show this, I link each applicant address to the corresponding census locality and compute the number of applicants per locality in the KG2 and Year 1 lotteries held in 2010. Then, I compare the number of applicants per locality with the census population counts of children of corresponding ages (4 and 5, respectively). Figure 3 shows the results of this exercise. Panel A shows that the KG2 lottery contains 40 percent of the 4-year-old population, while Panel B shows that the Year 1 lottery contains 28 percent of the 5-year-old population.[24] The applicant data appear highly representative, as the census population can explain 84 percent and 77 percent of the variation in the applicant population for each intake grade. Furthermore, the figure shows that this relationship is well approximated by a linear function. This implies that, on average, localities are proportionally represented among the pool of applicants.[25]

Parents' Ranking of Schools.—No Catholic school is undersubscribed. When submitting the ROL, 50 percent of the applicants rank all feasible schools. Among

[22]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, so the Year 5 test better matches the assessment to the achievement level of students.

[23]Access to student date of birth was subject to approval by the IEA Amsterdam. Students' surnames and names were retrieved from the Benchmark.

[24]Using applicants participating in lotteries held in other calendar years yields quite similar results.

[25]Appendix Table C1 also shows that the applicant population (independent of the lottery outcome) is also equally distributed across the four quarters of birth, as is the distribution of the general population. There is also no difference between the shares of applicants and the general population that have a popular surname.

those that rank a smaller number of schools, parents typically rate 6 out of 10 schools. The distribution of applicants across the proportion of schools ranked is shown in Figure 4. The ranking of schools also reveals that parents prefer, in general, single-sex over coed schools, and this holds independent of the applicant's gender. Figure 5 shows the average ranking per school for girls and boys separately. Note that the lower the value , the more preferred a school is. Preferences play a central role in lottery-based studies in isolating the random components of the data-generating mechanism. This is discussed further when the empirical strategy is presented in Section 5.

Balance.—Table 2 presents summary statistics and tests for balance among lottery winners and losers.[26] To formally test for the quality of the lotteries, I regress different predetermined variables on a dummy variable indicating whether the applicant won the lottery, where winning the lottery means that the applicant is offered a slot in a single-sex school. All regressions control for lottery fixed effects (the interaction of year and grade of application, SEN status and gender) to exploit the within-lottery randomness only. Point estimates and standard errors of this test are reported in column 3. Lottery winners and losers are similar on a range of predetermined observable characteristics. Note that differences are not only statistically insignificant but also substantively small in magnitude. The F-statistic fails to reject the null that the covariates, taken as a whole, are significant (p-value 0.775).[27]

School Inputs.—Although the Catholic sector is part of the mainstream public education system in Malta, there are a number of dimensions on which Catholic schools differ from state-run schools. The former are privately managed, have greater discretion over the hiring and dismissal of teachers, school texts and pedagogical approaches, and may extend activities beyond the core curriculum. There are also substantial differences in average class sizes. Because there is practically one state primary school per locality, the student population at some of those schools is relatively low, potentially leading to greater individualized instruction, while Catholic

[26]This balance table (and those included in the appendix) excludes the 2009 KG2 sample, as the application form was not found.

[27]Balance regressions were also performed for the single-sex offer within the sample of lottery winners for a Catholic school slot and for the Catholic offer. The results, reported in Appendix Table C2, are similar to those described here. There are also no significant differences between randomized and non-randomized applicants. If students 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 comparison of students who received admission to single-sex schools through priority (i.e., whose lottery numbers had no impact on admission) and those entering because they were randomized are reported in Appendix Table C3.

schools operate under full capacity.[28,29]

On the contrary, for historical and institutional reasons, the Catholic sector seems to comprise a more homogeneous set of schools. I use rich administrative data on teachers and school characteristics to test this empirically. Columns 1 and 2 of Table 3 report mean values of school staff characteristics (Panel A), teacher characteristics (Panel B) and other school-level characteristics (Panel C) for coed and single-sex Catholic schools, respectively. Column 3, which reports the difference in mean covariate values, shows that coed and single-sex schools are statistically indistinguishable. For example, teachers in both school types are predominantly female (96%), ruling out the student-teacher gender effect widely explored in the educational literature.[30] There is no difference in teacher age (∼37 years old), qualifications (∼83% report having a bachelor's degree) or years of experience (∼10 years). There is also no difference in teacher wages, as one would expect given that they are determined primarily by teacher's years of experience and education level.

I also find statistically indistinguishable school-level characteristics between single-sex and coed Catholic schools. There is no difference in the total number of hours per year of instruction or in the number of hours per week dedicated to each subject (∼5 hours per week for English, Maltese and math). Single-sex and coeducational Catholic schools do not seem to differ in terms of geographical location, which may trigger differential access to resources. Finally, there is no difference in class size (∼25.5), which is consistent with the information that Catholic schools operate at full capacity.[31] Appendix Table C4 shows the single-sex school breakdown

[28]The average class size at state primary schools is 17.1 (SD 3.69) students, well below the established regulatory threshold of 30 (or 26 in cases where there is at least one student with SEN). In Catholic schools, the average size of the class is 25.4 (SD 0.87) students.

[29]Surprisingly, religious instruction seems not to be the overriding factor that differentiates these two school sectors. The Roman Catholic religion is taught in all state-run schools and for the same number of hours per week as in the Catholic schools. Parents do have the right to opt-out, but very few choose to do so (2% and 6% during the 2008-2009 and 2015-2016 academic years, respectively).

[30]For example, Bettinger and Long (2005); Dee (2007); Paredes (2014); Antecol, Eren, and Ozbeklik (2015); and Lim and Meer (2017). A potential concern when evaluating the effect of single-sex education is that the high proportion of same-gender teachers in girls-only and boys-only schools may confound the interpretation of the results. Same-gender teachers may provide better role models, especially for girls, and may have some advantages in managing student discipline and classroom order, especially for boys; thus, the single-sex school effect may simply reflect teacher-student gender matches. Park, Behrman, and Choi (2018) and Lee et al. (2014) address this issue for students in single-sex schools in Korea. Park, Behrman, and Choi (2018) find that gender matching between students and math teachers is significantly related to test scores and STEM outcomes for male students in boys-only schools. They do not find an effect for teachers in other subjects or for female students in girls-only schools. Lee et al. (2014) also find little evidence that the boys-only school effect varies by the gender composition of the teachers.

[31]The maximum number of students allowed in each primary class is thirty. This number is reduced by two for each student with a statement of needs in the class. The average class size of 26

for girls-only and boys-only Catholic schools.

5. The Effect of Single-Sex Primary Schools

5.1. Identification Strategy

To identify the effect of attending a single-sex primary school, I exploit the random allocation of students to single-sex and coeducational schools within the Catholic sector. The identification strategy relies on two facts. First, the lottery numbers generate exogenous variation in the type of school offered to the student. In practice, this allocation is exogenous when comparing applicants who face the same risk of assignment to a single-sex school. Second, as discussed in the previous section, single-sex and coed schools Catholic schools are similar along many observable dimensions. Thus, differences in student performance are mainly attributable to the homogeneous gender composition rather than other characteristics of the schools. The estimating equation takes the following form:

$$y_i = \beta_1 SingleSex_i + \beta_2 Catholic_i + \gamma' R_i + \delta' X_i + \phi_{t(i)} + \varepsilon_i, \tag{1}$$

where y_i is the academic outcome of student i. The term $SingleSex_i$ is an indicator equal to 1 if the student attends a single-sex primary school and zero otherwise.[32] The term $Catholic_i$ is an indicator equal to one if the student attends a Catholic school (either single sex or coed) and zero otherwise. The vector X_i represents a set of applicants' baseline covariates (such as locality) that, although not necessary for identification, are included to increase precision. The dummies R_i, described below, are indicators for lottery-specific *risk sets*. The term $\phi_{t(i)}$ represents time fixed effects that capture shocks in the test year that may affect all students, while ϵ_i is the error term. I interpret equation (1) as describing the average achievement that would be revealed by assignment to single-sex education in an experiment that holds Catholic education fixed. Our coefficient of interest is β_1.

Ordinary least squares (OLS) estimates of equation (1) would capture the average causal effect of single-sex schooling under full compliance. In practice, administrative assignment may fail to conform fully to a randomized design because some students may not comply with the treatment that the lottery assigns to them.

students is consistent with the regulation that allows only two SEN students per class.

[32] Given that all single-sex schools are Catholic, this term is equivalent to the interaction between a Catholic indicator and the single-sex indicator. For brevity, I use the simplified notation.

For example, students who are offered a single-sex school slot may decide to deviate from assignment and attend their local state school. Analogously, students not offered a slot may manage to secure admission at the discretion of the principals or by gaining sibling priority when a younger sibling wins a lottery. As this would potentially bias OLS estimates, the variables $SingleSex_i$ and $Catholic_i$ are instrumented with lottery offers. The excluded instruments are Z_i^{ss} and Z_i, which represent single-sex and Catholic offer indicators, respectively.[33]

Risk sets.—To operationalize the identification strategy, it is necessary to accommodate reapplications and parental preferences over schools. Regarding reapplication patterns, consider, for example, the basic setting faced by the cohort of students born in 2005. There are three lottery participation possibilities. They may participate only in the KG2 lottery; they may participate in the Year 1 lottery only; or they may participate in both the KG2 and Year 1 lotteries (in two consecutive years). Therefore, these participation patterns define three possible groups. The same applies for the cohort of students born in 2006. Because parental characteristics may differ with respect to this application pattern, I solve the selection controlling for group membership, meaning that the identification comes from lottery-induced variation within groups.[34] This is in the spirit of the self-revelation approach of Dale and Krueger (2002) and standard in the lottery-based literature.

On the other hand, wining-losing probabilities are not independent of revealed preferences, and one would ideally 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 could focus on offers at a 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 latter 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

[33] In practice, given that each lottery in which a student participates is a valid instrument for single-sex schooling, I leverage here all the lotteries in which each cohort participates. Thus, Z_i^{ss} and Z_i correspond to a set of indicators for lottery offers at different grades. I examine the robustness of the results to defining the instruments as the outcome of the first lottery only.

[34] Parents seems to be persistent in attempting to obtain places at Catholic schools, with approximately 70 percent of rejected first-time applicants reapplying a second time. I empirically investigate the presence of observable differences in parental characteristics across applicants with different application patterns and find no statistically significant difference. This is shown in Appendix Figure D1. Because the baseline data are limited, I remain agnostic about the possibility that unobserved characteristics may differ significantly and report results adjusted for student application patterns.

obvious dimensionality constraint given by the extremely large number of school-rank combinations one would find in the data. To address this issue, one can impose ad hoc solutions of the type conditioning on the three top choices or, alternatively, control for the simulated conditional probability of getting admission (propensity score), as proposed by Abdulkadiroğlu et al. (2017).[35] This method has two advantages: it attenuates the dimensionality problem and maximizes the number of individuals that contribute 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 holding constant applicant 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. The drawback is that I lose observations given the changes in the lottery system from 2011 onward, when the Secretariat ceased asking parents for the ROL of schools.

5.2. Impact on End-of-Primary-School Test Scores

Table 4 shows the estimated single-sex school effect on English, Maltese and math test scores. I report results using all the lotteries affecting the 2005 and 2006 cohorts (columns 1-3) and for the subset of lotteries with parents' preferences over schools (columns 4-6). The first-stage results (columns 1 and 4) indicate that, on average, receiving a singe-sex offer increases the probability of attending a single-sex school by at least 60 percent. The reduced-form estimates, reported in columns 2 and 5, indicate that being offered a single-sex slot is associated with an increase in test scores in all subjects. The 2SLS estimates, reported in columns 3 and 6, reveal that students at single-sex schools perform significantly better than those at coed schools in all subjects. The point estimates are largely consistent across the individual subjects, on the order of 0.63σ for English, 0.74σ for Maltese and 0.55σ for math, when conditioning on propensity score.[36]

[35] 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 the propensity score is applicable to any centralized mechanism satisfying the equal treatment of equals (ETE) property.

[36] Further results (not reported) show that students in coed Catholic schools performed worse (relative to students in coed state schools) in all of the core subjects. The point estimates for *Catholic* are consistently negative (and significant in some cases). This is a puzzling result and difficult to reconcile with the fact that all Catholic schools are highly demanded every year. One possible explanation could be related to the smaller and more manageable class sizes at state schools. Another possible explanation is that parents with a lucky lottery number for a Catholic slot relax after securing admission, while parents of applicants that did not manage to secure a Catholic

To put these results in perspective, the annual equivalent of these overall effects is in the range of 0.07 to 0.1 standard deviations. This is four to five times higher than the "per-pupil" effect found in class size studies (i.e., the effect of reducing the class size by one student) but less than half of the charter school effect. These findings are consistent with the results documented in the literature on single-sex education for students at higher educational levels of about $0.15 - 0.20\sigma$ after 3 years of attending a single-sex middle or high school (Jackson, 2019; Lee et al., 2014; Dustmann, Ku, and Kwak, 2018).

The results described above leverage all lottery outcomes. Some studies that also address a high reapplication level use only the outcome of the first lottery in which an individual participates when estimating the treatment effect (see, for example, Angrist et al., 2010; Ketel et al., 2016). As a robustness test, I conduct the same analysis using as an instrument the outcome of the first lottery alone. The results, reported in Table 5, are statistically indistinguishable from the main results, except for the math outcome, which is not significant.

5.3. Response Heterogeneity

Table 6 explores the heterogeneity in the single-sex school effect across student gender. I find that both boys and girls experience meaningful test score gains from attending a single-sex school. Column 5 shows that the effects for girls are statistically significant for the language subjects, with a size of approximately 0.40σ for English and 0.53σ for Maltese. Column 6 reports the results for boys, which are all statistically significant. Overall, the impacts for boys appear to be larger than those for girls in the language subjects and smaller than those of girls in math. Overall, these findings show that single-sex education at childhood improves student performance, particularly for those subjects in which girls and boys do not traditionally have a comparative advantage. In addition, the finding that single-sex education benefits both boys and girls contrasts with recent studies evaluating single-sex schools or classrooms at high school or university. For example, Jackson, 2012; and Booth, Cardona-Sosa, and Nolen, 2018 find significant effects only for female students, while Lee et al., 2014 and Jackson, 2019 find evidence only for male students.

Given that the assignment mechanism used by Catholic schools is strategy-proof

slot compensate by contributing more effort. Cullen, Jacob, and Levitt (2006) provides empirical evidence on the substitutability between parental involvement and school quality in the education production function at Chicago Public Schools.

(i.e., applicants' ROLs of schools measure parents' true preferences), I next examine treatment effect heterogeneity along preferences over single-sex schools. As explained in Subsection 2.2, parents are asked to rank schools in order of preference from their first to their last choice. They are not constrained in the number of schools they can rank, and, on average, they rank 60 percent of the schools in their choice set. Multiple-ranked responses are superior to single (first) choice because, in the latter 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. With ROLs, one can gather more information because the same individual provides multiple outcomes by removing the prior chosen school from the subsequent choice set. In other words, if parents systematically choose schools that share a common attribute, one can infer that there is a strong preference for that attribute.[37]

I estimate a mixed rank-ordered logit model on parental rankings of school data to infer the intensity of preferences over school attributes (McFadden and Train, 2000).[38] In particular, I estimate for each applicant i the weight ($\hat{\theta}_i$) his or her parents place on the single-sex attribute when choosing schools. The fact that this parameter depends only on baseline data and is independent of the lottery outcome motivates the following second-stage equation:

$$y_i = \gamma_1 SingleSex_i + \gamma_2(SingleSex_i\hat{\theta}_i) + \gamma_3 Catholic_i + \gamma_4' R_i + \gamma_5' X_i + \phi_{t(i)} + \mu_i. \quad (2)$$

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

The results of this test are shown in Figure 6. Although the effects are imprecisely estimated, they are informative of the heterogeneous treatment effect by preference

[37] Empirical studies on students' heterogeneous preferences over school attributes based on a demand system approach can be found in Hastings, Kane, and Staiger (2009) and Abdulkadiroğlu, Agarwal, and Pathak (2017).

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

for single-sex schooling. As expected, the effects are weakly increasing with $\hat{\theta}_i$, especially for the language subjects. However, note that the highest quartile seems not to drive the results in any of the test scores.

5.4. Impact on Elective Subjects

Table 7 presents 2SLS estimates of the effects of single-sex schooling on the probability of taking at least one science subject, the probability that both subject options taken are science, the probability of taking at least one prevalently female subject, and the probability of taking at least one vocational subject. These estimates follow the same lottery-based specification in equation (1) using all randomized applicants born in 2005 and 2006. Column 1 shows the result for the whole sample, while columns 2 and 3 break down the overall effect by gender.[39] I present here results controlling for application pattern (as in the first three columns in Table 6).[40]

The results are consistent with the idea that single-sex education renders gender less salient, and thus students' choice of subjects is less gender-typical. Girls who attended a girls-only primary school are 21 percent more likely to choose all science subjects than girls coming from coed Catholic schools (column 2). Similar point estimates, but in the opposite direction, apply for the probability of taking at least one prevalently female subject, although they are imprecisely estimated. Boys are 11 percent less likely to choose at least one science course than boys coming from a coed primary school column 3). Finally, both female and male students are less likely to choose vocational subjects as electives. Given that academic courses better match university entrance requirements more vocational courses, these results suggest that attending a single-sex school increases the odds of boys and girls entering university.

The effect of single-sex education on (less) gendered course-taking documented here marks a departure from other studies that found no effect of single-sex schooling on STEM course selection (Jackson, 2012) or career choices (Park, Behrman, and Choi, 2018) for female students. One possible explanation for these null effects may be related to the timing of exposure to a gender-homogeneous learning environment. While Jackson (2012) and Park, Behrman, and Choi (2018) consider students assigned to different school types in secondary school, here I examine students exposed to single-sex education in childhood and are thus, arguably, more likely to be influenced by the environment.

[39] Instead of running separate regressions by gender as I did with test scores, I fully interact the right-hand side variables with a female indicator.

[40] Data collection is still in process and using only applicants in lotteries with preferences will produce relatively small samples. Hence, the results must be interpreted with caution.

A possible concern about the single-sex effect on later academic outcomes is that the characteristics of the secondary schools students attended could be correlated with the type of school attended at the primary level. This raises the question of whether the observed differences among single-sex and coed graduates are attributable to other factors, such as facing different curriculum alternatives. However, this is much less of a concern in this setting. Admission to Catholic secondary schools is ultimately determined by lotteries, as in the case of primary schools. In addition, all schools provide a broad but very similar subject choice set that includes both vocational and more academically oriented courses. Appendix E provides additional information regarding elective subjects and the transition from primary to secondary school within the Catholic sector.

6. Mechanisms of Single-Sex Schools

In this section, I use student and teacher survey data to study the underlying mechanisms that led to the substantial impact of single-sex education on the academic achievement of boys and girls. The educational literature notes that the single-sex school effect operates through direct and indirect channels. On the one hand, gender peer effects may be at work as a result of the student interactions in the classroom (e.g., peer discipline or bullying incidents, self-concept of ability). On the other hand, there may be changes in teacher behavior in single-sex schools. These indirect channels refer to the gender alignment in teachers' pedagogical practices, the discipline methods used, and the ability level to which the class is pitched (Jackson, 2019). Although the survey data do not make it possible to fully explore and measure all the mediating factors behind the positive effects of single-sex education, they can provide important insights.

I evaluate teacher and student responses to school type using PIRLS 2016 questionnaires. The teacher survey covers Year 5 teachers only and not the whole school staff. The student survey covers the 2006 student cohort. Hence, the results of this analysis should be interpreted with some caution because small sample sizes may lead to rather imprecise estimates. The estimates presented here are based on the main specification, so all comparisons are made among teachers (students) in single-sex and coed 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' (students') answers from coeducational schools. With the exception of absenteeism, all other measures are standardized to have mean zero and standard deviation one. Appendix A provides a detailed description of how the

outcomes examined in this section were constructed.

Teachers.—Teachers at single-sex schools show no systematic difference in their level of satisfaction with the profession and work from teachers at coed schools, but they do report a better quality of the teacher-student relationship. 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 teacher-student relationship is based on answers to the item "The students are respectful of the teachers". The results are shown in Panel A of Table 8. The first column reports estimates of the effect of girls-only schools on teacher responses, while the second column reports estimates for boys-only schools. The effects on teacher satisfaction in both school types are large and negative but not statistically significant. There are, however, differences in the quality of the relationship between students and teachers. Teachers working in girls-only schools are more likely to report that students are respectful to them. The estimates for teachers working in boys-only schools are also positive but not significantly different from zero.

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, although this is imprecisely estimated. The survey does not allow to differentiate whether there are different discipline methods across school types, but teachers at girls-only schools are more likely to report that the school employs and enforces clear rules of conduct. The difference between teachers in boys-only schools and those in coeducational schools is not statistically significant. These results on classroom disruption and school discipline are reported in Panel B of Table 8. Classroom climate is captured by teacher reports on general student behavior ("The students behave in an orderly manner") or if teachers report that disruptive students limit their teaching ability. School discipline is measured by two dummy variables related to a teacher's 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".

Instructional practices in both girls- and boys-only schools seem to be more teacher-guided, meaning that teachers are less likely to leave students to work independently on an assigned plan or goal. However, there is no evidence that teachers at single-sex schools aligned their pedagogical practices with the prevalent gender in the classroom (engaging students' interests, using multiple perspectives or linking content to prior knowledge). Measures of teacher instructional practices are shown in Panel C of Table 8. It also shows that teachers in girls-only schools are

more likely to report that they use individualized methods of teaching than are their counterparts at coed schools, although this finding is not statistically significant. In contrast, teachers at boys-only schools are marginally less likely to report giving individualized feedback and instructions. Finally, in both single-sex school types, teachers report less use of ability-grouping methods for teaching (tracking). Although none of the effects is statistically significant, the negative pattern is consistent with the argument that gender-homogeneous environments should produce less variation in student abilities within a given class.

Complementary to the school environment and teachers' instructional practices is the effect of single-sex schooling on the school's emphasis on academic success. Teachers at girls-only schools reported significantly lower curriculum expertise, while there is no such evidence for teachers at boys-only schools, as shown in Panel D of Table 8. Curriculum expertise is an index derived from how teachers characterized understanding the school's curricular goals and their degree of success in implementing the school's curriculum. Teachers were also surveyed about their students' desire to do well in school and their ability to reach academic goals. The estimates are small and insignificant, indicating that teachers' perceptions of students' performance are comparable across school types.[41]

School principals were also surveyed about the quality of the teacher-student relationship, classroom environment and the school's emphasis on academic success. Estimates using the sample of principals confirm the main finding described in this section using the teacher survey. Overall, single-sex schools provide a better teacher-student relationship and classroom climate. The results using the principal survey data are reported in Appendix Table F2.

Students.—Student satisfaction with the school does not seem to differ according to the type of school attended. However, single-sex schooling does appear to impact other measures of student (dis)satisfaction, such as truancy and bullying. This is shown in Panel A of Table 9. The student satisfaction index is derived from three survey questions that ask students how much they agree with the following statements: "I like being in school", "I feel like I belong at this school", and "I am proud to go to this school". Both of the coefficients are positive and much larger for girls than for boys but not statistically significant. Regarding student absenteeism, girls in girls-only schools are 27 percent less likely to be absent from school than girls

[41] If I exclude from the sample teachers working in the 3 schools that do not participate in the Benchmark and thus do not contribute to the estimates on test scores, I obtain similar findings to those presented here. Appendix Table F1 shows the results of this exercise.

at coed schools. The point estimates for boys are much smaller (2 percent) and not statistically significant. The estimates also reveal that boys-only schools lead to a significant reduction in bullying incidents relative to coed schools.

Boys in single-sex schools are less likely to report that teachers are fair to them, as can be seen in Panel B of Table 9. This is striking, given the higher quality of the teacher-student interaction reported by the teachers and the higher level of school satisfaction reported by the students. The point estimate for girls is positive but imprecisely estimated.

Consistent with the findings described above using teacher responses, estimates using student survey data show no evidence of a greater alignment of instruction at single-sex schools than at coed schools. This is shown in Panel C of Table 9. The measure of alignment of pedagogical practices is derived using students' answers to the following statements: "My teacher gives me interesting things to read", "My teacher is easy to understand", and "My teacher does a variety of things to help us learn". The point estimates are positive for girls and negative for boys and, in both cases, not statistically different from zero.

Estimates of girls- and boys-only schools across student self-assessment and confidence outcomes are, in general, not statistically significant. This is shown in Panel D of Table 9. All these questions refer to reading skills, the subject assessed by PIRLS. Although reading is a subject where boys have no advantage over girls, boys in boys-only schools show higher self-confidence levels (measure by their agreement with statements such as "Reading is harder for me than for many of my classmates" or "I am just not good at reading") that boys in coeducational schools.

Overall, the results describe here on the mechanisms mediating the single-sex school effect using teacher and student survey data are consistent with recent findings in other contexts. For example, Lee et al. (2014) find that teachers in boys-only schools in Korea employ stricter discipline and teaching methods and that single-sex schooling reduces the number of male students bullied. They are also in line with the findings for Trinidad and Tobago reported in Jackson (2019), which show no evidence that the single-sex school effect is mediated by a greater alignment of instructional practices to the needs of each gender. Finally, the fact that boys in single-sex schools reported higher self-confidence levels in reading is consistent with the evidence from Germany that single-sex learning environments reinforce academic self-concept of ability in atypical subjects (Kessels and Hannover, 2010).[42]

[42]In Appendix Table F3 I report OLS estimates of equation (1) that consider the entire Year 5 student population in 2016 and not just the randomized lottery applicants. I find a similar pattern of a positive effect of single-sex education on student satisfaction for both boys and girls and a

7. Conclusion

This paper investigates whether single-sex education in childhood affects students' test scores and subject take-up later in secondary school. I leverage random variation from admission lotteries used by the Catholic schools in Malta. I exploit rich administrative school data to show that the setting resembles an ideal educational experiment: schools are indistinguishable from one another in many important dimensions (e.g., curriculum design, teacher quality, class size), and students' allocation to single-sex or coeducational schools is determined by random lottery numbers.

I find that attending a single-sex school leads to significant achievement gains of around 0.70σ on the end-of-primary-school English and Maltese tests and of 0.55σ on the mathematics test. Furthermore, I find that the benefits of gender-homogeneous schools are positive for both boys and girls and that the estimated effects are not driven by parents with strong preferences for single-sex education. I also find that the single-sex school effects are long-lasting. Female and male students who attended single-sex primary schools prefer different curriculum tracks in secondary school. Girls choose more science courses that girls coming from a coed primary school, while boys choose less. Both choose fewer vocational courses, which indirectly reflect that single-sex education increases the odd of entering university.

Survey data reveal that the mechanisms mediating the single-sex school effect are related to higher quality in the teacher-student relationship, teachers use of guided instruction, clear and strict discipline policies, a better school environment measured by bullying incidents and a decrease in truancy.

Overall, these findings suggest that single-sex education can improve student achievement and educational attainment, and it could do so at little or no cost. However, to have a complete grasp of the nature and impact of same-gender learning environments, further work is needed. Interventions such as single-sex education may be designed more effectively by having a better understanding of the optimal timing and its consequences for non-academic outcomes.

greater self-confidence in reading for boys.

REFERENCES

Abdulkadiroğlu, Atila, Nikhil Agarwal, and Parag A. Pathak (2017). "The Welfare Effects of Coordinated Assignment: Evidence from the New York City High School Match". In: *American Economic Review* 107.12, pp. 3635–89.

Abdulkadiroğlu, Atila, Weiwei Hu, and Parag A. Pathak (2013). *Small High Schools and Student Achievement: Lottery-Based Evidence from New York City*. Working Paper 19576. National Bureau of Economic Research.

Abdulkadiroğlu, Atila, Parag A. Pathak, and Christopher R. Walters (2018). "Free to Choose: Can School Choice Reduce Student Achievement?" In: *American Economic Journal: Applied Economics* 10.1, pp. 175–206.

Abdulkadiroğlu, Atila et al. (2011). "Accountability and Flexibility in Public Schools: Evidence from Boston's Charters And Pilots". In: *The Quarterly Journal of Economics* 126.2, pp. 699–748.

Abdulkadiroğlu, Atila et al. (2017). "Research Design Meets Market Design: Using Centralized Assignment for Impact Evaluation". In: *Econometrica* 85.5, pp. 1373–1432.

Alan, Sule, Seda Ertac, and Ipek Mumcu (2018). "Gender Stereotypes in the Classroom and Effects on Achievement". In: *The Review of Economics and Statistics* 100.5, pp. 876–890.

Angrist, Joshua et al. (2002). "Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment". In: *American Economic Review* 92.5, pp. 1535–1558.

Angrist, Joshua et al. (2010). "Inputs and Impacts in Charter Schools: KIPP Lynn". In: *American Economic Review* 100.2, pp. 239–43.

— (2012). "Who Benefits from KIPP?" In: *Journal of Policy Analysis and Management* 31.4, pp. 837–860.

Antecol, Heather, Ozkan Eren, and Serkan Ozbeklik (2015). "The Effect of Teacher Gender on Student Achievement in Primary School". In: *Journal of Labor Economics* 33.1, pp. 63–89.

Bettinger, Eric P. and Bridget Terry Long (2005). "Do Faculty Serve as Role Models? The Impact of Instructor Gender on Female Students". In: *American Economic Review* 95.2, pp. 152–157.

Booth, Alison, Lina Cardona-Sosa, and Patrick Nolen (2018). "Do single-sex classes affect academic achievement? An experiment in a coeducational university". In: *Journal of Public Economics* 168, pp. 109–126.

Brenoe, Anne Ardila and Ulf Zoelitz (2018). *Exposure to More Female Peers Widens the Gender Gap in STEM Participation*. Working Paper. University of Zurich, Department of Economics.

Calleja, James (2008). "Example from Malta". In: *Permeability Between Vocational Education and Training and Higher Education. Examples from Austria, Czech Republic, Germany, Malta, The Netherlands and Slovenia*. Karin Luomi-Messerer (Ed.), pp. 61–74.

Card, David and A. Abigail Payne (2017). *High School Choices and the Gender Gap in STEM*. Working Paper 23769. National Bureau of Economic Research.

Carlana, Michela (2019). "Implicit Stereotypes: Evidence from Teachers' Gender Bias". In: *The Quarterly Journal of Economics* 134.3, pp. 1163–1224.

Chetty, Raj et al. (2011). "How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star". In: *The Quarterly Journal of Economics* 126.4, pp. 1593–1660.

Choi, Eleanor Jawon, Hyungsik Roger Moon, and Geert Ridder (2014). "Estimation of an Education Production Function under Random Assignment with Selection". In: *American Economic Review* 104.5, pp. 206–11.

Cullen, Julie Berry, Brian A. Jacob, and Steven Levitt (2006). "The Effect of School Choice on Participants: Evidence from Randomized Lotteries". In: *Econometrica* 74.5, pp. 1191–1230.

Cunha, Flavio and James Heckman (2007). "The Technology of Skill Formation". In: *American Economic Review* 97.2, pp. 31–47.

Cunha, Flavio, James J. Heckman, and Susanne M. Schennach (2010). "Estimating the Technology of Cognitive and Noncognitive Skill Formation". In: *Econometrica* 78.3, pp. 883–931.

Cutajar, Mario (2007). "Educational Reform in the Maltese Islands". In: *Malta Review of Educational Research* 5.1, pp. 3–21.

Dale, Stacy Berg and Alan B. Krueger (2002). "Estimating the Payoff to Attending a More Selective College: An Application of Selection on Observables and Unobservables". In: *The Quarterly Journal of Economics* 117.4, pp. 1491–1527.

Dee, Thomas S. (2007). "Teachers and the Gender Gaps in Student Achievement". In: *Journal of Human Resources* 42.3, pp. 528–554.

Deming, David J. (2011). "Better Schools, Less Crime?" In: *The Quarterly Journal of Economics* 126.4, pp. 2063–2115.

Deming, David J. et al. (2014). "School Choice, School Quality and Postsecondary Attainment". In: *American Economic Review* 104.3, pp. 991–1013.

Dobbie, Will and Roland G. Fryer (2015). "The Medium-Term Impacts of High-Achieving Charter Schools". In: *Journal of Political Economy* 123.5, pp. 985–1037.

Doris, Aedín, Donal O'Neill, and Olive Sweetman (2013). "Gender, single-sex schooling and maths achievement". In: *Economics of Education Review* 35, pp. 104–119. ISSN: 0272-7757.

Dustmann, Christian, Hyejin Ku, and Do Won Kwak (2018). "Why Are Single-Sex Schools Successful?" In: *Labour Economics* 54, pp. 79–99.

Eccles, Jacquelynne S. (1999). "The Development of Children Ages 6 to 14". In: *The Future of Children* 9.2, pp. 30–44. ISSN: 10548289, 15501558.

Eisenkopf, Gerald et al. (2015). "Academic performance and single-sex schooling: Evidence from a natural experiment in Switzerland". In: *Journal of Economic Behavior & Organization* 115, pp. 123–143.

Gunderson, Elizabeth A. et al. (2012). "The Role of Parents and Teachers in the Development of Gender-Related Math Attitudes". In: *Sex Roles* 66.3, pp. 153–166.

Hastings, Justine S., Thomas J. Kane, and Douglas O. Staiger (2009). *Heterogeneous Preferences and the Efficacy of Public School Choice*. Working Paper. Yale University.

Heckman, James, Rodrigo Pinto, and Peter Savelyev (2013). "Understanding the Mechanisms through Which an Influential Early Childhood Program Boosted Adult Outcomes". In: *American Economic Review* 103.6, pp. 2052–86.

Hoxby, Caroline (2000). *Peer effects in the classroom: Learning from Gender and Race Variation*. NBER Working Papers 7867. National Bureau of Economic Research.

Hoxby, Caroline and Sonali Murarka (2008). "Methods of Assessing the Achievement of Students in Charter Schools". In: *Charter Schools Outcomes. Mahwah, NJ: Lawrence Erlbaum Associates.*

Jackson, Kirabo C. (2012). "Single-sex schools, student achievement, and course selection: Evidence from rule-based student assignments in Trinidad and Tobago". In: *Journal of Public Economics* 96.1, pp. 173–187.

Jackson, Kirabo C. (2019). "Can Introducing Single-Sex Education into Low-Performing Schools Improve Academics, Arrests, and Teen Motherhood?" In: *The Journal of Human Resources*, forthcoming.

Jepsen, Christopher (2015). *Class size: Does it matter for student achievement?* Tech. rep. 190. IZA World of Labor.

Kessels, Ursula and Bettina Hannover (2010). "When being a girl matters less: Accessibility of gender-related self-knowledge in single-sex and coeducational classes and its impact on students' physics-related self-concept of ability". In: *British Journal of Educational Psychology* 78.2, pp. 273–289.

Ketel, Nadine et al. (2016). "The Returns to Medical School: Evidence from Admission Lotteries". In: *American Economic Journal: Applied Economics* 8.2, pp. 225–54.

Klein, Sue et al. (2018). *Tracking Deliberate Sex Segregation in U.S. K-12 Public Schools.* Tech. rep. Feminist Majority Foundation.

Lancsar, Emily, Denzil G. Fiebig, and Arne Risa Hole (2017). "Discrete Choice Experiments: A Guide to Model Specification, Estimation and Software". In: *PharmacoEconomics* 35.7, pp. 697–716.

Lavy, Victor and Analia Schlosser (2011). "Mechanisms and Impacts of Gender Peer Effects at School". In: *American Economic Journal: Applied Economics* 3.2, pp. 1–33.

Lazear, Edward P. (2001). "Educational Production". In: *The Quarterly Journal of Economics* 116.3, pp. 777–803.

Lee, Soohyung et al. (Dec. 2014). *All or Nothing? The Impact of School and Classroom Gender Composition on Effort and Academic Achievement.* NBER Working Papers 20722. National Bureau of Economic Research, Inc.

Lim, Jaegeum and Jonathan Meer (2017). "The Impact of Teacher–Student Gender Matches: Random Assignment Evidence from South Korea". In: *Journal of Human Resources* 52.4, pp. 979–997.

Lu, Fangwen and Michael Anderson (2015). "Peer Effects in Microenvironments: The Benefits of Homogeneous Classroom Groups". In: *Journal of Labor Economics* 33.1, pp. 91–122.

Lucas, Adrienne M. and Isaac M. Mbiti (2014). "Effects of School Quality on Student Achievement: Discontinuity Evidence from Kenya". In: *American Economic Journal: Applied Economics* 6.3, pp. 234–63.

Malta Today (2015). *The cost of private education: What are parents really paying for?*

McFadden, Daniel and Kenneth Train (2000). "Mixed MNL models for discrete response". In: *Journal of Applied Econometrics* 15.5, pp. 447–470.

OECD (2016). *Education at a Glance 2016*, p. 508.

Paredes, Valentina (2014). "A teacher like me or a student like me? Role model versus teacher bias effect". In: *Economics of Education Review* 39, pp. 38–49. ISSN: 0272-7757.

Park, Hyunjoon, Jere R. Behrman, and Jaesung Choi (2013). "Causal Effects of Single-Sex Schools on College Entrance Exams and College Attendance: Random Assignment in Seoul High Schools". In: *Demography* 50.2, 447–469.

— (2018). "Do single-sex schools enhance students' STEM (science, technology, engineering, and mathematics) outcomes?" In: *Economics of Education Review* 62, pp. 35–47.

Pathak, Parag A. (2011). "The Mechanism Design Approach to Student Assignment". In: *Annual Review of Economics* 3.1, pp. 513–536.

Pop-Eleches, Cristian and Miguel Urquiola (2013). "Going to a Better School: Effects and Behavioral Responses". In: *American Economic Review* 103.4, pp. 1289–1324.

Riordan, Cornelius H. (1990). *Girls and Boys in School: Together Or Separate?* Teachers College Press, Teachers College, Columbia University. ISBN: 9780807729922.

Rouse, Cecilia Elena (1998). "Private School Vouchers and Student Achievement: An Evaluation of the Milwaukee Parental Choice Program". In: *The Quarterly Journal of Economics* 113.2, pp. 553–602.

Ruble, Diane N. and Carol L. Martin (1998). "Gender development". In: *Handbook of Child Psychology: Social, Emotional, and Personality Development*. W. Damon & N. Eisenberg (Ed.), pp. 933–1016.

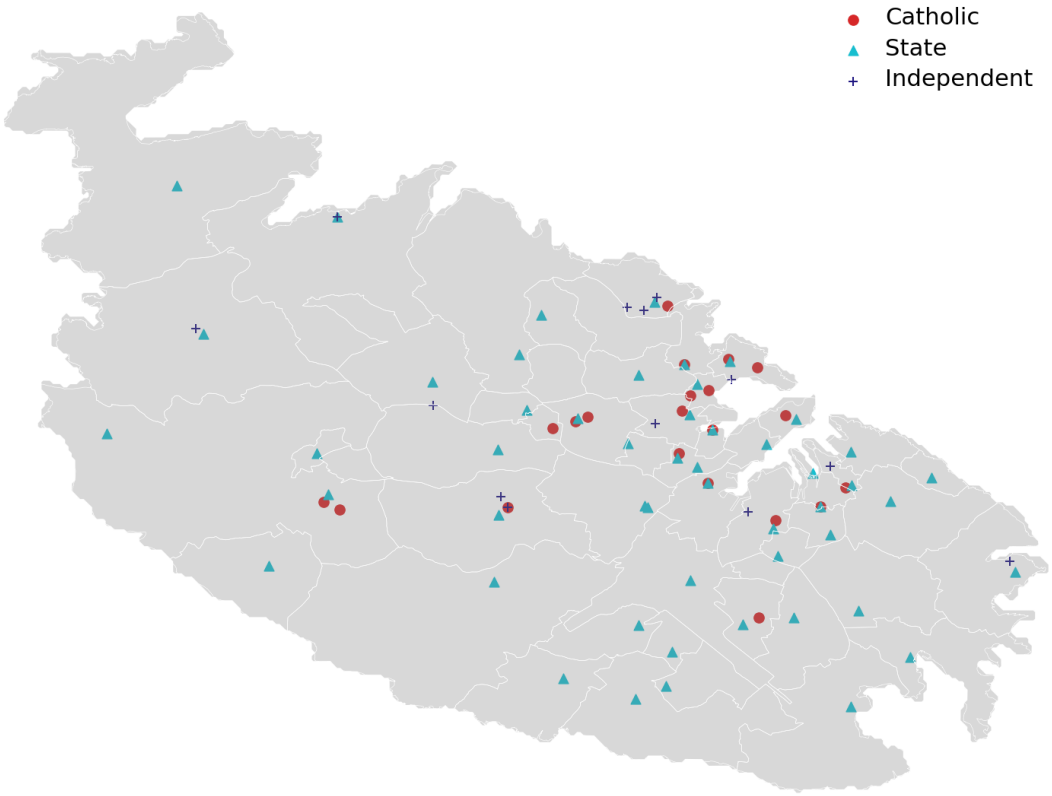
Sax, Linda, Casey Shapiro, and Eagan Kevin (2011). "Promoting Mathematical and Computer Self-Concept among Female College Students: is There a Role of Single-Sex Secondary Education?" In: *Journal of Women and Minorities in Science and Engineering* 17.4, pp. 325–355.

Sohn, Hosung (2016). "Mean and distributional impact of single-sex high schools on students' cognitive achievement, major choice, and test-taking behavior: Evidence from a random assignment policy in Seoul, Korea". In: *Economics of Education Review* 52, pp. 155–175.

Sullivan, Alice (2009). "Academic self-concept, gender and single-sex schooling". In: *British Educational Research Journal* 35.2, pp. 259–288.

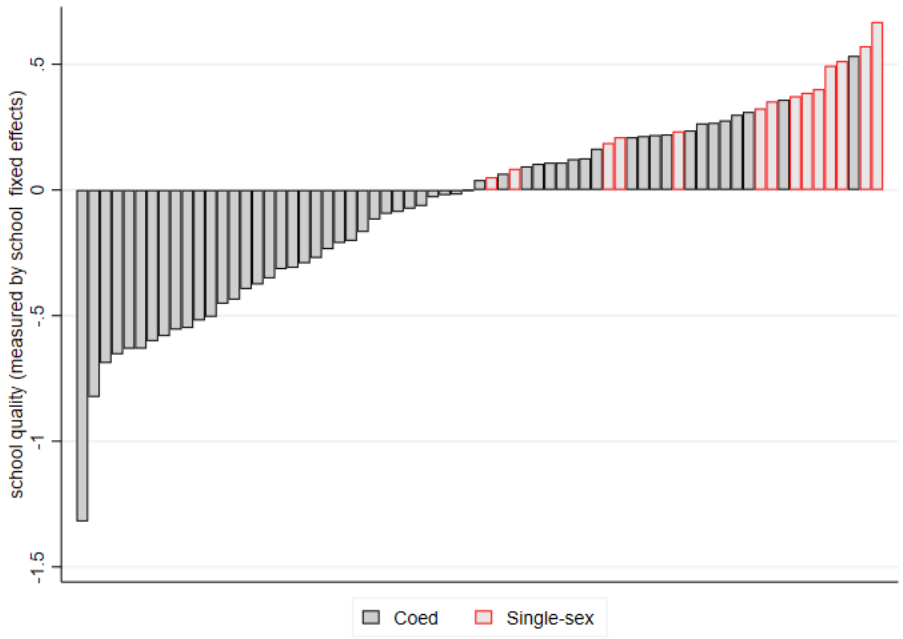
Whitmore, Diane (2005). "Resource and Peer Impacts on Girls' Academic Achievement: Evidence from a Randomized Experiment". In: *The American Economic Review* 95.2, pp. 199–203.

Figure 1. Location of Primary Schools in Malta



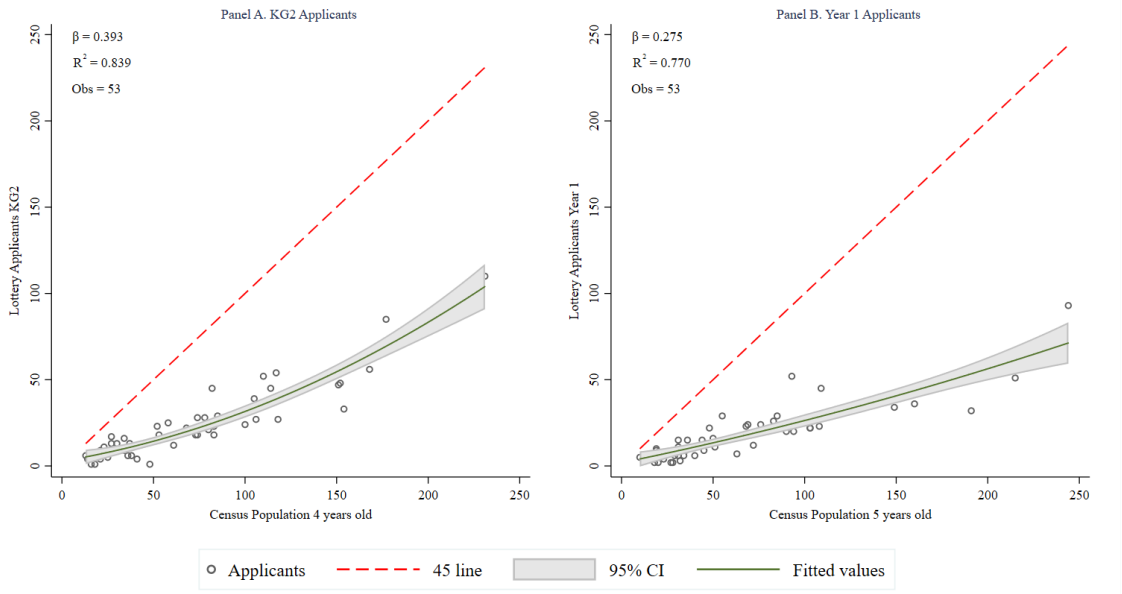
Notes: This map displays the location of primary schools in the island of Malta. Each marker represents a school. Catholic schools are indicated in red dots, state schools are indicated in light-blue triangles, while independent schools are indicated in blue crosses. The number of schools is 21, 51 and 14 for the Catholic, State and Independent sector, respectively. The map also shows the division of the island into 54 localities.

Figure 2. School Quality



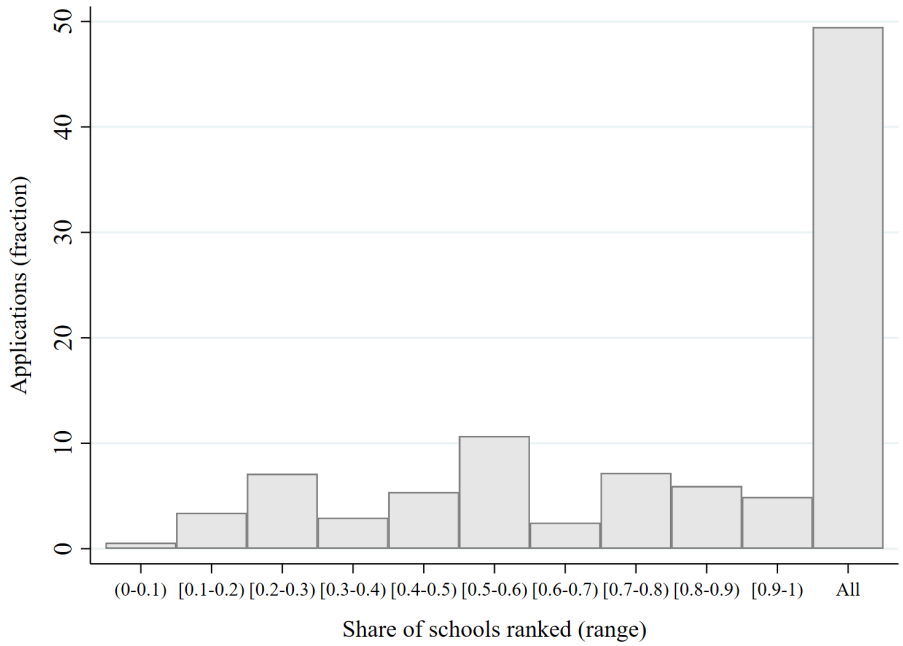
Notes: Each bar represents a school. The y-axis shows estimates results from a OLS regression of students composite score at end-of-primary exam on the full set of year and school fixed effect dummies. The composite score is the average of the scores in English, Maltese and mathematics and was standardized to have mean zero and unit variance by year. Sample includes all End of Primary Benchmark test-takers from state and catholic schools during 2014-2015.

Figure 3. Lottery Applicants Representativeness



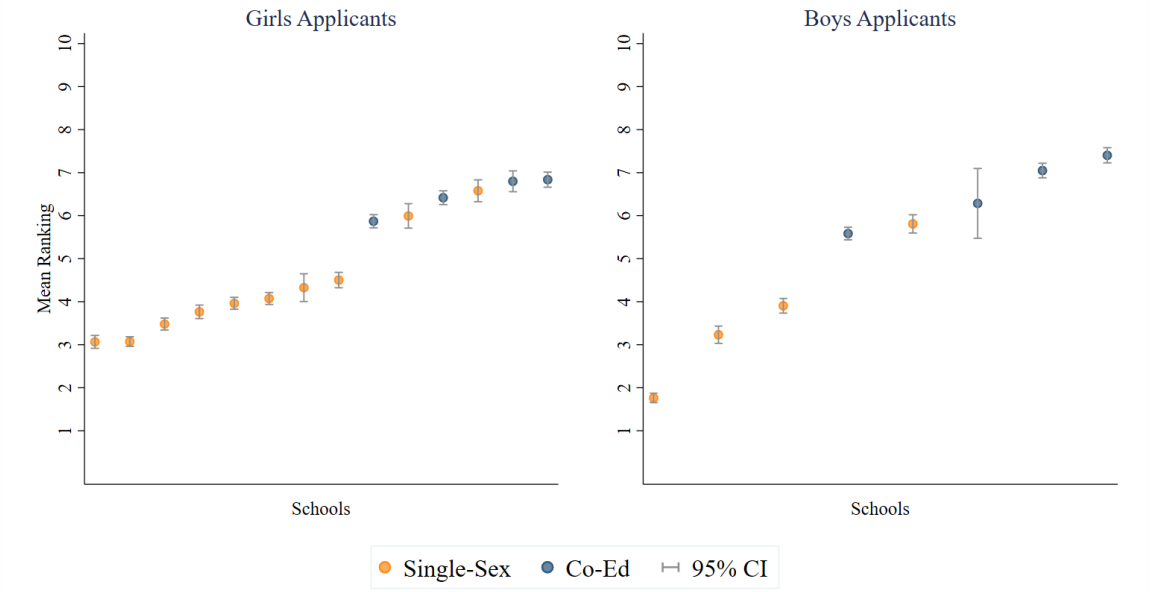
Notes: Panel A (Panel B) shows the applicant population for KG2 (Year 1) lottery in each census locality for 2010. Coefficient reported comes from a regression on the number of applicants per locality on the census population for a given age.

Figure 4. Applicants Distribution According to the Share of School Ranked



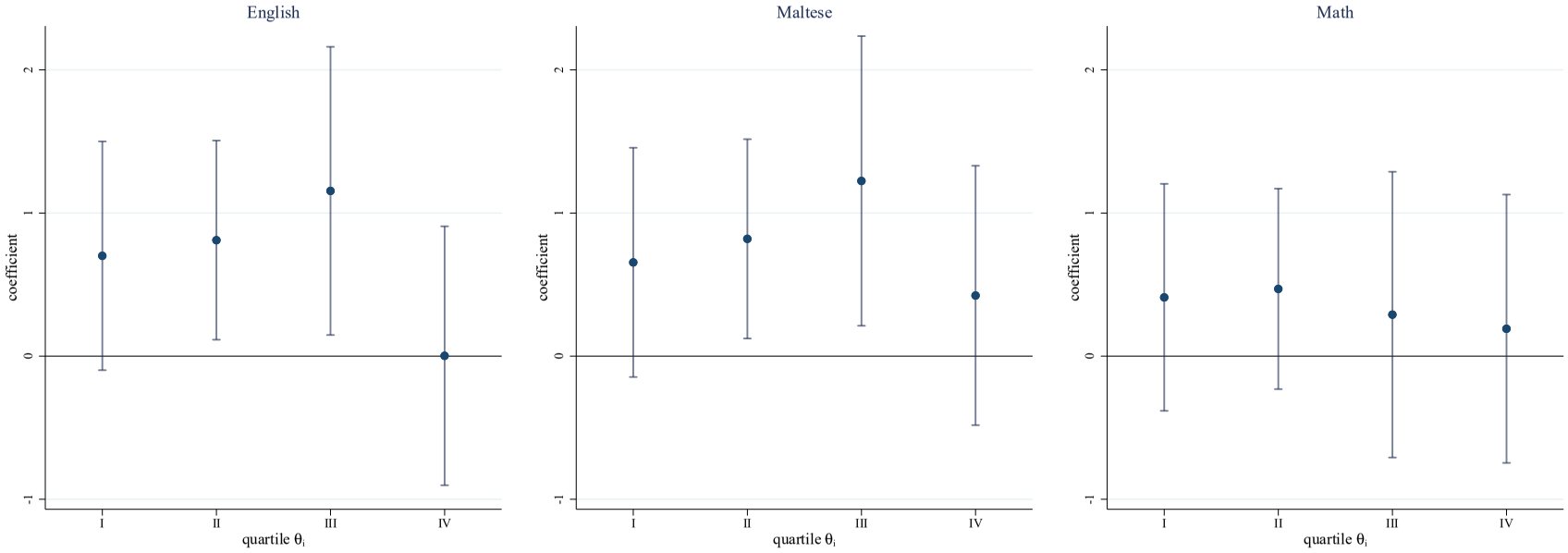
Notes: Each bar represents the fraction of applicants whose share of school ranked falls within the specified range.

Figure 5. 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 6. Heterogeneity in Treatment Effect by Weight 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 $SingleSex_i$ interacted with dummies for the quartile of single-sex weight ($\hat{\theta}_i$) on test scores from the 2SLS model described in equation (2). Regression controls for baseline demographic characteristics, quartiles of the single-sex attribute weight, year of exam fixed effect and lottery fixed effect.

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

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

	Applicants not Offered Single-Sex Slot (1)	Applicants Offered Single-Sex Slot (2)	Balance Regression (3)
Born Quarter 1	0.241	0.267	0.029
			(0.018)
Born Quarter 2	0.257	0.240	-0.011
			(0.019)
Born Quarter 3	0.242	0.237	-0.002
			(0.018)
Born Quarter 4	0.257	0.253	-0.015
			(0.018)
Biblical Name	0.101	0.097	-0.011
			(0.013)
Popular Surname	0.247	0.254	-0.003
			(0.018)
Mother Age	33.7	34.4	-0.125
			(0.226)
Father Age	36.0	37.1	0.067
			(0.248)
Distance to Local School	1.25	1.27	-0.062
			(0.117)
High Quality Local School	0.283	0.301	0.006
			(0.019)
Southern Harbour	0.148	0.140	-0.012
			(0.015)
Northern Harbour	0.321	0.307	-0.001
			(0.019)
South Eastern	0.159	0.170	0.006
			(0.016)
Western	0.194	0.195	-0.011
			(0.017)
Northern	0.175	0.186	0.019
			(0.015)
Social Assistance	4119	4113	-6.030
			(31.081)
Observations	1773	1064	
F-test			47.750
p-value			0.775

Notes: Columns 1 and 2 report mean values for applicants according to their admission lottery status. 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 applicant received a single-sex offer and zero otherwise. For detailed description of the variables, see Appendix A. Each regression includes lottery fixed effects. Sample comprises randomized applicants in non-degenerate lotteries. The F-statistics jointly test balance for all baseline covariates. An asterisk indicates statistically significant differences 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			
School Size (m2)	4673.3	4032.3	-641.0
Distance to Valletta (miles)	1.92	2.53	0.616
Subject instruction time (hs 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
Total instruction time (hs per year)[a]	883.04	936.25	53.208
Class Size	25.7	25.4	-0.394
Observations	4	17	

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. Unless otherwise indicated, all variables comes from administrative school data for 2010. [a] Variables from PIRLS 2016.

Table 4. Single-Sex School Effect on Test Scores

	All Lotteries			Lotteries with Preferences		
	First Stage (1)	Reduced Form (2)	2SLS (3)	First Stage (4)	Reduced Form (5)	2SLS (6)
English	0.777***	0.238***	0.320***	0.596***	0.441**	0.638***
	(0.053)	(0.066)	(0.086)	(0.109)	(0.167)	(0.190)
Observations	2590	2590	2590	1275	1275	1275
Maltese	0.777***	0.254***	0.346***	0.598***	0.534**	0.742***
	(0.053)	(0.075)	(0.098)	(0.109)	(0.240)	(0.277)
Observations	2587	2587	2587	1273	1273	1273
Math	0.785***	0.141	0.194	0.602***	0.391*	0.554**
	(0.050)	(0.111)	(0.141)	(0.115)	(0.209)	(0.239)
Observations	2555	2555	2555	1257	1257	1257

Notes: This table reports 2SLS estimates of the single-sex school effect on end-of-primary test scores. First stage in columns 1 and 4 reports coefficient for the single-sex offer instrument. All models include year, risk set controls and demographics (quarter of birth, biblical name and region fixed effects). Columns 1-3 sample use all the lotteries and risk set controls are defined by the application pattern. Columns 4-6 sample use lotteries with ROL and risk set controls are defined by the interaction between the propensity score and the application pattern. Robust standard errors, clustered at school level, are reported in parenthesis. Significance levels are indicated by * $< .1$, ** $< .05$, *** $< .01$.

Table 5. Robustness Check: Single-Sex School Effect on Test Scores

	All Lotteries			Lotteries with Preferences		
	First Stage (1)	Reduced Form (2)	2SLS (3)	First Stage (4)	Reduced Form (5)	2SLS (6)
English	0.771***	0.361***	0.488***	0.541***	0.270**	0.443**
	(0.070)	(0.094)	(0.139)	(0.140)	(0.118)	(0.176)
Observations	1665	1665	1665	896	896	896
Maltese	0.771***	0.366***	0.504***	0.542***	0.407*	0.612**
	(0.070)	(0.108)	(0.147)	(0.140)	(0.243)	(0.304)
Observations	1662	1662	1662	894	894	894
Math	0.780***	0.149	0.205	0.551***	0.283	0.385
	(0.064)	(0.135)	(0.179)	(0.139)	(0.227)	(0.278)
Observations	1641	1641	1641	883	883	883

Notes: This table reports 2SLS estimates of the single-sex school effect on end-of-primary test scores. First stage in columns 1 and 4 reports coefficient for the single-sex offer instrument for the first lottery a student participates. All models include year, risk set controls and demographics (quarter of birth, biblical name and region fixed effects). Columns 1-3 sample use all the lotteries and risk set controls are defined by the application pattern. Columns 4-6 sample use lotteries with ROL and risk set controls are defined by the interaction between the propensity score and the application pattern. Robust standard errors, clustered at school level, are reported in parenthesis. Significance levels are indicated by * $< .1$, ** $< .05$, *** $< .01$.

Table 6. Single-Sex School Effect on Girls and Boys Test Scores

	All Lotteries			Lotteries with Preferences		
	All	Girls	Boys	All	Girls	Boys
	(1)	(2)	(3)	(4)	(5)	(6)
English	0.320***	0.390**	0.288***	0.638***	0.405*	0.901**
	(0.086)	(0.168)	(0.084)	(0.190)	(0.237)	(0.448)
Observations	2590	1119	1471	1275	623	652
Maltese	0.346***	0.406***	0.351***	0.742***	0.528*	1.070*
	(0.098)	(0.120)	(0.115)	(0.277)	(0.277)	(0.633)
Observations	2587	1119	1468	1273	623	650
Math	0.194	0.314**	0.149	0.554**	0.712	0.449*
	(0.141)	(0.155)	(0.168)	(0.239)	(0.517)	(0.268)
Observations	2555	1112	1443	1257	619	638

Notes: This table reports 2SLS estimates of the single-sex school effect on end-of-primary test scores, separately for girls (columns 2 and 5) and boys (columns 3 and 6). All models include year, risk set controls and demographics (quarter of birth, biblical name and region fixed effects). Columns 1-3 sample use all the lotteries and risk set controls are defined by the application pattern. Columns 4-6 sample use lotteries with ROL and risk set controls are defined by the interaction between the propensity score and the application pattern. Robust standard errors, clustered at school level, are reported in parenthesis. Significance levels are indicated by * $< .1$, ** $< .05$, *** $< .01$.

Table 7. Single-Sex School Effect on Option Subjects

	All Lotteries		
	All	Girls	Boys
	(1)	(2)	(3)
At least one science subject	-0.112*	-0.089	-0.116*
	(0.063)	(0.202)	(0.066)
Two science subjects	-0.027	0.211*	-0.064
	(0.050)	(0.110)	(0.054)
At least one *female* subject	-0.124**	-0.191	-0.074
	(0.068)	(0.194)	(0.077)
At least one vocational subject	-0.176***	-0.325**	-0.118*
	(0.055)	(0.162)	(0.061)
Observations	1303	1303	1303

Notes: Table reports 2SLS estimates of the effect of single-sex primary school on the outcome variables shown in each row. Regressions for female and vocational subjects add a control for the number of prevalently female and vocational subjects offered, respectively. Risk set controls are defined by the application pattern. Sample includes student cohorts born in 2005 and 2006 at (18 out of 19) catholic secondary schools that participated in the primary admission lotteries. Robust standard errors 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	-0.051	-0.382
	(0.290)	(0.274)
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 belongs to the Catholic sector and 0 if it belongs to the State sector. Sample includes 165 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 A for details on the definition of the outcome in each row.

Table 9. Effect of Single-sex School on Students Inputs

	Girls-only Schools (1)	Boys-only Schools (2)
Panel A: Student Satisfaction & School Bullying		
Student satisfaction	0.602	0.026
	(0.374)	(0.206)
Absenteeism	-0.269*	-0.021
	(0.143)	(0.074)
Bullying index	0.056	-0.388*
	(0.299)	(0.234)
Panel B: Teacher-Student Relationship		
Teachers at my school are fair to me	0.208	-0.204*
	(0.254)	(0.121)
Panel C: Instructional Practices		
Aligned pedagogical practices	0.306	-0.279
	(0.345)	(0.170)
Panel D: Self-assessment and confidence		
I usually do well in reading	0.341	0.097
	(0.385)	(0.180)
Reading is easy for me	-0.216	-0.034
	(0.359)	(0.191)
Reading is harder for me than any other subject	0.064	-0.072
	(0.303)	(0.187)
Reading is harder for me than for many of my classmates	0.094	-0.251
	(0.279)	(0.224)
I am just not good at reading	0.241	-0.362*
	(0.288)	(0.195)

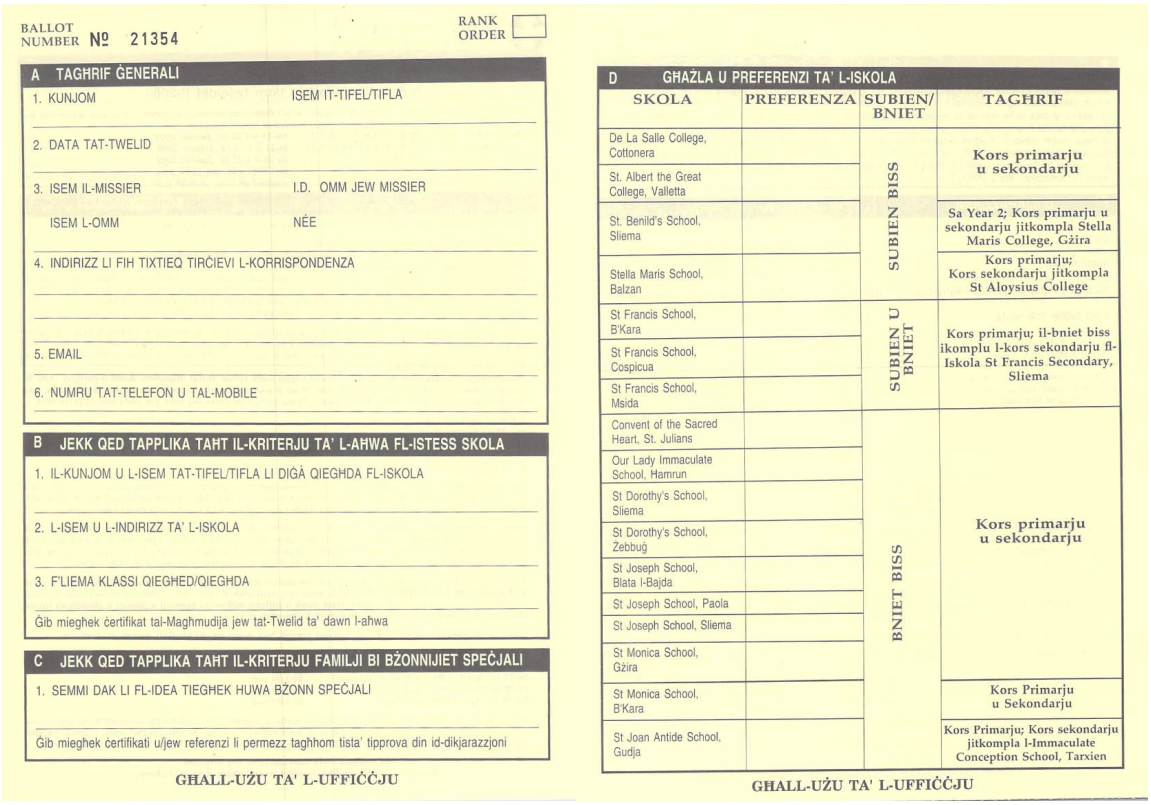
Notes: Table reports 2SLS estimates of the effect of single-sex school on the outcome variable shown in each row. Regressions include an indicator for girls-only schools, an indicator for boys-only schools and the interaction between a catholic sector indicator and an indicator for female student. As such, coefficients in column 1 (2) are interpreted as the effect of single-sex education for girls (boys) relative to girls (boys) students at coeducational catholic schools. Sample includes 840 randomized applicants born in 2006 that participated in KG2 and Year 1 lotteries and were matched with the PIRLS survey. Robust standard errors are reported in parenthesis. Significance levels are indicated by * < .1, ** < .05, *** < .01. See Appendix A for details on the definition of the outcome in each row.

A. Variable Definition and Sources

A.1. Catholic Schools Lottery Data

The lottery analysis file is constructed using applications, school assignment and enrollment provided by the Secretariat for Catholic Education in Malta for school years 2008-2011. The catholic school application form is divided into four parts (A-D) as shown in the figure below. Section A ask parents to submit information about the applicants' full name, date of birth and gender. It also requires the name of the father and the mother and to provide the ID of any of them, their address and contact information. In Section B, parent need to declare if the applicant has siblings already enrolled in any of the catholic schools and report the school and year attended. Section C asks parent of any special educational need the applicant may have. In Section D parents submit their rank order list (ROL) by ranking schools in order of their preference.

Figure A1. Application Form



BALLOT NUMBER № 21354

RANK ORDER

A TAGĦRIF 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 taghhom 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			
St. Benild's School, Sliema			Sa Year 2; Kors primarju u sekondarju jitkompla Stella Maris College, Gżira
Stella Maris School, Balzan			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			
St Francis School, Msida			
Convent of the Sacred Heart, St. Julians		BNIET BISS	Kors primarju u sekondarju
Our Lady Immaculate School, Hamrun			
St Dorothy's School, Sliema			
St Dorothy's School, Zebbuġ			
St Joseph School, Blata l-Bajda			
St Joseph School, Paola			
St Joseph School, Sliema			
St Monica School, Gżira			
St Monica School, B'Kara			Kors Primarju u Sekondarju
St Joan Antide School, Gudja			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.

Demographic information of applicants are used to compute the variables in the balance table:

Biblical Name is a dummy variable equal to one if the student first name is a proper name from the Bible. 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.

Popular Surname is an dummy 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.

Mother (Father) Age can be recovered from the reported mother (or father) id number. The last two digits of a person ID in Malta correspond to the person year of birth.

Distant to Local School is the distance (in miles) between the applicant home address to the local state-run school.

High Quality Local School variable is an indicator equal to one if the state-run school in the locality the applicant resides is above the national average and zero if it is below. To infer school quality I use a fixed effect model based on the students performance at the Benchmark during 2014 and 2015.

Social Assistance is computed as the average of the social assistance expenditure per beneficiary (in euros) in the locality in which the applicant resides.

A.2. Applicant outcomes

End of Primary Benchmark Test scores for English, Maltese and Math exam are available for years 2014-2017 and was provided by the Educational Assessment Unit (EAU), Department of Curriculum Management. Each observation in the Benchmark data file corresponds to a student's test results in a particular year. Exam scores are standardized to have mean zero and unit standard deviation within a a subject-year.

Option Subjects Administrative data on student's choice of elective subjects was collected in catholic secondary schools. Optional subjects comprise a set of academic and vocational subjects and students have two choose two at the end of their second year of secondary education (Year 8). The sample comprises Year 8 students in catholic schools for the scholastic years 2016-2019.

A.3. Teacher Survey

I use the Progress in International Reading Literacy 2016 (PIRLS 2016) data to obtain measures of the teachers school satisfaction, teaching practices and other inputs. PIRLS is run by the International Association for the Evaluation of Educational Achievement (IEA) and, in Malta, covers all teachers of Year 5 students.
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 1 for Very often, 2 for Often, 3 for Sometimes, and 4 for Never or almost never. Each variable scale was reverted. I sum teacher responses to each of the statements and standardized the index to be have zero mean and 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 1 for Agree a lot, 2 for Agree a little, 3 for Disagree a little, and 4 for Disagree a lot. The variable scale was reverted and standardized to have zero mean 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 answer categories are 1 for Agree a lot, 2 for Agree a little, 3 for Disagree a little, and 4 for Disagree a lot. The numerical values assigned to each answer category is reversed. The index is constructed by summing teachers responses to each of the above two questions, standardized to have zero mean and standard deviation of one.
The measure of *individualized instruction and feedback* is derived using answer to the following three statements: "I use individualized instruction for reading", "Provide materials that are appropriate for the reading levels of individual students", and "Give individualized feedback to each student". The answer categories for the first item are 1 for Always or almost always, 2 for Often, 3 for Sometimes, and 4 for Never. The answer categories for the last two items are 1 for Every or almost every lesson, 2 for About half the lessons, 3 for Some lessons, and 4 for Never. The numerical values assigned to each answer category is reversed. Teachers responses to the above

three questions are summed and then standardized to have zero mean and standard deviation of one.

To measure the 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", and "Teachers' ability to inspire students". The answer categories for the first three items are 1 for Every or almost every lesson, 2 for About half the lessons, 3 for Some lessons, and 4 for Never. For the last items the answer categories are 1 for Very high, 2 for High, 3 for Medium, 4 for Low and 5 for Very low. The numerical values assigned to each answer category is reversed. Each variable is standardized before creating the average.

Guided instruction is a measure derived from a survey question asking teachers how often they organize students "to work independently on an assigned plan or goal". The answer categories are 1 for Always or almost always, 2 for Often, 3 for Sometimes, and 4 for Never. The variable is standardized to have zero mean and standard deviation of one.

Ability grouping variable is derived from a survey question asking teachers how often they "create same-ability groups". The answer categories are 1 for Always or almost always, 2 for Often, 3 for Sometimes, and 4 for Never. Answer categories were reversed before standardizing to have zero mean and standard deviation of one.

Teachers' measure of 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". The 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". The answer categories for both measures are 1 for Very high, 2 for High, 3 for Medium, 4 for Low and 5 for Very low. The numerical values assigned to each answer category is reversed. The responses to the above questions are summed and then standardized to have zero mean and standard deviation of one.

A.4. Student Survey

The *Student satisfaction* variable is an index derived from three survey questions which ask students how much they agree with the following statements: "I like being in school", "I feel like I belong at this school", and "I am proud to go to this school". The answer categories are 1 for Agree a lot, 2 for Agree a little, 3 for Disagree a little, and 4 for Disagree a lot. The variable scale was reverted and student responses to each of the statements are summed and standardized to have a mean of zero and

unit variance.

Absenteeism is a dummy variable equal to one if the student reports being absent from school at least once a month.

The *bullying index* combines responses to the reported frequency a student experience the following things from other students in their school: "Made fun of me or called me names"; "Left me out of their games or activities"; "Spread lies about me"; "Stole something from me"; "Hit or hurt me (e.g., shoving, hitting, kicking)"; "Made me do things I didn't want to do"; "Shared embarrassing information about me"; "Threatened me". The answers range from 1 for At least Once a week, 2 for Once or twice a month, 3 for A few times a year, and 4 for Never. The numerical values assigned to each answer category is reversed. The index is constructed by summing students responses to each of the above eight questions, standardized to have zero mean and standard deviation of one.

The measure of the quality of the teacher-student relationship is constructed based on the student agreement to the statement "Teachers at my school are fair to me". The answer categories are 1 for Agree a lot, 2 for Agree a little, 3 for Disagree a little, and 4 for Disagree a lot. The variable scale was reverted and standardized to have zero mean and standard deviation of one.

Teachers alignment of pedagogical practices is derived using students answers to the statements: "My teacher gives me interesting things to read", "My teacher is easy to understand", and "My teacher does a variety of things to help us learn". The answer categories are 1 for Agree a lot, 2 for Agree a little, 3 for Disagree a little, and 4 for Disagree a lot. The numerical values assigned to each answer category is reversed. Students responses to the above three questions are summed and then standardized to have zero mean and standard deviation of one.

Three items in the questionnaire supply information about student self-assessment related to reading: "I usually do well in reading", "Reading is easy for me", and "Reading is harder for me than any other subject". Another two do it in relation to the student self-confidence (in reading): "Reading is harder for me than for many of my classmates" and "I am just not good at reading". The answers range from 1 for Agree a lot, 2 for Agree a little, 3 for Disagree a little, and 4 for Disagree a lot. Each variable scale was reverted and standardized to have zero mean and standard deviation of one.

B. Matching Rate

The overall match rate between randomized students and the Benchmark 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.[43] Except for the few cases in which I have the student ID number, the key matching variables used were: (a) surname, name and date of birth; (b) surname, name and mother name; (c) surname, name and father name. Table B1 below show the match rate per lottery grade and year.

Table B1. Matching Rate between Lottery Applicants and Exam Takers

Lottery Year	Total Applicants	Randomized Applicants	Match Rate	Balance Regression	
				All Lotteries	Benchmark Participating Schools
	(1)	(2)	(3)	(4)	(5)
Panel A: Lottery KG2					
2009	1091	901	0.765		
2010	1182	1023	0.774		
Panel B: Lottery Year 1					
2010	979	834	0.788		
2011	952	716	0.777		
Panel C: Lottery Year 4					
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.

[43]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 for this paper started in 2016 than to a selective matching.

I assess the quality of the matching procedure by testing if the match rate differ by lottery status. The match differential for lottery winners and losers is about -10.6 percent (column 4), so the probability of being matched is lower if the applicant is a lottery winner, which is consistent with (ii). When applicants who received an offer from any of the three non-participating schools are excluded, the estimated coefficient falls to 0.002 (SE = 0.015), indicating that admission lottery winners are not more likely to be matched than losers.

Note that being a Benchmark participating school is a fact that is unlikely to be related to whether a student is a lottery winner or loser. Moreover, these students would not be part of the regression sample when evaluating the effect of single-sex education on test scores.

C. Additional Balance Tables

Table C1. Child Population and Lottery Applicants

	Child Population (1)	Applicants not Offered Single-Sex Slot (2)	Applicants Offered Single-Sex Slot (3)
Born Quarter 1	0.248	0.241	0.267
Born Quarter 2	0.245	0.257	0.240
Born Quarter 3	0.249	0.242	0.237
Born Quarter 4	0.257	0.257	0.253
Popular Surname	0.243	0.247	0.254
Southern Harbour	0.195	0.148	0.140
Northern Harbour	0.296	0.321	0.307
South Eastern	0.182	0.159	0.170
Western	0.149	0.194	0.195
Northern	0.178	0.175	0.186
Observations	7109	1773	1064

Notes: This table shows summary statistics for children born 2005-2006 (column 1) and for the applicant sample according to their admission lottery status (columns 2 and 3). Data in column 1 comes from the the National Obstetric Information System and the National Statistic Office. Applicants sample (columns 2 and 3) is restricted to randomized applicants in non-degenerate lotteries. For detailed description of the variables, see Appendix A.

Table C2. Descriptive Statistics and Balance Regression

	Lottery Winners			Catholic Offer		
	Coed Offer (1)	Single-Sex Offer (2)	Balance Reg. (3)	Lottery Losers (4)	Lottery Winners (5)	Balance Reg. (6)
Born Quarter 1	0.234	0.267	0.032	0.244	0.261	0.019
			(0.040)			(0.019)
Born Quarter 2	0.195	0.240	0.041	0.259	0.239	-0.015
			(0.038)			(0.019)
Born Quarter 3	0.273	0.237	-0.019	0.241	0.239	-0.001
			(0.042)			(0.018)
Born Quarter 4	0.296	0.253	-0.054	0.253	0.259	-0.002
			(0.044)			(0.019)
Biblical Name	0.132	0.097	-0.031	0.100	0.099	-0.012
			(0.031)			(0.013)
Popular Surname	0.296	0.254	-0.046	0.243	0.259	0.009
			(0.043)			(0.019)
Mother Age	34.6	34.4	0.348	33.6	34.5	-0.192
			(0.461)			(0.238)
Father Age	37.6	37.1	-0.123	35.9	37.1	0.173
			(0.481)			(0.260)
Distance Local School	1.78	1.27	-0.379	1.22	1.31	-0.019
			(0.338)			(0.117)
Quality Local School	0.434	0.437	0.012	0.431	0.436	-0.003
			(0.049)			(0.022)
Southern Harbour	0.179	0.140	-0.045	0.147	0.144	-0.007
			(0.036)			(0.016)
Northern Harbour	0.242	0.307	0.061	0.325	0.303	-0.009
			(0.041)			(0.020)
South Eastern	0.257	0.170	-0.090*	0.152	0.178	0.029
			(0.041)			(0.016)
Western	0.218	0.195	-0.016	0.193	0.197	-0.012
			(0.038)			(0.017)
Northern	0.101	0.186	0.090*	0.181	0.176	0.000
			(0.031)			(0.016)
Social Assistance	4086	4113	44.210	4123	4109	-17.808
			(67.637)			(32.368)
Observations	149	1064		1624	1213	
F-test			35.123			55.301
p-value			0.510			0.463

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 received a single-sex (catholic) offer. Regression includes lottery fixed effects. Sample is restricted to randomized applicants in non-degenerate lotteries. The F-statistics jointly test balance for all baseline covariates. Asterisk indicates statistically significant differences at the 5% level.

Table C3. 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). Each 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 the variables, see Appendix A. The F-statistics jointly test balance for all baseline covariates. Asterisk indicates statistically significant differences at the 5% level.

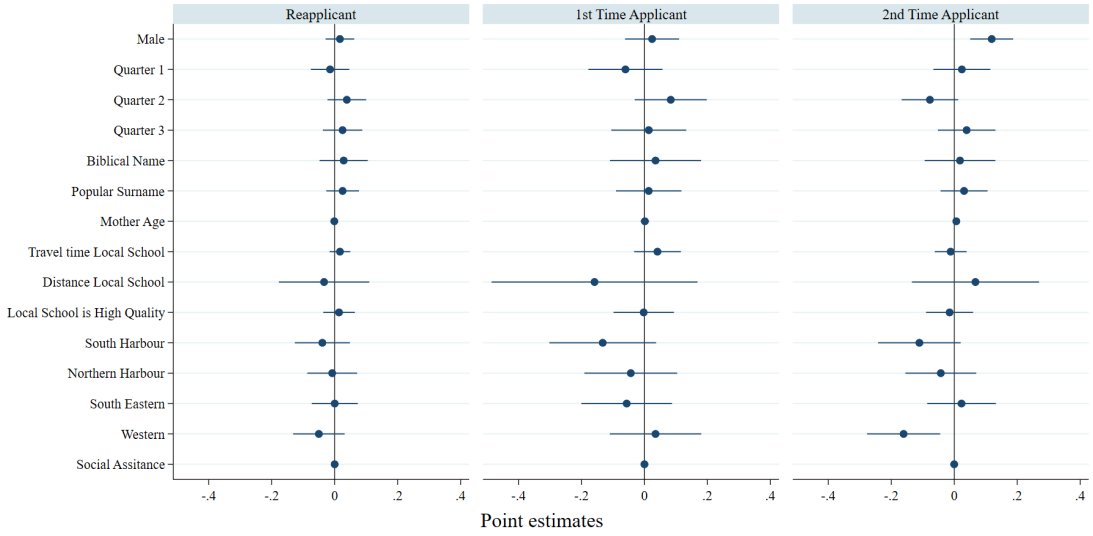
Table C4. Coeducational, Girls-only and Boys-only Catholic School Characteristics

	Catholic Schools			Diff (2)-(1)	Diff (3)-(1)
	Coed	Girls Only	Boys Only		
	(1)	(2)	(3)	(4)	(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					
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
Subject instruction time (hrs x 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
Total instruction time (hrs x year)[a]	883.0	907.8	974.1	24.813	91.069
Class Size	25.7	25.3	25.4	-0.458	-0.325
Observations	4	10	7		

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. Unless otherwise indicated, all variables comes from administrative school data for 2010. [a] Variables from PIRLS 2016.

D. Lottery Reapplication

Figure D1. 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.

E. Catholic Secondary Schools

E.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.[44] Thus, secondary schools comprises students coming from both single-sex and coed primary schools.

E.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 E1 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,

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

Hospitality, Agribusiness, Health and Social Care, Media Literacy Education and Textiles and Fashion.[45] Prevalently female subjects include Home Economics, Health and Social Care, Hospitality, Textile and Fashion and Art/Drama.

Table E1. Optional Subject Set and Primary School Attended

	Prevalently Female Subjects		Vocational Subjects	
	Girls (1)	Boys (2)	Girls (3)	Boys (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 coming 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.

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

Table E2. 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	0.022	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.

F. Mechanisms

Table F1. 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	-0.069	-0.260
	(0.308)	(0.279)
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 belongs to the Catholic sector and 0 if it belongs to the State sector. Sample includes 154 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 F2. 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	-1.928	-2.004
	(0.595)	(0.631)
Panel B: Classroom Environment		
Absenteeism/Late arrival at school	-0.433	-0.123
	(0.589)	(0.624)
Classroom disturbance	-1.158*	-0.914
	(0.587)	(0.622)
Intimidation or verbal abuse among students	0.144	0.516
	(0.600)	(0.636)
Physical fights among students	-0.998*	0.203
	(0.574)	(0.609)
Panel C: Teacher and Student Assessment		
Curriculum expertise	0.041	0.029
	(0.594)	(0.630)
Students' performance	0.455	0.680
	(0.582)	(0.617)

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 belongs to the Catholic sector and 0 if it belongs to the State sector. Sample includes 72 school principals 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 * $< .1$, ** $< .05$, *** $< .01$.

Table F3. Effect of Single-sex School on Students Inputs

	Girls-only Schools (1)	Boys-only Schools (2)
Panel A: Student Satisfaction & School Bullying		
Student satisfaction	0.096	0.127***
	(0.151)	(0.047)
Absenteeism	-0.101***	-0.055
	(0.036)	(0.056)
Bullying index	-0.028	-0.187
	(0.171)	(0.210)
Panel B: Teacher-Student Relationship		
Teachers at my school are fair to me	-0.092	-0.120
	(0.130)	(0.125)
Panel C: Instructional Practices		
Aligned pedagogical practices	-0.145	-0.142
	(0.108)	(0.149)
Panel D: Self-assessment and confidence		
I usually do well in reading	-0.004	0.065
	(0.057)	(0.073)
Reading is easy for me	-0.133***	-0.038
	(0.036)	(0.131)
Reading is harder for me than any other subject	-0.015	-0.222
	(0.068)	(0.136)
Reading is harder for me than for many of my classmates	-0.109	-0.229*
	(0.067)	(0.137)
I am just not good at reading	0.155**	-0.269*
	(0.070)	(0.138)

Notes: Table reports OLS estimates of the effect of single-sex school on the outcome variable shown in each row. Regressions include an indicator for girls-only schools, an indicator for boys-only schools and the interaction between a catholic sector indicator and an indicator for female student. As such, coefficients in column 1 (2) are interpreted as the effect of single-sex education for girls (boys) relative to girls (boys) students at coeducational catholic schools. Sample includes 3015 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 * $< .1$, ** $< .05$, *** $< .01$.