Do school admission policies provide equal opportunities? Three empirical studies on educational inequality

Maria Constanza Gonzalez Parrao

A dissertation submitted in partial fulfilment of the requirements for the degree of

Doctor of Philosophy

of

University College London

Institute of Education, Department of Social Science

April 2020

Declaration

I, Maria Constanza Gonzalez Parrao, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the work.

Chapter two “Are lotteries the best chance for the success of students and schools? A systematic review and meta-analysis of school randomised admissions”, is jointly co-authored with Dr Gabriel Gutierrez and Associate Professor Dr Alison O'Mara-Eves, both from the Department of Social Science at the UCL Institute of Education. As the guarantor of this study, I mainly developed all stages of the systematic review, led the analyses and drafted the final manuscript. The work of Dr Gutierrez and Dr O'Mara-Eves was primarily as a guidance and comparison points. Hence, I contributed with the vast majority of this work.

Abstract

The present thesis focuses on the topic of education inequality at the school level. It aims to explore how different school admission policies can hinder or promote equality of opportunities in education. To achieve this, the first research chapter, co-authored with Gabriel Gutierrez and Alison O'Mara-Eves, serves to contextualise from an international perspective the extent and impact of randomised school admissions. Through a systematic search, this paper synthesises the evidence with a narrative summary and a meta-analysis of the effects that school systems or programs with random admissions have on academic and non-academic areas of students. The results focus on the countries and types of schools or programs that use lottery admissions, as well as on the purpose, implementation and evaluation of these admissions in the available literature. The other two papers examine the relationship between education inequality and admission policies in Chile's school system. Under the premise that the more the schools can shape their student body through the admission process, the more segregated the students will be, the second research chapter looks into selective admissions in primary and secondary education. Following a panel of schools and using a flexible differences-in-differences approach and a linear regression model with school and time fixed effects, this paper evaluates the effect of using different selective admission mechanisms on student academic and school socioeconomic outcomes. Finally, the third research chapter looks into the case of a new school offering primary and secondary education. Using a unique dataset and exploiting lottery admissions to allocate the entire student body, this paper estimates the effect of this educational program on student outcomes. In the context of a school system transitioning to a national admission scheme including a random mechanism, this research provides suggestive evidence of the challenges that new schools could face under the admissions reform.

Impact statement

In order to maximise the impact that this thesis could have, I identify three potential ways in which this impact could be materialised in the short and long term.

Empirical evidence to broaden the public discussion on school admissions

The major contribution of this thesis is providing rigorous quantitative evidence on different school admission practices and their policy implications. Because of the recently enacted School Inclusion Act (2015), the Chilean education system is facing a transition period in terms of the school admission processes. The implementation of this reform has become a hot topic both inside and outside academia. Therefore, the current national debate could benefit from this new information.

More generally, in the context of an increase in school choice systems internationally, and in an effort to generate evidence-based education policy, these studies could serve in the long term for any school system interested in changing its admission processes.

Inside the academic world, this research could highlight the effects of having selective admissions on student and school outcomes, as well as introduce the international evidence gathered around lottery admission systems. This impact could be brought about through conference presentations and seminars, or through dissemination outputs such as peer-reviewed journal papers. Outside academia, these findings could help to engage with policymakers to emphasise the strengths of the new admission system and their implications for families, students, and schools.

Methodological standard in the educational research field

Methodologically, the systematic review and meta-analysis chapter provides a rigorous, transparent and consistent piece of research. This study could set a benchmark for the development of systematic reviews and meta-analysis in education research in Chile, where these methodologies are still not commonly used. This potential impact could be materialised in the form of a peer-reviewed academic publication and its subsequent dissemination within the academic community.

Connecting research to the school community

The research on the school lottery case (chapter 4) could have a direct impact as an evidence-based input for the development of the school that served as the research sample.

Part of the conversation with their staff when asking for and obtaining the data was to put this knowledge at their service in a way that made sense to their school community. Therefore, this academic effort could potentially have an impact outside academia in the form of presentations of the study to the school staff, or readable/friendly reports for the school community.

Acknowledgements

As every PhD student could assert, this process is full of positive and yet other less wonderful experiences. The path is as challenging as immensely rewarding, and it leads to a professional and personal growth that would otherwise be difficult to accomplish. And as we all know, it takes a village…

I would like to thank my supervisors Lorraine Dearden, Gill Wyness, and Alison O'Mara-Eves for their guidance, constant support, and for always setting the bar high. I feel very fortunate to have had the opportunity of working under their watch.

In the same line, I sincerely appreciate my examiners' comments and suggestions. Their input helped me improve this final version of my thesis.

This process would not have been possible without the support from the Chilean National Commission for Scientific and Technological Research (CONICYT) and the UK Economic and Social Research Council (ERSC). I appreciate their trust in my work and hope that after their initial sponsorship, I will keep making a contribution to social science research. Additionally, I would also like to acknowledge the Chilean Ministry of Education's *Centro de Estudios* for their assistance with the datasets for my research, and the school used in the third research chapter for letting me analyse their data.

Family and friends are ultimately the ones who get you through this. I am grateful for the unconditional support from Felipe, Pepa, Javier, and my family in Chile; your routine questions about when I would submit pushed me the right amount to get this done. My IOE team – Gabriel, Rodrigo, Sara and Alejandra – was also essential in this journey. Our feast sessions, mixing the frivolous with the intellectual, were a constant challenge to do better and helped me realise that we would all, eventually, finish this run with success.

Last but not least, this work is dedicated to my favourite people: Elena, Camilo, Eloísa, Agustín, and Clemente. There is no better motivation than imaging that you, too, could have better and equal educational opportunities.

Contents

List of figures

List of tables

1. Introduction

1.1. Why study school admissions?

The present thesis originates by looking at overarching questions about school admissions. How should school places be assigned? How much of this decision should be handed to parents or education authorities? Which allocation system is better? The answer to this last question will depend on what we understand by *better*. One example would be to allocate students automatically to their nearest school. This would entail a straightforward system but could increase school segregation if the neighbourhoods are also segregated. An alternative example would be one where parents could indicate their school preference and take these into account when allocating students. This system would provide a voice to families by letting them decide the type of education they want for their children; however, if many families prefer the same school, a tie-breaking mechanism needs to be in place, which inevitable, will leave students out of such school. Then, it follows that there is no one school admission system that could satisfy all educational actors.

As there seems to be a consensus that education is an important part of people's lives, school admissions are an unsettled topic because it merges policy with moral values and has different implications for families, schools, and authorities (Coldron et al., 2008). Whether a school system focuses more on social diversity, community building, academic excellence, or another educational principle, its admission system will reflect such principles. Then, it is not trivial which school admission system is in place, as it will mirror values that are set as priorities by communities. These ideals could be within education, such as fairness (Hargreaves, 1996), but could also relate to values that transcend education, such as social mobility (Burgess & Briggs, 2010).

A study of the school admission processes among OECD countries identified at least twelve ways to allocate students, which could be grouped into three main mechanisms: student selection by academic means, student allocation by an authority, and parental choice (Smithers & Robinson, 2010). Although these mechanisms could be used simultaneously in different combinations and extents, their implications will vary because, precisely, they are grounded on different values and purposes.

The first of these mechanisms, academic selection, is a practice used worldwide. Although it is more commonly used in secondary education, the age of students at which this is implemented varies across countries, ranging from 10 to 16 years old (OECD, 2013). The aim of

this type of student allocation is to sort students according to their educational abilities and needs and, therefore, boost the academic performance of those students who are most able. The second mechanism, student allocation by an authority, would relate to the assignment of students according to their residence area. This could be understood as a practical way to allocate students by, for example, preventing them to commute to and from schools. However, this mechanism is less commonly used, particularly on its own. Indeed, by looking at the policies of the last 15 years, schools are defining their admissions less on residential criteria and more on student selection (OECD, 2019).

Within the third mechanism, which is also called school choice, parents are given the option to choose their school of preference. Nevertheless, given the capacity limits of each school, there will always be an issue between the demand and supply of school seats as some will be under and others will be oversubscribed. Moreover, as the pool of available schools increases, both the choice of parents and the allocation of students become progressively complex. Under the need of tie-breakers, residential criteria are commonly used, and admission lotteries are an additional option along with first-come-first-served, religious affiliations, and admission codes defining the prioritisation criteria (Smithers & Robinson, 2010). Because the allocation of students under school lotteries is based on a random process, school lotteries would be able to minimise discrimination and other sorts of student selection. Then, their grounding value would be to provide education equality in the access to education (Belfield & Levin, 2005).

The school education system in Chile is based on school choice. Although the residential criteria do not apply in this context, forms of the other two general admission mechanisms are in place (i.e. student selection and parental choice). These are the school admissions areas where this thesis will focus.

This research is motivated by two aspects, the idea that student allocation is associated with key values in education – justice and equality – and the lack of evidence surrounding school admissions in Chile. Discussing my positioning as a researcher is relevant in this context as it will be connected with the recognition of this thesis as valid evidence around school admissions (Elliott, 2005; Jafar, 2018). By understanding the drive behind why and how this research was conducted, the relevance of its findings could be better judged. These studies were defined under the conjecture that those schools cherry-picking students would not be providing an educational value to their students, which would ultimately have a detrimental effect on them and the school system in general. Therefore, what motivates this thesis is generating new and robust evidence to influence policymaking into considering admission mechanisms that could benefit students and the system as a whole. By using quantitative research methods, these

hypotheses would be tested and, either way, would provide evidence of the implications that using different admission mechanisms would have on students and schools.

The Chilean school system, then, contextualises the motivation for this thesis. As such, the next section describes its key characteristics and policies, as well as its connection with the main research questions that this thesis aims to address.

1.2. The school system in Chile

In the 1980s, under dictatorship, most social services in Chile were restructured into market systems. For the education sector, this meant the introduction of a school choice scheme aimed to provide families with the opportunity to choose the best school for them according to their preferences. By making schools compete for student enrolment, school choice systems would promote the improvement and efficiency of the whole school system (Barrera-Osorio & Patrinos, 2009; Friedman & Friedman, 2002).

With the implementation of school choice, the educational context was consolidated under three major school types: public schools, which are funded and run by a public administrator and account for roughly less than half of the schools in the country; private-subsidised schools, which are publicly funded and run by a private administration, accounting for slightly more than half of the schools; and private schools, which are privately run and funded by their own families, and represent just a small proportion of the schools.

The Chilean school choice system is funded through a student voucher that the Ministry of Education transfers directly to schools once students are enrolled. The implementation of this voucher in the Chilean context has its particular features (Gallego & Sapelli, 2007). First, it can be used in any school receiving public funding regardless of the characteristics of such school. Second, the voucher represents the baseline funding for schools, but it may also be topped-up with a co-payment from families. Third, from 2008, the subsidy amount varies according to the student's social background. Finally, it allows some forms of student selection, at least until the last education reform in 2015.

There are several education laws from the Chilean school context that are relevant to this thesis. *Table 1.1* summarises the main acts included, in one way or another, throughout these studies. The Preferential School Subsidy Act (Ministry of Education, 2008) introduced an increased voucher for "priority students", which are defined by the Ministry of Education as those students whose socioeconomic characteristics do not allow them to succeed in their schooling. As a reference, this preferential subsidy accounts for approximately 70% more than the regular student voucher (Ministry of Education, 2012a).

Aiming to have "a school system characterised by the equity and quality of its service" (Ministry of Education, 2009) the General Education Act replaces the previous education law enacted in 1990, at the end of the Pinochet dictatorship. For this reason, this Act has rather broad objectives, such as promoting and providing free pre-school education, supporting and strengthening public school education, creating the Education Quality Agency to measure the quality and equity of the school system at the national level and the Superintendence to oversee the implementation of educational regulations at the national level, and protecting the basic rights of families and students along their schooling. Within this last area, the Act regulates the schools' admission process.

The General Education Act prevented the selection of students up to 6th grade (12-year-olds) due to either academic performance or their socioeconomic background. However, schools could select students under any criteria from 7th grade (13-year-olds). Also, this Act provided independence to schools to define their own admission process, requiring only that their procedures would be known beforehand by the community.

Table 1.1 Summary of Education Acts

Education act	Main changes
Preferential School Subsidy Act (2008)	• Increased voucher for vulnerable students
General Education Act (2009)	• Promote pre-school education • Support public education • Create the Education Quality Agency and Education Superintendence • Protect the rights of families and students
School Inclusion Act (2015)	• Schools are required to be non-profit • Gradual elimination of co-payment • Unified and centralised school admission system

In turn, based on the principles of "transparency, inclusive education, universal accessibility, equity, no arbitrary discrimination, and school choice" (Ministry of Education, 2015), the School Inclusion Act introduces three structural changes to the school system. First, it requires for all publicly-funded schools to be non-profit. This way, it restricts the use of public

resources exclusively for educational purposes. Second, it proposes a parallel movement of school funding. Along with gradually increasing the student voucher, it decreases the co-payment that schools may charge to families until this extra-fee is eliminated. Third, the reform makes a fundamental shift in the admissions process regulation, from each school to the Ministry of Education. Instead, it implements a unified and centralised admissions system for all schools that receive state funding. This last area is the focus of this thesis.

The new admission regulation specifies that schools need to admit all applicants if they have places available. In case of oversubscription, applicants with siblings in the school, children of school staff, and students classified as vulnerable by the Ministry of Education have priority admission. Subsequently, a randomised process assigns the remaining places by taking into account the school preferences of each applicant. Therefore, this random component would give the same chance to all applicants to study in the school of their choice.

The implementation of the new system started immediately, and in 2016 it was already rolled-out in one region of the country (including approximately 60 schools) and only for specific grades: PK to 1st grade (5 to 7-year-olds), 7th grade (13yo), and 9th grade (15yo). Along with incorporating more and larger regions each year, from its second year of implementation, the admission system was used for allocating students in additional school grades. By the start of the school academic year in March 2020, all new students in the country will have used the new centralised admission system.

With the School Inclusion Act, publicly-funded schools in Chile will become non-selective. This is a significant change for its education system, particularly in terms of the principles of school choice and competition under it was created.

1.3. Thesis questions and research chapters

Given the equality purpose of the Chilean new school admissions system, this thesis is framed under two main questions. First, to what extent do different school admission practices promote or hinder equal opportunities to access school education. Second, to what extent equal access opportunities can be translated into equal learning opportunities from a more comprehensive perspective. Then, it is crucial to know more about the international evidence available on this topic, as well as the pre-reform context in the Chilean school system in order to adjust expectations regarding the reform.

In order to address these overarching questions, this thesis presents three different studies. The first research chapter, co-authored with Gabriel Gutierrez and Alison O'Mara-Eves, aims to review the scope of the evidence on the impact of lottery school admissions on student achievement. By exploring where these school admissions are used, how are they implemented,

and how have they been evaluated in the literature, this study provides a systematisation of the international context for the extent and overall impact of randomised admissions. The analyses were based on a systematic literature search under rigorous standards, which generated a descriptive map of the evidence, a narrative synthesis of its outcomes, and a meta-analysis of its effects.

The results of the quantitative synthesis show a positive but small effect on math and reading performance and high levels of heterogeneity within studies. While analyses tried to account for these differences, the results seem statistically robust. Moreover, the review's evidence does not present quality concerns as it is mostly exempt from risks of bias and shows a degree of conceptual consistency among its primary studies.

The second research paper aims to contextualise the introduction of the new admissions system in Chile by exploring how selective admission practices have an effect on students' academic performance and the socioeconomic composition of schools. The analysis focuses on the use of entry tests and parent interviews as archetypes of student selection by individual ability and family background. Using panel data, I am able to account for the use of selective admission mechanisms over time. Then, through a flexible difference-in-differences approach with two-way fixed effects, I exploit the changes in the use of these school admission policies.

Results suggest that, on average, selective schools would not be having a substantial academic benefit from selecting students. When testing for heterogeneous effects, results indicate that private and private-subsidised schools seem to be driving the academic effects in 4th grade (10-year-olds). In turn, the few public schools that always select in 8th grade (14-year-olds) show considerable academic gains and seem to attract similar students from higher socioeconomic status.

The third study aims to inform this ongoing reform by evaluating the case of a new private-subsidised school using lottery admissions before the centralised system was in place. By exploiting its lottery data and the fact that the school was oversubscribed, I estimate intention-to-treat effects of being able to enrol in the school on student academic outcomes. I also analyse the implications these effects may have regarding the new admissions policy scenario.

Results indicate that despite the high interest that the school generated in the community, the analyses on different academic outcomes show negative effects for students who enrol in the school compared to students who did not. Nevertheless, as the school establishes over time, these differences become statistically non-significant.

Through these three empirical studies, this thesis adds to the discussion about school admission policies at the international, country, and school levels. Particularly, its major academic contribution is providing rigorous and empirical evidence using different quantitative methodologies to address the thesis overarching research questions. In addition, the thesis also

contributes in terms of discussing the policy implications to inform public debate and policy-making processes on this topic.

1.4. Ethical considerations

The three research chapters of this thesis are based, one way or another, on data collected from human participants: the first research used completed studies on school admissions as its input data; the second research was based on secondary data provided by the Chilean Ministry of Education; and the third research used school data linked to administrative data from the Chilean Ministry of Education.

For this reason, and as part of the research standards of University College London, ethical approval was sought at the beginning stage of these studies. This ethics review procedure put together details of each research project, such as descriptions of the participants to be included and the methods to be used, aiming to identify potential issues and discuss preventive actions to minimise such threats. These forms were then assessed and approved by an internal reviewer (typically one of the thesis supervisors) and an external reviewer (an additional researcher within the same Department). Because of the low ethical risks posed by these studies, none had to be referred to UCL's Research Ethics Committee. The ethics review forms can be found in section *Z* of the Appendix.

The main ethical topics involving the three studies presented in this thesis are related to confidentiality and anonymity concerns, data storage and security issues, and the dissemination and use of its findings.

First, the analyses presented in this thesis were based only on anonymised data and each of these datasets was used solely for research purposes. The systematic review included aggregated data from already available studies. The rich panel data composed for the second study consisted of student- and school-level data that was already anonymised by the Ministry of Education. Furthermore, the data provided by the school for the third research chapter was not initially anonymised; however, the final dataset used for the analyses was anonymised at the student level. Consequently, the thesis poses a low threat to confidentiality breaches and anonymity issues since the results presented cannot be traced back to individual students or schools.

Secondly, the storage and access to the datasets used for each study followed the UCL guidelines to minimise its loss or misuse. Due to the detailed nature of the data used in the three studies, the data files were encrypted and stored in institutional online drives. Then, the security of these datasets was assured by restricting its access only to the main researcher (myself). Finally, along with using these data for research purposes only, the use of its findings is also

framed within the academic and policymaking domains. Therefore, the main results and policy implications of these studies are expected to have broad dissemination and reach both academic and public availability.

Overall, the studies in this thesis represent a low to no risk for human participants since, following institutional standards, several ethical implications have been addressed and measures have been taken to minimise potential threats to participants or data protection.

The thesis is structured as follows: Chapter 2 presents the systematic review and meta-analysis of school randomised admissions. Chapter 3 includes the study on selective admissions in Chilean schools. Chapter 4 introduces the final research paper, which is focused on a school lottery admission case in Chile. Then, Chapter 5 presents a final discussion incorporating all three research papers. Chapter 6 lists the bibliography used in the thesis and Chapter 7 presents the ethics review forms submitted to UCL for each of the research chapters. The last three chapters include the Appendix material for Chapter 2, Chapter 3, and Chapter 4, respectively. Each reference to the Appendix sections is clearly identified in the main body of the thesis with numeric citations.

2. Are lotteries the best chance for the success of students and schools? A systematic review and meta-analysis of school randomised admissions[1]

2.1. Introduction

2.1.1. Background

A lottery is a decision-making process in which the outcome cannot be predicted (i.e. it is random) and cannot be influenced by those who use it or implement it. The use of lotteries for different social decisions is not recent. It can be traced back to medieval times with a religious purpose, and even in our modern lives there are examples of the use of these randomly-based decisions in several social contexts: trial jury selection, the start of sports games, military drafts, the distribution of tickets for highly requested events, or the allocation of school places in education. The common value of this use and one of the key contributions of lotteries is that they give justice to the decision-making process. However, while this mechanism guarantees equality of opportunities, it does not necessarily generate equality in the resources assigned (Duxbury, 1999; Stone, 2008).

In the context of primary and secondary education, randomly-based decisions are commonly set on school choice systems and mainly aim to solve the issue of student allocation into schools. School choice systems base their development and efficiency on a broad and diverse supply of schools and a competition dynamic among these schooling alternatives. In many cases, this type of school system goes in hand with an extension of the private sector and the use of vouchers to spend public funding on a school chosen by families. School choice policies began to be introduced in the second half of the 20th century. Some regions of Canada and Australia started introducing school choice regulations in the ’60-’70s (Heyneman, 2009), Chile’s national school vouchers reform began in 1981 (Mizala, 2007), and Minnesota was the first state to establish a school choice plan in the U.S. in 1987 (Hill & Jochim, 2009).

Moreover, lotteries have been incorporated into the schools' admission processes in different cases and forms. Some examples include the United States’ Charter Schools Program, which requires the use of lotteries in case of oversubscription of students (U.S. Department of Education, 2002); schools in England are allowed to use random allocation of students, but not

[1] Co-authored with Gabriel Gutierrez and Alison O'Mara-Eves.

as the main admissions criterion (Department for Education, 2014); New Zealand schools allocate students combining lotteries with residential and other student-grouping criteria (Sutton Trust, 2007); the voucher school system in Sweden also resorts to admission lotteries in cases of oversubscription (Stone, 2008); and more recently, Chile's school reform includes a new centralised admission system with a random tie-breaking component for all schools with public funding (Ministry of Education, 2015). Regardless of the particular implementation of school lotteries in these cases, they all aim to provide non-discriminatory access to education.

In a school choice framework, the greater the school offer, the more complex becomes, on one hand, the decision of families regarding which school(s) to apply to, and on the other, the allocation of students into these schools given their limited capacity. However, only if schools are unable to select among their applying students, the school choice principles of competition and efficiency would be accomplished. Otherwise, the influence of schools on their admission process outcomes – for example, in the form of enrolment on a first come, first served basis, or through the use of interviews to parents or entry tests to students – would lead to a segregated system (Betts, 2005; MacLeod & Urquiola, 2009; Musset, 2012).

Then, lotteries could be critical to regulate admission policies and help pursue the goal of equity in education under school choice systems (Belfield & Levin, 2005; Hill, 2005; Social Market Foundation, 2004; Stone, 2008; Walford, 1996). These random mechanisms would be able to remove discrimination or handpicking of students in the admission process and would also eliminate the ability of parents to ensure a school place due to, for example, a housing decision or an interview outcome. Hence, compared to school systems with other admission processes, the use of randomised admissions has the potential to increase the heterogeneity of students, both at the academic and socioeconomic levels.

Randomly-based decisions are proposed to generate a fair outcome because all applicants are assumed equal, yet these processes are commonly challenged for the absence of consideration of what applicants need or deserve. If there are grounds to differentiate students in a school admission process then a randomly-based decision, by its own, would not provide equal treatment of school places. This is why lotteries are frequently used in combination with other admission mechanisms, for example, as priority criteria of admission where certain student characteristics (e.g. eligible for free school meals) are prioritised in the process (Sutton Trust, 2007). Even in these cases, once priorities are resolved, the remaining students can be assumed to be in equal conditions and then a random allocation would, in theory, provide a just decision-making mechanism in the access to education.

Although we are aware of some individual school programmes or policy evaluations including school random admissions (Allen, Burgess, & McKenna, 2013; Cullen, Jacob, & Levitt, 2006; Deming, Hastings, Kane, & Staiger, 2014; Hoxby & Rockoff, 2005), these show mixed

results in terms of student achievement and include a range of other academic (e.g. different levels of education attained) and non-traditional (e.g. arrest rates) outcomes. Moreover, and to the best of our knowledge, there are no research efforts to consolidate the international literature available rigorously and systematically and synthesize the effectiveness of this evidence regarding school lotteries. There are, however, two related studies which share either the research method or research topic with this review and could serve as indirect precedents.

The first related study is a meta-analysis on the achievement effect of private voucher programmes with an international approach (Shakeel, Anderson, & Wolf, 2016). It includes 19 studies from eleven different voucher programmes, and it consolidates their effectiveness using pupil math and reading outcomes. The study finds an overall positive and statistically significant achievement effect of private school vouchers, with heterogeneous effects by subject, location, and funding type. The focus of Shakeel et al.'s review is private scholarship programmes, that is, the offer of funding to attend a private school of choice. One of this review's inclusion criteria required the use of randomised controlled trials, however, the lottery in these voucher programmes does not necessarily decide a school place in an admission process but rather the opportunity to be offered a scholarship. Hence, though this study partly shares our proposed methods, it does not answer our research question. In addition, and as an external review of the research indicates (Lubienski, 2016), the goal of having an international focus is not well achieved as it ultimately includes studies from three countries and the vast majority of them are from the U.S., which undermines the usefulness of the meta-analysis.

The second related study is focused specifically on randomised admissions in education (Stasz & Von Stolk, 2007). The research starting point is the UK's School Admissions Code draft which, for the first time, allows schools to use lotteries to manage their vacancies. Given the scarce evidence on this topic, the study focused on lottery schemes in four different countries, considering their purpose, implementation, and evaluations. This exploratory research finds mixed results on the effect of random admissions on student achievement and few evaluations of these schemes on equity, arguing that these types of outcomes are not generally intended or examined. While the study shares some common motivations and research questions with this proposed review, it is not based on the principles of systematic reviews; hence its results may not be replicable, representative, or account for other research biases. The authors conclude that "further research is required to understand how lottery schemes operate in different contexts and what the associated impacts are" (p.vii).

In summary, randomised admission procedures at the school level have been introduced in different countries and contexts with the goal of reducing inequalities in the access to education. Yet, while it has the potential to offer equal educational opportunities, the existing literature on the topic do not provide a clear answer as to whether this promise is met or what

would this imply in practice for students and schools. Given the lack of rigorous academic efforts to examine the evidence on school randomised admissions, this systematic review proposes to build up the literature and inform researchers, government agencies, school systems, and families involved with school lotteries. In the context of an increased offer of school choice schemes internationally (Musset, 2012), this review becomes of special relevance for its education policy implications. The review would also be of benefit as we anticipate that our research strategy will accomplish a more comprehensive international perspective that would help to fill the current evidence gap on school lotteries.

2.1.2. Objectives

The review aimed to, firstly, map and systematise the evidence available on the impact of school systems or educational programs using randomised admissions. Secondly, it intended to synthesise evaluations of the effect of randomised school admissions on student academic performance and school socioeconomic composition measures to determine the impact of such policies.

The main research question guiding the systematic review was: what is the scope of the evidence available on the impact of randomised school admissions on student achievement, the socioeconomic composition of schools, and a range of other outcomes that may be associated to the admission process? To inform this, secondary research questions focused on (i) where are these school admissions used; (ii) which schools use this type of admission, for what purpose, and how are they implemented; and (iii) how has this type of school admission been evaluated in the available literature?

The following PICO (Population, Intervention, Comparator, Outcome) elements specify the boundaries of interest for these research questions; in the Methods section to follow, we operationalised these into eligibility criteria (and added further parameters) that were used to standardise decisions about the relevance of research to the review's questions.

Population

The population of interest was based on two groups. First, primary and secondary schools, regardless of their administration type (public, private, other) and how they are financed (publicly, privately, via vouchers, mixed funding, other). This group was the focus of the descriptive mapping of the evidence retrieved. The second group of interest were students at primary and secondary schools, which was expected to be the base for the synthesis of results.

Intervention

The review's intervention consisted of school admission processes based on a random assignment of students. This included admission processes where all or some students are randomly selected, where other admission criteria are combined with the school lottery, and where the random admission is part of either a specific educational programme or a broader school policy. Therefore, we anticipated seeing heterogeneity in the range of studies on which the review would be based.

It is worth noting that the offer of scholarship vouchers at the school level was not incorporated as part of the review's intervention. These mechanisms do not randomise school places and students accessing the school through such scholarships may differ in observable and unobservable characteristics from the rest of students in the school.

Comparison groups

Three comparison groups were considered for the review, although studies needed to focus on at least one of them to be eligible. First, other similar areas such as school districts, states, or councils that used alternative, non-random types of school admission processes. This applied when an entire area (instead of a particular school or group of schools) used randomised admissions. Secondly, schools in similar areas that used another type of admission process. This applied when the study focused on a particular area and compared schools within such an area. Finally, the third comparison group were students that applied to a school with random admission but were not assigned a place. This applied when a study concentrated on schools with randomised admissions and compared students who applied to those schools. Examples of other non-random types of school admissions include residential criteria or catchment areas, selective admissions by student ability, or religious criteria.

Outcomes

The primary outcomes of interest were focused on educational achievement and school socioeconomic composition measures. Previous literature suggests that the effect of educational programmes may differ by topic of study (Chabrier, Cohodes, & Oreopoulos, 2016; Krowka, Hadd, & Marx, 2017). Hence, school- or student-level academic performance measures were considered for any subject (i.e. reading, math, sciences, etc.). Additionally, these measures could be in the form of national tests or instruments specially designed for the purpose of the research. As this is a commonly used outcome, we expected studies to present these measures as standardised test scores or to provide sufficient information to standardise the academic achievement measure reported. For the latter, we were interested in school socioeconomic composition measures as indicators of segregation in education. These could be reported as a

socioeconomic index value, as a measure of variation within/between schools, or as another indicator of segregation in schools.

Secondary outcomes were considered contingent to the results of the screening process; in other words, secondary outcomes were largely determined by what was measured in the primary studies. While all outcomes were extracted for the purpose of the evidence map, only those that were considered logically related to the random admission process were included in the subsequent in-depth synthesis. Following the relevant literature already identified, examples of potential secondary outcomes included school graduation rates, school absenteeism measures, or student socioemotional measures.

2.2. Methods

A protocol for this review was produced, peer-reviewed and published in May 2018[2]. The systematic processes of literature search and screening of studies were conducted as outlined in the protocol unless stated otherwise. Both the protocol and this review follow the PRISMA reporting guidelines (Moher et al., 2015; Moher, Liberati, Tetzlaff, & Altman, 2009; Shamseer et al., 2015).

The review process was managed with the online software EPPI-Reviewer 4 (Thomas, Brunton, & Graziosi, 2019), which provides tools for collaborative work in handling references and analysing data. The quantitative synthesis was conducted using the R general package for meta-analysis (Harrer, Cuijpers, & Ebert, 2019; Schwarzer, 2007).

2.2.1. Eligibility of studies

We aimed to be as comprehensive as possible in gathering relevant literature. Hence, the review did not restrict the search of references by publication status and targeted both peer-reviewed and grey literature. The eligibility of studies was be determined by the following hierarchical criteria:

Release timeframe

Studies had to be published from 1970 onwards. As previously discussed, the international development of school choice policies began to appear around this decade, so we did not expect to identify eligible studies before this year. Then, establishing this time limit allowed for early evaluations to be considered in the mapping of evidence while also keeping the content of the studies relevant.

[2] Link to access the published protocol https://doi.org/10.1016/j.ijer.2018.05.001

Language

Following the resource constraints of the review, studies written in English or Spanish were considered. When available, this eligibility criterion was applied directly as a search limit in each information source.

Focus on randomised school admissions

The main or secondary topic of the studies had to involve the school's admission process, and specifically, the random allocation of students into schools. For example, if the main focus of a study is a specific educational program which additionally comprises the use of random assignment of students into schools, then it was still eligible for the review. In the initial screening stage by titles and abstracts, this feature needed to be explicitly mentioned in the information available for each reference.

Empirical studies

Studies had to include an analysis of empirical data, either at the school or student level, and either from primary or secondary sources. The review excluded theoretical discussions, educational policy reports, opinion pieces or similar documents that did not contain empirical data analyses.

Quantitative study design

Studies had to have a quantitative research component with a measured effect of the review's intervention of interest (i.e., random assignment of students into schools) against a comparator, as previously outlined in Section *2.1.2*. Design types considered include controlled experiments, before-after evaluations, matching techniques, and similar. Mixed-methods designs were not initially excluded provided that the quantitative part abided by this criterion.

Outcomes within school years

Studies had to report at least one outcome for any stage within primary and/or secondary school education, such as those specified in section *2.1.2*. Since the education levels may differ according to the school system, we accounted for these differences in the analyses in order to have comparable outcomes. Following the Population of interest described in section *2.1.2*, the review did not focus on preschool or higher education outcomes; however, references were not initially excluded if the random assignment of students occurred at the preschool education level but the outcomes were measured in later in time.

2.2.2. Information sources

Table 2.1 shows the list of databases and search engines that were considered for the literature search phase, including 17 peer-reviewed catalogues (of which eleven have a specific geographic focus, and three also include grey literature), three open access sources, two institutional databases and three dissertation archives. Additionally, references from systematic reviews found in the search stage were also examined to identify further potentially relevant primary studies.

A first search was done between December 2016 and January 2017, which yielded 86% (n=8,867) of the total references considered in the search strategy. A second search was conducted in October 2018 aiming to bring up to date the relevant literature; hence, it focused on new records available between January 2017 and the search date. The update literature search provided 14% (n=1,460) of the records considered in the review, which, taking into account the shorter timeframe than the original search, suggests that this is a research topic of increasing interest. More details on the relevance of updating the review literature can be found in section *8.1* of the Appendix.

Table 2.1 List of information sources to include in literature search

1. Educational Resources Information Center (ERIC, ProQuest, development database)
2. African Journals Online
3. American Doctoral Dissertations (EBSCO)
4. Australia and New Zealand Database (ProQuest)
5. Australian Education Index (ProQuest)
6. British Education Index (EBSCO)
7. Campbell Collaboration
8. Dissertations and Theses Global (ProQuest)
9. East & South Asia Database (ProQuest)
10. East Europe, Central Europe Database (ProQuest)
11. Education Abstracts (EBSCO)
12. Education Database (ProQuest)
13. EThOS (e-theses online service)
14. German Education Portal
15. Google Scholar (in English and Spanish)
16. India Database (ProQuest)
17. Institute of Education Sciences
18. JSTOR
19. Middle East and Africa Database (ProQuest)
20. OpenGrey
21. PRISMA Database (ProQuest)
22. Scientific Electronic Library Online (SciELO, in English and Spanish)
23. Social Science Database (ProQuest)
24. UK and Ireland Database (ProQuest)
25. Web of Science

Note: unless stated in the table, the literature search in each source was conducted in English.

2.2.3. Search strategy

The first objective of the review is to map the evidence of school lotteries evaluations. Therefore, we purposely developed a search strategy with a general set of terms that was education-focused but simple enough to capture relevant literature.

Free-text fields (title and abstract) searches included the same main structure to ensure a consistent process. However, the strategy was adapted for each database or electronic source according to its searching functions and saving/retrieving system.

In a first cluster of terms, the free term *random* was suspected to gather references out of the scope of schools and education. By conditioning its use with the terms *admission* or *lottery*, the search was more likely to focus on school admissions studies. For this, we used the search function "NEAR" or "ADJ" (adjacent), when available and adapted accordingly, on the research databases and search engines. In a second cluster of terms, the free terms *school* and *student* were expected to capture relevant literature related to the population of interest of the review. In a third cluster, alternative terms related to quantitative research were used to detect empirical studies. Finally, each search included, when available, year and language limits according to the eligibility criteria. *Table 2.2* exemplifies the search strategy used for the development database.

Table 2.2 Example of search strategy for ERIC (ProQuest) database

Search cluster focus	Search terms
Intervention	"random" NEAR3 "admission" OR "lottery"
	AND
Population	"school" OR "student"
	AND
Research design	"evaluation" OR "effect" OR "impact" OR "gain"
Additional limits in first search	Years: from 1970 / Language: English, Spanish
Additional limits in update search	Years: from 2017 / Language: English, Spanish

To compensate for a potential over-simplicity of the search strategy, we took additional measures to find relevant literature by (a) including 25 information sources with different geographical focus and considering both peer-reviewed databases and grey literature, and (b) checking the references lists of systematic reviews found in the literature search. Moreover, the search strategy was first piloted in the development database and resulted in a number and range of references that the review team deemed as appropriate. For example, the set of studies already identified and discussed in the background section was entirely included in the pilot search results.

The literature search was conducted by Reviewer #1 (R1) and a log was created for each search. Section *8.2* of the Appendix shows further details on the cases and rationale for adapting the original search strategy, the results yielded in each information source and the detailed search procedure for two databases with retrieving restrictions.

2.2.4. Studies selection process

R1 conducted the references search, gathered the results in one dataset and removed duplicates. To ensure the inclusion criteria were clear and adequate before the screening by title

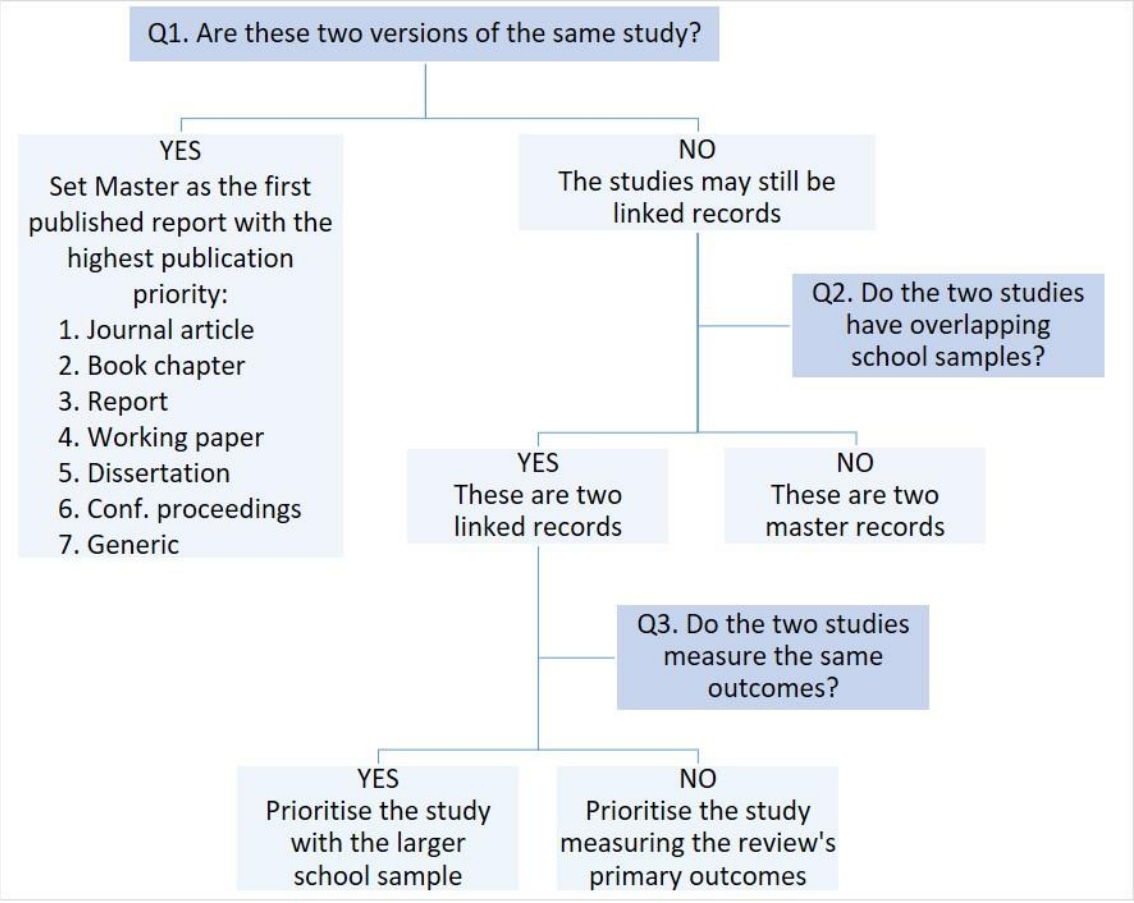
and abstract, R1 and R2 carried out a pilot test by double-screening a random set of 100 references. This exercise yielded an agreement rate well above the 95% threshold.

Once the inclusion criteria had been tested and agreed, R1 screened by title and abstract all search results and R2 double-screened a random sample of 30%, coding the exclusion criteria and any relevant detail for each reference. The agreement level in this process was assessed according to the previous standards, and disagreements were discussed and amended as necessary.

R1 retrieved full papers of all included references and records were kept for not retrievable studies. Subsequently, and using the same criteria, R1 screened by full text all included references and R2 double-screened a random sample of 30%. In this stage, they again identified and coded the reason for each excluded reference. The level of agreement between the researchers was assessed once more following the previous standards, and disagreements were discussed and amended accordingly.

For the purposes of transparency and consistency, the review team created classification rules for defining master and linked records, which are necessary when more than one publication relates to an evaluation of the same data. R1 surveyed the included studies and identified these references. *Figure 2.1* shows the decision tree for these guidelines.

Figure 2.1 Rules for defining master and linked records



2.2.5. Data extraction process

A three-step process was conducted on each included reference after the screening by full report: data extraction to map the studies and inform the synthesis stage, coding for quality and relevance appraisal of primary studies, and calculation of effect sizes for the synthesis of results (Oliver & Sutcliffe, 2012).

Included studies were coded using an extraction tool developed by the team of researchers based on codesets of public access: the Cochrane risk of bias tool (Chandler, McKenzie, Boutron, & Welch, 2016) and other code examples available in the online management tool EPPI-Reviewer 4 (Thomas et al., 2019). The data extraction tool included three main sections: (1) General Information, covering administrative details of the study; (2) Aim and Sample, providing details on the objective and contexts of the study; and (3) Evaluation, summarising the data and methods used in the study analyses and results. Appendix section *8.3* includes the coding tool with all the variables extracted for each record.

For the calculation of effect sizes, and as anticipated, the outcomes were mainly measured as continuous variables; hence we calculated standardised mean differences, with 95% CI, between treatment and control groups (Lipsey & Wilson, 2001; Thomas, O'Mara-Eves, Kneale, & Shemilt, 2017), being these areas, schools, or students according to the comparison groups discussed in section *2.1.2*. For reliability purposes and to avoid potential biases in the effect sizes, the research team defined iteratively a set of rules and assumptions for the calculation of effect sizes, and the process was supported whenever necessary by appropriate calculation software (Wilson, n.d.). Appendix section *8.4* details these agreed guidelines.

R1 performed the data extraction and calculation of effect sizes for all primary studies and R2 conducted an independent effect sizes calculation for a random sample of 30% of references. The outcome data for this subgroup of studies was compared, discussed and reconciled when necessary. In cases where R2 did not agree with a study code, the researchers discussed and reached a final resolution.

2.2.6. Risk of Bias assessment

As the review aims to synthesise the results of studies that have minimised their risk of bias (Harden & Gough, 2012), a critical appraisal of primary records was conducted according to the following criteria: selection, baseline imbalance, identification, compliance, attrition, contamination, sample reporting, and outcome reporting bias. This assessment tool was adapted from codesets of public access: the Cochrane risk of bias tool (Chandler et al., 2016) and other code examples available in the online management tool EPPI-Reviewer 4 (Thomas et al., 2019). Three answer categories were given for each bias dimension: high risk, low risk, and unclear. The research team agreed to categorise each study as with a high or low risk of bias and

use the third option only in extremely unusual cases in which there was genuinely insufficient information for making a judgement. Finally, the results of the individual assessment of these risks of bias were translated into an overall concern judgement (high, moderate or low) for each primary study.

Appendix section *8.5* includes the assessment tool, which defines the concepts evaluated and provides examples for the answer categories. R1 conducted the risk of bias assessment for all primary studies. R2 did an independent risk of bias assessment for 30% of references based on the data extracted from studies. The answers for this subgroup of records were compared, discussed, and reconciled when necessary.

2.2.7. Synthesis of results

As a first step to inform the synthesis of results, all included references were part of a descriptive mapping of the evidence. The in-depth analysis of the review synthesised the evidence on school randomised admissions through a narrative synthesis of the outcomes measured in the review literature, and a statistical meta-analysis of a range of those effect sizes.

We took this combined approach to synthesise the results in order to maximise the contribution of each master record towards the review's research questions and to better understand the similitudes and differences within this set of evidence, which would later help contextualise the results of the quantitative synthesis (Borman & Grigg, 2009). Hence, while all master records are contained within the narrative synthesis, the inclusion criteria for the meta-analysis were refined. See the results section *2.3.5* for details on these criteria.

Descriptive mapping of studies

Based on the data extraction tool, we produced a descriptive map using mainly tabular forms to summarise the key characteristics of the included studies and provide a general picture of the review's relevant literature. The features to highlight for the mapping were contingent on the primary studies found.

Narrative synthesis of outcomes

For the narrative synthesis, we used a thematic synthesis of the results from all included studies. The themes were inductively developed from the primary studies and the coding was performed by R1. The robustness of the thematic synthesis of results was checked based on the groups of primary studies that contributed to each theme and the relevance of these relative weights in relation to the review's questions (Thomas, O'Mara-Eves, Harden, & Newman, 2017).

Quantitative synthesis of outcomes

We calculated the effect of schools using randomised admissions for the review's primary and secondary outcomes. If there were at least five records reporting relevant and comparable outcome measures, we meta-analysed these effects and presented the results using forest plots.

Since the review intended to collect evidence on school randomised admissions from a global perspective and from different educational programmes, we expected random differences between the included studies. To account for this between-studies heterogeneity, the meta-analysis used random-effects models with the Hartung-Knapp adjustment to provide more conservative estimates (Borenstein, Hedges, Higgins, & Rothstein, 2009; Lipsey & Wilson, 2001). In addition, the I^2 statistic was calculated to measure inconsistency in the subset of studies included in each of the in-depth analyses conducted (Higgins, Thompson, Deeks, & Altman, 2003).

Moreover, the results of the data extraction and critical appraisal informed the pertinent subgroup and sensitivity analyses to evaluate the variation of findings across studies associated with their research designs and risk of bias characteristics (Harden & Gough, 2012).

To explore conceptually relevant patterns and the statistical heterogeneity in the findings, subgroup analyses were conducted considering, first, when data was imputed or assumptions were made when calculating effect sizes. This analysis was conducted to test whether effect sizes that were calculated using imputed values for missing pieces of information were systematically more conservative or larger compared with effect sizes for which complete information was available. We also performed analyses to test if the calculation of effect sizes was systematically different according to particular features of the studies. We tested different sample characteristics and methodological approaches, given by the studies' intervention as described in section *2.1.2*, the school type they focus on, the samples' geographical location, the studies' analysis strategies, sample sizes, and/or groups of covariates used to calculate the estimates.

In cases where statistically different subgroups were identified, we tested if such variables could predict the estimated effect size using meta-regressions. The year of publication was also included in these analyses as a continuous variable. We assessed its results by paying attention to both the statistical significance of the predictor and the total variance explained through the R^2 analogue for meta-analyses (Borenstein et al., 2009).

Additionally, to examine the robustness of the review findings, sensitivity analyses were performed in terms of assessing the relative weight of potential outliers according to funnel and/or forest plots, and by excluding relevant studies following the subgroup analyses. When conducting these consistency analyses, special attention was given to whether the direction, the

significance, or the magnitude of the original effects was sensitive to any of these variables described (Thomas, O'Mara-Eves, Kneale, et al., 2017).

2.2.8. Meta-biases

If studies finding no effects of the intervention are less likely to be published, then the literature set of a systematic review could be vulnerable to publication bias. Although we tried to minimise publication bias by not discriminating by publication status on the studies' eligibility criteria, and there is no established method for definitively demonstrating the presence or absence of publication bias, we assessed the potential for this risk of bias in two ways: first, by comparing effect sizes of published studies with those unpublished studies (i.e., grey literature) in our sample, and secondly, by conducting a correlation analysis between the effect size and the primary study's sample size. For the former, a systematically larger effect size for published studies compared to unpublished studies may indicate the presence of publication bias in this topic. For the latter, a statistically significant negative correlation might indicate that small effect sizes are associated with larger samples, which would follow the predictions of publication bias, specifically, that small effect sizes based on small sample sizes did not have enough statistical power to reach statistical significance and were consequently not published (O'Mara-Eves & Thomas, 2016). We did not consider funnel plots to explore the potential presence of this bias since its visual inspection and asymmetry test demand a large number of primary studies and does not uniquely justify that its results may be due to publication bias (Thomas, O'Mara-Eves, Kneale, et al., 2017).

Additionally, a range of other potential biases (selection, baseline imbalance, identification, compliance, attrition, contamination, sample reporting, and outcome reporting) were integrated into the critical appraisal of all references included in the in-depth analysis. These potential biases were individually assessed using meta-regression analyses, and the overall risk of bias for each primary record was taken into account using subgroup analyses, comparing studies with high, moderate and low concerns of risk of bias.

2.2.9. Quality and relevance of the review

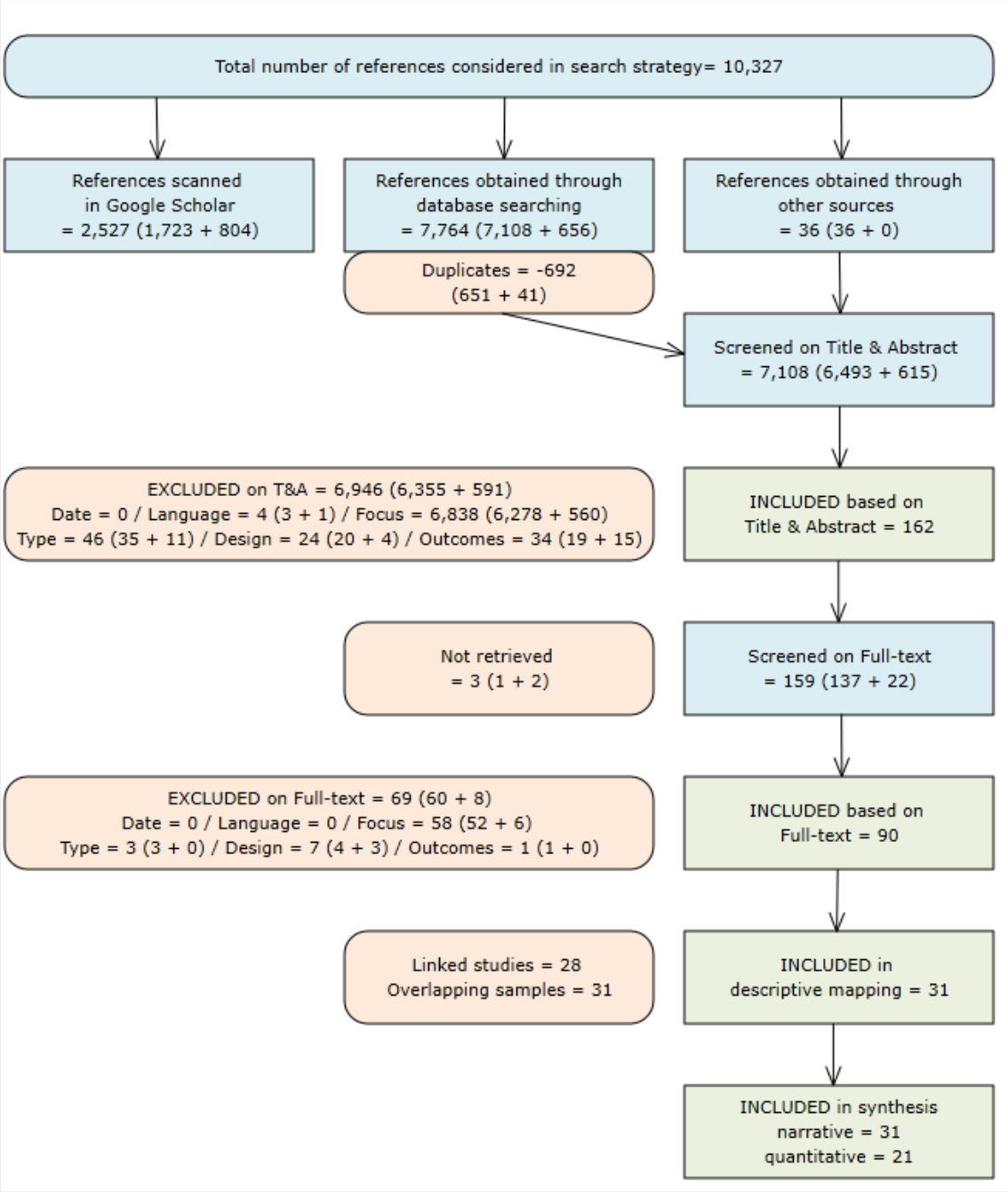
The quality and relevance assessment of the review followed the Weight of Evidence framework (Gough, 2007). We appraised the overall strength of the evidence used to address the review's research questions by reflecting on individual and particular judgements. First, we assessed the general quality of the review by looking at its execution process. Second, we considered the appropriateness of its design and the scope of its focus to evaluate the review's "fitness for purpose" and discuss their implications for addressing its research questions.

2.3. Results

2.3.1. Selection of studies

Figure 2.2 shows the flow chart for the selection of studies considered and used in the review. Each box shows the combined number of references from the initial and update searches; the additional figures in parenthesis specify the results of the first and update search, respectively.

Figure 2.2 Selection process of the review references



Note: T&A = title and abstract. When applicable, the first figure shows the total number of records for each search stage. The figures in parenthesis detail the number of records for the first and update searches, respectively.

The review considered more than 10,000 references when taking into account all the stages of the studies selection process. Three-quarters of those were identified and retrieved through database searching, whereas the rest were references scanned in Google Scholar.

Additionally, a total of 36 new records were identified through other sources during the first literature search, while none were retrieved from other sources in the update search. These new references derive from two types of sources: linked versions of the same study, and references used in relevant systematic reviews and "quasi-reviews". Section *8.6* of the Appendix provides further details about these cases. Overall, the low number of records obtained by alternative sources compared to the total number references identified through database searching, and the fact that the review had already identified the majority of records used in relevant systematic reviews and "quasi-reviews" suggests that our search strategy successfully detected the relevant literature to address the review questions.

As stated in the protocol, 30% of the references were double-screened based on title and abstract and on full text, aiming to achieve at least 95% agreement. Section *8.7* of the Appendix summarises the screening of references for each search phase, including the disagreements between R1 and R2. In the few cases of coding disagreement, the researchers discussed and reconciled the answers, involving R3 into the discussion whenever necessary.

A total of 162 records were included based on title and abstract, of which three dissertation records could not be retrieved. These references were published between 1973 and 2018, most commonly as peer-reviewed journal articles (38%). Of those screened on full text, 57% (n=90) matched the inclusion criteria and 69 were excluded. The majority of these records were excluded on focus (n=58), were not empirical studies (n=3), did not have a quantitative design (n=7), or measured outcomes outside the school years (n=1). Section *8.8* of the Appendix details the reason for exclusion and publication type of these not retrievable and excluded references.

Finally, of the 90 records potentially included by full text, 31 were unique studies (see section *8.9* of the Appendix with a list of these references). This set acted as our master records and was the base of the synthesis of results. The reduction of references between those included by full text and the final number of master records was related to either linked studies or studies with unknown or multiple-location samples. It is not uncommon in this field to see multi-state samples, or for data from the same schools to appear in different research papers. To avoid double-counting samples, the review team created and followed a rule to prioritise studies with known samples and/or studies with a broader focus on school types. Then, due to our eligibility and extraction rules, the studies that were not identified as master records were a combination of linked studies (i.e. two versions of the same research, n=28) and overlapping samples (i.e. two studies with total or partial sample overlapping, in which cases the study with the larger school

sample size was prioritised, n=31). See section *8.10* of the Appendix with this distinction for each of these records.

2.3.2. Descriptive map of evidence

To contextualise the evidence, the descriptive mapping addresses and is structured according to the secondary research questions of the review. *Table 2.3* presents the characteristics of included studies (n=31) following the PICO elements. This section focuses mainly on the population, intervention, and comparison groups, as defined in section *2.1.2*. Subsequently, the narrative and quantitative synthesis of the results intend to answer the main research question by focusing on the outcomes of primary studies.

Where are random school admissions used?

Among the 31 master records, we found one study from each of China, Netherlands, England, and Uruguay, while the rest of the studies identified (n=27) are from the United States, covering twelve different states of the country. Since the school system of each of these states operates under more or less different rules, we are able to account for a variety of educational contexts within that country. It is worth noting that 22% of the U.S studies (n=6) have unreported locations, mainly due to confidentiality issues.

Which types of schools use lotteries?

More than half of the included studies (n=17) focus on schools within the public sector and around 40% study charter schools or their equivalent; namely, schools that are publicly funded but are independently run by an educational foundation, charity, or private entity. Finally, there is one study (#7 in *Table 2.3*) that focuses on the experience of randomised admissions in a private school.

Interestingly, within each of these categories, there is also a range of different types of schools. Within public schools, we found studies on vocational schools, with transition programs to higher education, with intensive bilingual programs, with greater independence in the curriculum, with reduced total enrolment, and for gifted and talented students. Within charters or their equivalent, there are Montessori schools, non-profit and for-profit schools, with boarding programmes, or with a “no excuses” academic focus.

Table 2.3 Characteristics of included studies

#	Study	Location	Population (school type, sample size, ages evaluated)	Intervention (policy and randomisation levels)	Comparison group	Outcomes measured
1.	Abdulkadiroglu (2011)	Boston, USA	14 pilot (public) schools students 8-16 years old	Educational system policy - All places after priority	Students	Academic performance
2.	Abdulkadiroglu (2013)	New York City, USA	101 small (public) high schools students 14-17 years old	Educational system policy - All places after priority	Students	Academic performance Absenteeism Credits High school graduation
3.	Abdulkadiroglu (2017)	Denver, USA	31 charter schools students 9-16 years old	Educational system policy - All places after priority	Students	Academic performance
4.	Allen (2013)	Brighton and Hove, England	9 secondary (public) schools students 12-14 years old	Educational system policy - All places after priority	Areas	School quality School socioeconomic composition
5.	Angrist (2013)	Massachusetts, USA	23 charter schools students 10-16 years old	Educational system policy - All places after priority	Students	Academic performance Absenteeism Disciplinary High school graduation
6.	Ballou (2007)	Unknown, USA	5 magnet (public) schools students 10-12 years old	Educational system policy - All places after priority	Students	Academic performance
7.	Balsa (2016)	Montevideo, Uruguay	1 private school students 12-13 years old	School policy - All places after priority	Students	Academic performance Absenteeism Disciplinary Drop-out Educational expectations Retention School climate

#	Study	Location	Population (school type, sample size, ages evaluated)	Intervention (policy and randomisation levels)	Comparison group	Outcomes measured
8.	Bifulco (2009)	Connecticut, USA	2 inter-district magnet (public) schools students 13-14 years old	Educational system policy - All places after priority	Students	Academic performance
9.	Bui (2014)	Unknown, USA	2 magnet (public) schools with GT programmes students 12-13 years old	Educational system policy - All places after priority	Students	Academic performance
10.	Cook (2017)	Unknown, USA	undetermined number of magnet (public) schools students 12-13 years old	Educational system policy - All places after priority	Students	Academic performance
11.	Crain (1992)	New York City, USA	91 career magnet (public) school programmes students 14-15 years old	Educational system policy - A percentage of places (50%)	Students	Academic performance Absenteeism Credits Drop-out
12.	Cullen (2006)	Chicago, USA	19 open enrolment (public) high schools students 14-18 years old	Educational system policy - All places after priority	Students	Academic performance Absenteeism Credits Disciplinary Drop-out Educational expectations High school graduation Retention School climate
13.	Cullen (2009)	Chicago, USA	32 open enrolment (public) elementary schools students 6-11 years old	Educational system policy - All places after priority	Students	Academic performance Retention
14.	Culverhouse (2018)	Virginia, USA	4 magnet (public) schools students 11-12 years old	Educational system policy - All places	Schools	Academic performance

#	Study	Location	Population (school type, sample size, ages evaluated)	Intervention (policy and randomisation levels)	Comparison group	Outcomes measured
15.	Curto (2014)	Washington DC, USA	1 boarding charter school students 12-14 years old	Educational system policy - All places after priority	Students	Academic performance
16.	Dynarski (2018)	Michigan, USA	44 for-profit charter schools students 8-14 years old	School policy - All places after priority	Students	Academic performance Absenteeism
17.	Edmunds (2017)	North Carolina, USA	12 early colleges (public) students 17-18 years old	School policy	Students	High school graduation Transition to HE
18.	Engberg (2014)	Unknown, USA	undetermined number of magnet (public) schools students 13-14 & 16-17 years old	Educational system policy - All places	Students	Academic performance Absenteeism Disciplinary
19.	Foreman (2017)	Unknown, USA	10 charter schools students 8-14 years old	Educational system policy	Students	Academic performance
20.	Hastings (2006)	North Carolina, USA	undetermined number of public schools students 8-15 years old	Educational system policy - All places after priority	Students	Academic performance Absenteeism Disciplinary Retention
21.	Hastings (2012)	Unknown, USA	undetermined number of public and charter schools students 8-16 years old	Educational system policy - All places	Students	Academic performance Absenteeism
22.	Hemelt (2017)	North Carolina, USA	1 career academy (public) students 14-18 years old	School policy - All places after priority	Students	Academic performance Absenteeism High school graduation
23.	Hoxby (2005)	Chicago, USA	3 charter schools students 5-14 years old	Educational system policy - All places after priority	Students	Academic performance
24.	Hoxby (2009)	New York City, USA	42 charter schools students 8-14 years old	Educational system policy - All places after priority	Students	Academic performance
25.	McClure (2005)	San Diego, USA	1 charter school students 11-17 years old	School policy - All places	Students	Academic performance

#	Study	Location	Population (school type, sample size, ages evaluated)	Intervention (policy and randomisation levels)	Comparison group	Outcomes measured
26.	Nikolov (2017)	Pennsylvania, USA	1 in-district charter school students 14-15 years old	School policy - All places	Students	Academic performance Absenteeism Disciplinary
27.	Ruijs (2017)	Netherlands	2 Montessori schools students 15-18 years old	School policy - All places after priority	Students	Academic performance High school graduation Retention Socioemotional Transition to HE
28.	Steele (2017)	Portland, USA	12 (public) schools with language immersion programmes students 8-14 years old	Educational system policy - All places after priority	Students	Academic performance
29.	Unterman (2017)	New York City, USA	7 academies students 8-10 years old	School policy - All places	Students	Academic performance
30.	Wong (2014)	Los Angeles, USA	3 high-performing charter schools students 15-18 years old	Educational system policy - All places after priority	Students	Health
31.	Zhang (2009)	Wuhan, China	8 magnet (public) schools students 14-15 years old	Educational system policy - All places after priority	Students	Academic performance

With what purpose are lottery admissions used?

In more than half of the studies (n=21), it is explicitly mentioned that the schools in their sample use random admissions because it is required by the education law. To a lesser extent, four articles focused on public schools also highlight the principle of equality in access to school-level education. However, a quarter of the included references (n=9) do not explicitly report why the school systems they study use lottery admissions.

How have these randomised admissions been implemented?

Seventy-four percent of the included studies use lottery admissions as a school system policy, compared to an individual experience of the school, and 22 studies focus on admission systems where, after a priority admission process, all vacancies are subject to randomization.

Specifically, the most common priority admission criteria within the review studies are when older siblings are already enrolled in the school (n=23), and in the case of public schools, when a residence or proximity to the school criteria is in place (n=9). Although less common, other criteria mentioned in these studies are when an applicant's parent is part of the school staff, when either academic or socioeconomic background documentation is required, and when the applicant comes from a feeder school. Moreover, in four studies there is no priority admission and in another three references, this information is not reported.

Lastly, in eighteen of the studies, the randomised admission systems contemplate a waiting list for when a student who is offered a place does not finally enrol in the school. Yet, similarly to the points above, eleven studies do not report information about this aspect of the lottery admission implementation.

How have random admissions been evaluated?

Most studies use secondary data, generally provided by the same school system, and test results at the national or regional level. Moreover, two-thirds of the studies are based on rather small sample sizes (20 schools or less). One study (#2), uses a sample of more than a hundred schools, and five studies do not explicitly mention in the report the number of schools used in their research.

From the three comparison groups defined for the review, almost all studies (n=29) compare student-level data, whereas one study (#4) used greater geographical areas as comparison groups, and one study (#14) analyse school-level data.

The most common method of analysis, or the most advanced method used in the analyses, are instrumental variables (n=17) and linear regression models (n=7). Other main methods include difference-in-differences (#4), propensity scores (#3), and treatment effect

bounds (#18). Four studies (#11, #14, #25, and #26) use either analysis of variance (ANOVA), multivariate analysis of variance (MANOVA), Chi-square or T-tests to analyse their data.

Related to the analysis methods used, half of the studies report intention-to-treat (hereinafter, ITT) estimates, that is, the comparison of students according to the result of the lottery admission; two-thirds report treatment-on-the-treated (hereinafter, TOT) estimates, comparing students following the compliance of the lottery result; one-third of the studies report both of these analysis strategies and two studies analyse their data in other ways that do not involve these specific strategies.

Finally, most of the included studies use an overall common set of control variables in their analyses, which were categorised in three main groups: (1) demographic and socioeconomic control variables, including gender, age, race/ethnicity, free school meal eligibility, priority status, area, education and poverty measures at the neighbourhood level, parents educational attainment, and number of adults in the household; (2) educational covariates, including primary language of student (measured as language spoken at home, limited English proficiency, etc.), special education status, prior academic achievement, gifted and talented status, previous school attended, absences, suspensions, and retention; and (3) methodological control variables, including lottery fixed effects, year/grade fixed effects, at-risk (of attrition) status, indicator for the presence of a sibling in the lottery, and missing indicators. Moreover, a few studies (#7, #14, #25, and #26) do not use covariates in their analyses.

2.3.3. Risk of Bias in included studies

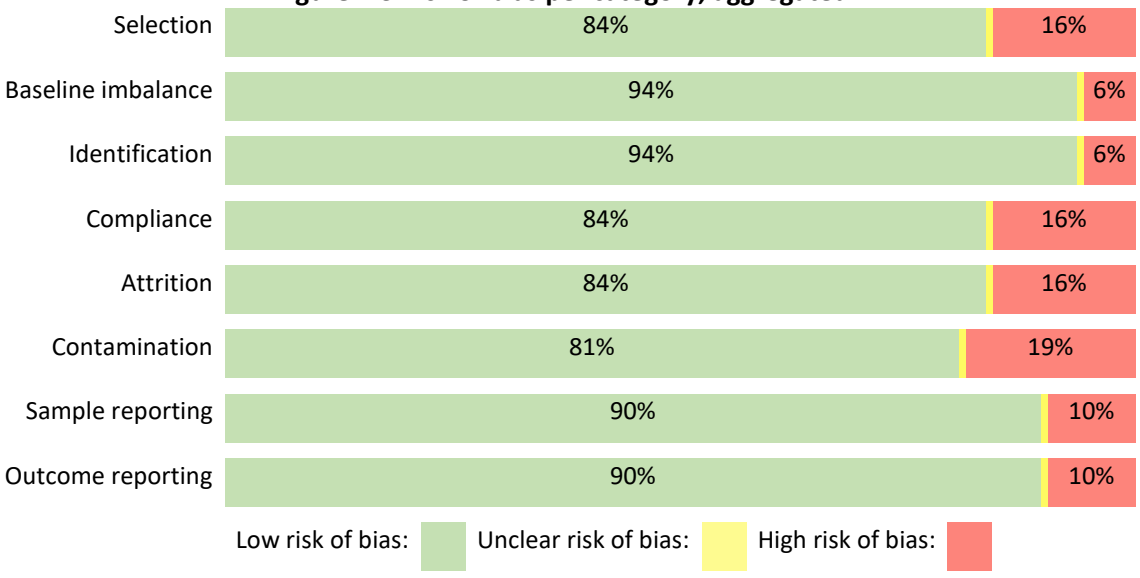
Table 2.4 presents the detailed assessment of each bias category for every included study, where the final column shows the overall judgement for each of these references.

We then analysed the aggregated information by bias category and overall concern, respectively. First, and as shown in *Figure 2.3*, we find that only a few of the primary studies (between two and six) have a high risk of bias in each of the categories defined for this review. The most common prevalence is regarding the risk of having selection bias (when the sample selection is not sufficiently justified, potentially undermining the representativeness of the sample), compliance bias (when the percentage of students who do not comply with the result of the admission lottery is high, or the study does not address appropriately this issue), and attrition bias (when data is lost either partially (minor missing data) or totally (complete loss of observations) in a non-random manner between the comparison groups). It is worth noticing that in the context of this review, a contamination bias would mainly occur through compliance and/or attrition issues, so this category is ultimately the sum of those two.

Table 2.4 Risk of bias assessment for each included study

Study	Selection	Baseline imbalance	Identification	Compliance	Attrition	Contamination	Sample reporting	Outcome reporting	Overall concern
Abdulkadiroglu (2011)	+	+	+	+	+	+	+	+	Low
Abdulkadiroglu (2013)	+	+	+	+	+	+	+	+	Low
Abdulkadiroglu (2017)	+	+	+	+	+	+	+	+	Low
Allen (2013)	+	+	+	+	+	+	+	+	Low
Angrist (2013)	+	+	+	+	+	+	+	+	Low
Ballou (2007)	+	+	+	+	+	+	+	+	Low
Balsa (2016)	+	+	+	+	+	+	+	-	Moderate
Bifulco (2009)	-	+	+	+	+	+	+	+	Moderate
Bui (2014)	+	+	+	+	+	+	+	+	Low
Cook (2017)	+	+	+	+	+	+	-	+	Moderate
Crain (1992)	+	+	+	-	+	-	-	-	High
Cullen (2006)	+	+	+	+	+	+	+	+	Low
Cullen (2009)	+	+	+	+	+	+	+	+	Low
Culverhouse (2018)	-	-	+	-	-	-	+	+	High
Curto (2014)	+	+	+	+	+	+	+	+	Low
Dynarski (2018)	+	+	+	+	+	+	+	-	Moderate
Edmunds (2017)	+	+	+	+	+	+	+	+	Low
Engberg (2014)	+	+	+	+	+	+	+	+	Low
Foreman (2017)	+	+	-	-	-	-	+	+	High
Hastings (2006)	+	+	+	+	+	+	+	+	Low
Hastings (2012)	+	+	+	+	+	+	+	+	Low
Hemelt (2017)	-	+	+	+	+	+	+	+	Moderate
Hoxby (2005)	+	+	-	+	+	+	+	+	Moderate
Hoxby (2009)	+	+	+	+	+	+	+	+	Low
McClure (2005)	+	+	+	-	-	-	+	+	High
Nikolov (2017)	+	-	+	-	-	-	-	+	High
Ruijs (2017)	+	+	+	+	+	+	+	+	Low
Steele (2017)	-	+	+	+	+	+	+	+	Moderate
Unterman (2017)	+	+	+	+	+	+	+	+	Low
Wong (2014)	-	+	+	+	-	-	+	+	High
Zhang (2009)	+	+	+	+	+	+	+	+	Low

Figure 2.3 Risk of bias per category, aggregated



Second, we assessed the overall concern about the potential for a study's findings to be at risk of bias (*Table 2.5*). We deemed that, for this research topic, evidence of compliance or attrition bias would seriously undermine confidence in a study's findings, and therefore any study with a high risk of compliance and/or attrition bias was assigned a rating of "high concern". "Moderate concern" was defined as having a high risk of any other bias, and "low concern" as having only low risks of bias. By combining these categories, the appraisal of primary studies suggests that the literature found is reliable, as more than half of the studies (n=18) have no high risks of bias. In addition, the quantitative synthesis of results takes into further consideration potential differences between studies with a low and high risk of bias.

Table 2.5 Overall risk of bias, aggregated

High concern: high risk of compliance and/or attrition bias	6	19%
Moderate concern: high risk of other biases	7	23%
Low concern: no high risk of biases	18	58%
Total number of included studies	31	

2.3.4. Thematic synthesis of results

The narrative synthesis of results serves to address two of the research questions. First, it complements the information from the descriptive mapping regarding the evaluation of school settings with lottery admission by presenting the variety of outcomes used in the review literature. Second, this section details the impact on these outcomes as reported by the included studies.

The primary outcomes set for this review were educational achievement and school socioeconomic composition, while secondary outcomes would be considered depending on the evidence extracted. *Table 2.6* provides an overview of the outcomes evaluated by primary records. 90% of these studies measure academic performance as the sole or main outcome of interest. To a lesser extent, there is a range of other outcomes evaluated both at the school and student level. It should be noted that this grouping shows broad categories of the outcomes evaluated, and these are not necessarily comparable measures. For example, disciplinary outcomes range from school behaviour reports to arrests by the local police. This section, then, is structured according to the following general categories: outcomes at the school level, non-educational outcomes, other outcomes related to the educational process, and academic performance as the traditional educational outcome. Each section details the level of heterogeneity of outcomes within such category.

Table 2.6 Outcomes evaluated, aggregated

Outcome categories	# of studies
School-level outcomes	
School climate	2
School quality	1
School socioeconomic composition	1
Non-educational outcomes	
Socioemotional	1
Health	1
Non-traditional educational outcomes	
Absenteeism	11
High school graduation	6
Disciplinary	6
Retention	5
Drop-out	3
Credits	3
Transition to higher education	2
Educational expectations	2
Traditional educational outcomes	
Academic performance	28

Outcomes at the school level

A small group of studies evaluate outcomes at the school level. One study measures school quality and socioeconomic composition, which additionally, is the only reference comparing areas. As such, the authors highlight that the catchment areas established along with the lottery admission play a crucial role in the school system since their results suggest that some students do access better quality schools but also there seems to be a small but statistically significant increase in the segregation of students.

Moreover, two studies focus on the students' perception of the school climate, finding different results. While in both settings the perception of safety at the school was significantly better for students in the treatment group, the studies find contrasting results in other measures such as liking or feeling happy at the school and trusting and respecting their teachers.

The results of these studies measuring school-level outcomes are mostly reliable as only one of them shows a moderate concern due to a risk of outcome reporting bias.

Non-educational outcomes

Two studies evaluate non-educational outcomes, focusing on two different measures. One study looks at health outcomes, measured as risky (e.g. use of alcohol, tobacco or drugs) and very risky behaviours (e.g. binge drinking or gang participation). Students in the treatment group showed significantly lower rates of very risky behaviour, but there were no differences with students in the control group in terms of risk behaviour. The authors note that schooling could be seen as a mechanism to reduce risky health behaviours in adolescents.

The other study analyses socioemotional outcomes, as the educational project aims to develop these qualities. However, the author finds no difference in the levels of motivation and independence shown by students in the treatment and control groups.

The results of this section should be taken with some caution. While the study focusing on socioemotional measures is of low concern, the study using health outcomes was assessed as having high concerns of bias due mainly to differential attrition issues with its sample.

Other outcomes related to the educational process

The group of non-traditional educational outcomes include absenteeism, high school graduation, disciplinary measures, retention, drop-out, credits, transition to higher education, and educational expectations. The general rationale for including these types of outcomes is that, if families choose schools for reasons that are not (exclusively) related to academic outcomes, then we could expect to see an effect on these measures that would respond to such expectation.

Two studies evaluate the students' expectations about their educational future, finding opposing results. While one study identifies a statistically significant positive effect on the students' expectation of getting a higher education degree, the other study finds no differences in such educational expectations.

Two studies estimate the effect of their interventions on measures that reflect a preparedness for higher education. One of these educational projects aims precisely to provide a better transition to postsecondary education and finds that students in the treatment group complete almost a year more in college compared to students in the control. The second study measures a more subtle outcome: enrolling in a science program, which would signal a better university readiness. However, the author does not find an effect on the probability of enrolling on this particular program.

There is also mixed evidence among the three studies focusing on drop-out rates. Two of them show that their educational projects significantly reduced drop-out rates, while the other finds that students in the treatment group are no more likely to drop out of school.

Three studies measure credits accumulation towards graduation. One of them finds no differences when comparing students who won the lottery admission with students who did not. In the other two there is an increase in the credits earned, although in one of these studies this result is relevant only for students with average reading performance (and not for students with reading performance below average).

Of the five studies that measure if students were retained in grade, two find that students who win the lottery admission are significantly less likely to repeat a grade, and in the other three studies there is no evidence of an effect of their educational interventions on grade retention.

Six studies focus on disciplinary outcomes, evaluating a variety of different measures. Four of these studies measure suspensions from school with mixed results: one finds a positive effect on both middle and high school students, one study finds a negative effect for entire sample of high school students, one finds a negative effect only for elementary school students, and one study does not find significant effects on suspensions. Three studies also measure in-school disciplinary incidents, showing fewer offenses in the three cases. Finally, one study evaluates out-of-school disciplinary measures, finding that students in the treatment group show fewer arrests and incarceration rates.

Six studies evaluate high school graduation, of which half explicitly measure having graduated on time. Results are mixed: two studies find a positive effect on graduation rates, one finds lower graduation rates for students who win the lottery admission, and the three other studies do not find statistically significant effects.

Eleven studies look at school attendance with a range of different outcomes, from continuous measures of number of days, percentage, or weekly average of days attended/absent, to binary measures of having at least five days of unexcused absences. Seven studies find a statistically significant effect on reducing absences, of which for one study this is relevant only for high school students (and not for students in elementary or middle school). One study find that the intervention is associated with a higher likeliness to be absent from school, although only for students with below-average reading performance. Two studies find no significant differences in attendance, and one study mentions evaluating this outcome but does not report it in the study manuscript.

In summary, there is a range of additional outcomes related to the educational process explored in the literature; the most commonly measured were disciplinary outcomes (six studies), high school graduation (six studies), and school attendance (eleven studies). However, for all these additional outcomes, the findings were inconsistent, which may (at least in part) be attributable to the different ways in which studies operationalised conceptually similar outcomes. The results on these non-traditional educational outcomes are generally of

confidence given that two-thirds of the studies present a low concern, while from the rest, two studies were assessed as having a high concern of bias. The category of bias most prevalent within the studies in this section is the risk of outcome reporting bias.

Academic performance

Using lottery admissions can be associated with signalling a policy of no discrimination. Under the assumption that these schools provide educational value to their students, which would not be conditional to their background or innate abilities, we could expect to see this educational value reflected in academic performance measures.

Of the 28 studies evaluating academic performance, one study evaluates this outcome using a measure of on-time grade progression. While the progression of students is overall high, the author finds no difference between students in the intervention and control groups.

The rest of the studies include subject measures. One study focuses on history, finding positive score gains for students, and one other measures achievement on social studies but finds no significant effects. Moreover, four studies evaluate performance in science, of which two find positive score gains, a third study finds a positive effect but only for non-black and female students, and the fourth study finds effects indistinguishable from zero.

Twenty-three studies include measures of math skills, either as tests focused on the overall subject performance or as tests measuring a specific math knowledge (e.g. algebra). Half of these studies measure ITT estimates, 78% measure TOT, a third measures both strategies and two studies use other ways to analyse their data. The results are mixed as half of the studies (52%) find positive effects on math test scores, while the rest find no statistical differences between students in the intervention and control groups.

Twenty-two studies use measures of reading, writing, or language skills (hereinafter, “reading”). Similar to the outcomes on math, 55% of these studies measure ITT estimates, 77% measure TOT, a third measures both strategies, and one study uses another approach for its data analysis. In 55% of these studies, results suggest positive and statistically significant score gains for students, though in the rest there are no significant differences for students who won the lottery admission.

Finally, six studies assess the students’ average performance, using a combination of the previous subject measures. Four of these studies measure ITT estimates, while two of them measure both ITT and TOT finding no difference in the direction of these estimates. Overall, these results suggest there are no significant differences between students in the intervention and control groups, and only one of the six studies finds a positive and statistical effect on the combined test score for students winning the admission lottery. However, two studies observe interesting distinctions in their analyses: one notes that while the popularity of a school is

positively correlated with its average performance, it is not associated with an individual treatment effect on academic performance; the other study identifies a significantly positive effect only for white female students, which contrasts with negative not significant effects for the other subgroups.

In summary, the majority of studies evaluating academic performance use subject measures, and from these, reading and math skills are the most common outcomes. Moreover, around half of these studies find positive score gains for students. We present these results with confidence since more than half of the studies (57%) have an overall low concern of bias, indicating that they do not show a risk of bias in any of the categories assessed. However, the risks of selection, identification, and regarding the reporting of the sample and/or outcomes are of moderate concern in seven of the 28 studies. Moreover, only five of these studies measuring academic performance are of high concern given their risk of attrition and/or compliance bias.

2.3.5. Meta-analysis

Although there is a range of outcomes evaluated by primary studies, many could not be included in the quantitative synthesis because their measures were too heterogeneous (e.g. absenteeism) or there were not enough studies to conduct a meta-analysis (e.g. high school graduation). Hence this section is mainly focused on the quantitative synthesis of academic performance outcomes.

Of the 28 primary records that evaluate the students' academic performance, 21 include either math or reading outcomes as a continuous measure. Academic performance in science, social studies or history was not considered due to the limited number of studies focused on these measures. Additionally, two studies measure academic performance using binary outcomes (#26 and #27 in *Table 2.3*), three studies only report an average performance measure between subjects (#13, #20 and #31), one study includes a unique comparison group (#14), and the effect sizes could not be calculated with confidence for one study (#29[3]). Hence, 68% of all primary studies, and 75% of studies evaluating academic achievement, ultimately inform the quantitative synthesis of results.

While the majority of these 21 studies report TOT estimates using instrumental variables, a considerable proportion of studies (52%) also estimate ITT. To maximise the contribution of all possible primary records into the quantitative synthesis, we conducted meta-analyses for both analysis strategies and each subject. Therefore, this section is structured according to these four

[3] This study evaluates both intention-to-treat and complier average causal effect. However, the report does not provide enough information to calculate a distinct effect size for each of these estimates. Efforts were made to contact the author, but these were unsuccessful.

combinations of subject and analysis strategy. Section *8.11* of the Appendix shows additional funnel plots to summarise the studies included in each analysis by subject and strategy.

In a general sense, we find positive although small effects on math and reading scores, and as expected, the ITT estimates are smaller than the TOT since these do not account for compliance. The overall effect size for math using ITT estimates is 0.04 (95% CI [0.00, 0.07], I^2=33%, *Figure 2.4*), where we fail to reject the null hypothesis of homogeneity of effect sizes but the weight of the overall effect lays heavily on just a few of its ten studies. Moreover, the overall effect size for math using TOT estimates is 0.10 (95% CI [0.04, 0.16], I^2=86%, *Figure 2.5*). Although this effect is more evenly distributed across the studies, it shows a substantial level of heterogeneity among these 17 primary references.

Figure 2.4 Meta-analysis summary for math, ITT

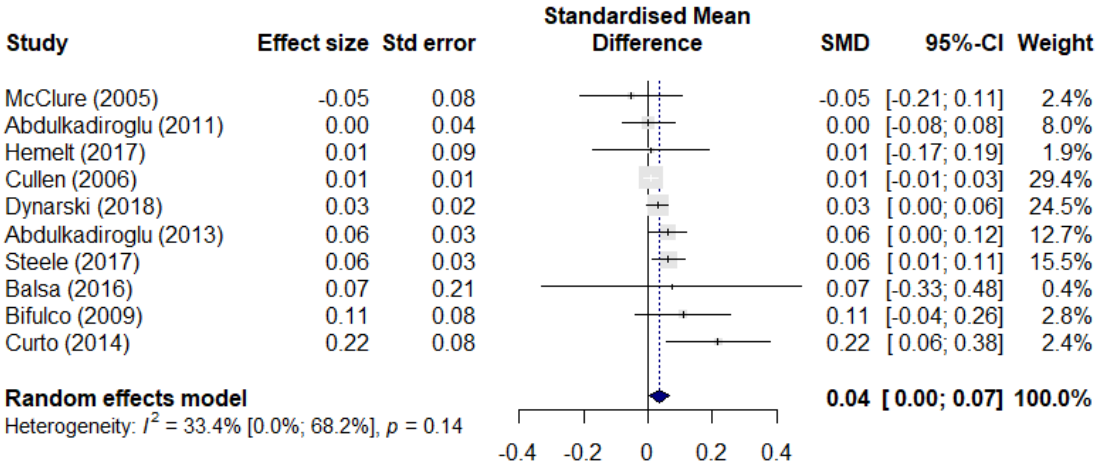
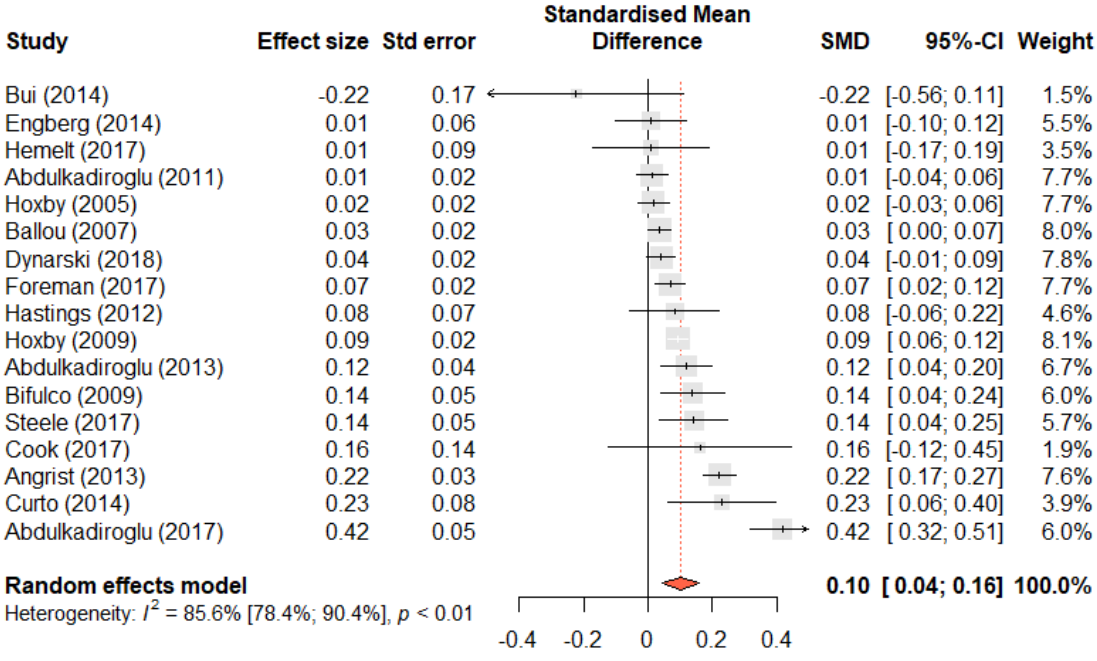


Figure 2.5 Meta-analysis summary for math, TOT



The overall effect size for reading using ITT estimates is 0.07 (95% CI [0.02, 0.11], I^2=79%, *Figure 2.6*), where we can see that the percent of total variation due to heterogeneity across these eleven studies is considerable and some of the studies contribute slightly to the pooled effect. The overall effect size for reading using TOT estimates is 0.10 (95% CI [0.05, 0.15], I^2=80%, *Figure 2.7*), and similarly, shows a high level of heterogeneity within its 16 primary studies despite their uniform weightings.

Figure 2.6 Meta-analysis summary for reading, ITT

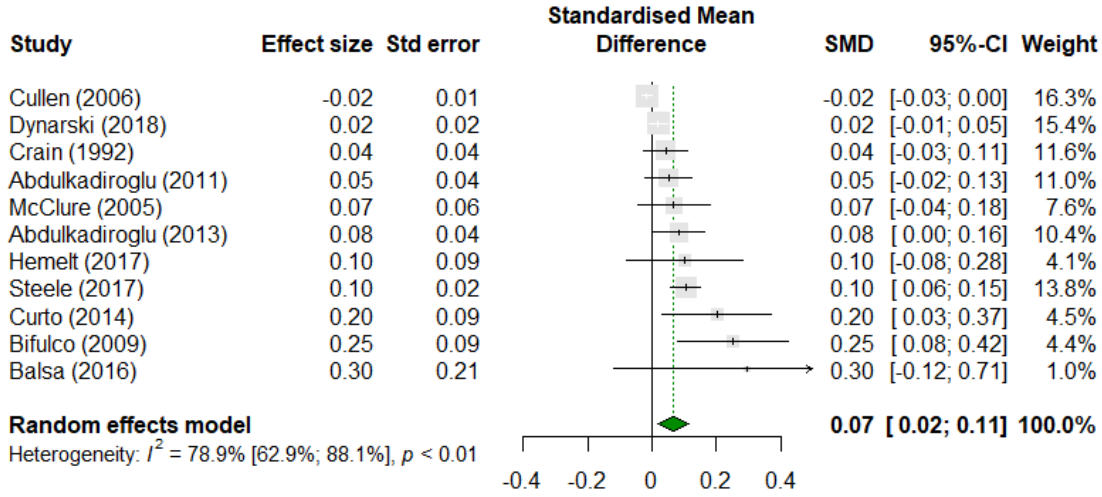
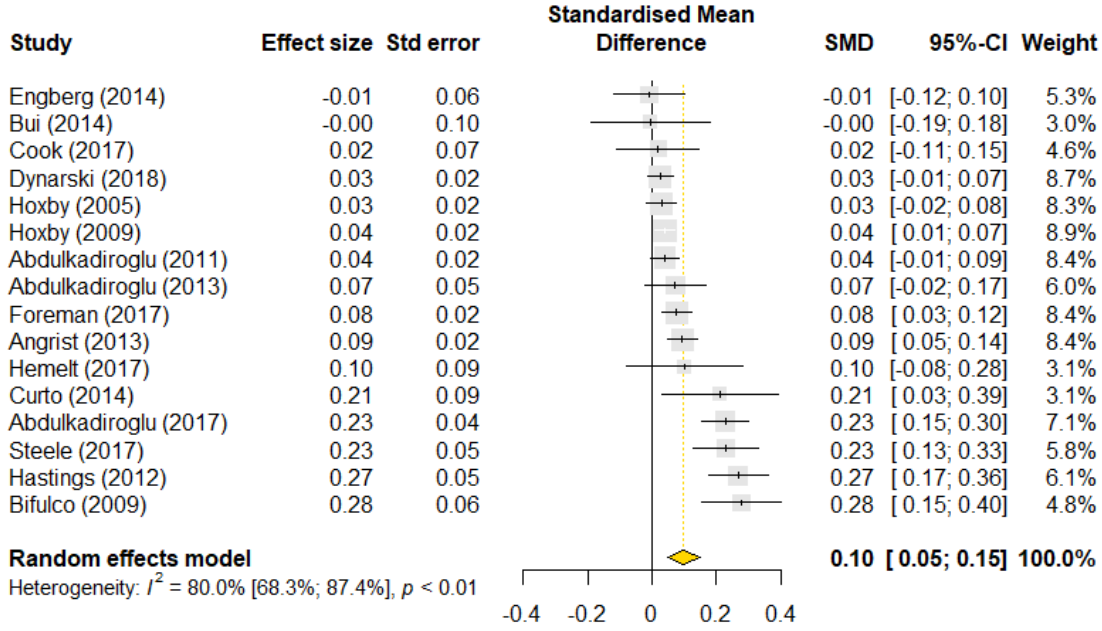


Figure 2.7 Meta-analysis summary for reading, TOT



Sub-group and sensitivity analyses

Subgroup analyses were evaluated following the description in section *2.2.7*. It is worth noting that, at the subject/analysis strategy level, there is little variation in the use of covariates within the primary record; hence, this variable was not included in the subgroup analyses. Section *8.12* of the Appendix shows the breakdown of the use of covariates in the studies included in the meta-analysis. Moreover, subgroups analyses were estimated only for groups with more than one study. Section *8.13* of the Appendix shows the detailed analyses per variable for each subject/analysis strategy. The figures below present these analyses, which are only shown for variables with statistically significant differences at the 95% confidence level and, for reference, the pooled effect is also shown at the bottom of each plot.

Additionally, along with assessing the robustness of the results according to the categories described in section *2.2.7*, we also examined the consistency of the results following the evidence that emerged from the review. Specifically, given that the main synthesis is divided by analysis strategy, we replicated the analyses using the subsample of primary studies that measured both evaluation strategies (n=7).

Math scores

For the synthesis of ITT estimates on math scores, we see that there are subgroup differences related to the methods used by the studies to analyse their data (test for subgroup differences p-value=0.03), and the assumptions made by the review team to calculate the effect sizes (p-value=0.02). *Figure 2.8* illustrates that studies using instrumental variables show a higher effect size (0.05, 95% CI [0.01, 0.09], I^2=27%) compared to studies using other approaches (namely, OLS and other tests such as MANOVA, ANOVA, Chi-square or t-tests). However, the effect size of this second group (0.01, 95% CI [-0.02, 0.04], I^2=0%) relies heavily on one study, suggesting that it would not be an appropriate comparison group. Moreover, further meta-regression analyses indicate that the method approach employed by studies would not be a predictor of the effect size on math (p-value=0.11).

Similarly, as shown in *Figure 2.9*, we can see that two of the studies discussed previously are also the group of references for which assumptions were made when calculating their effect sizes. Therefore, we are able to reach the same conclusion regarding the substantive interpretation of this subgroup analysis, which is also supported by the meta-regression results (p-value=0.10). Finally, the sensitivity analyses summary graph for the effect size in math, ITT (*Figure 2.10*) indicate that, overall, these results are robust against different specifications.

Figure 2.8 Forest plot for math, ITT by subgroups of methods

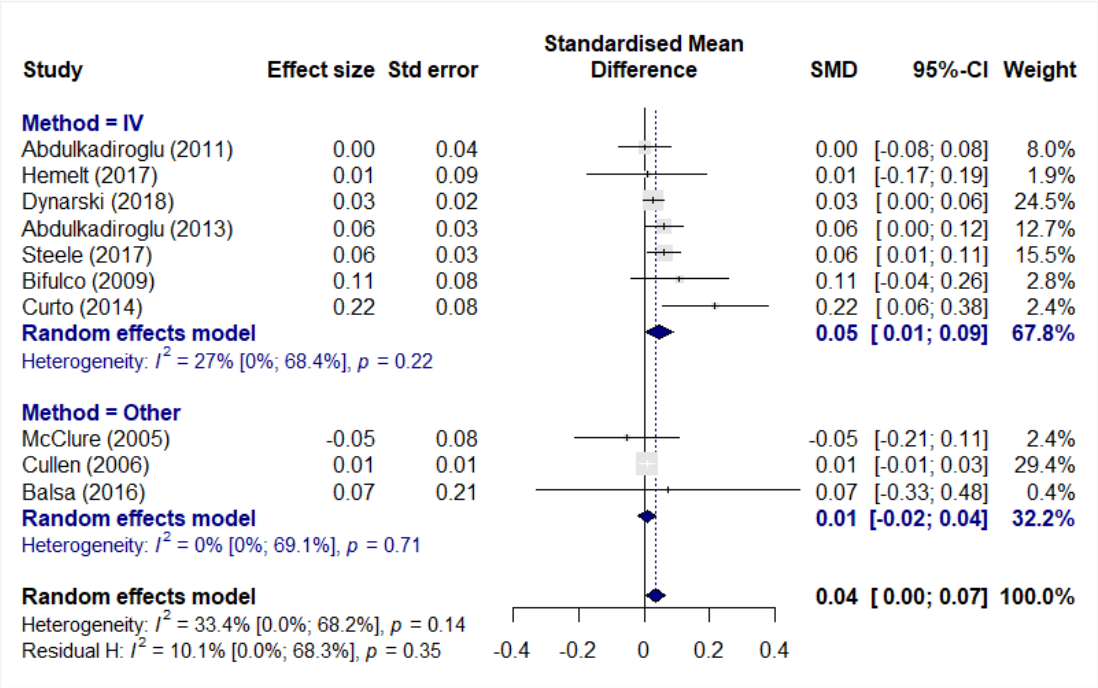


Figure 2.9 Forest plot for math, ITT by subgroups of effect size assumptions

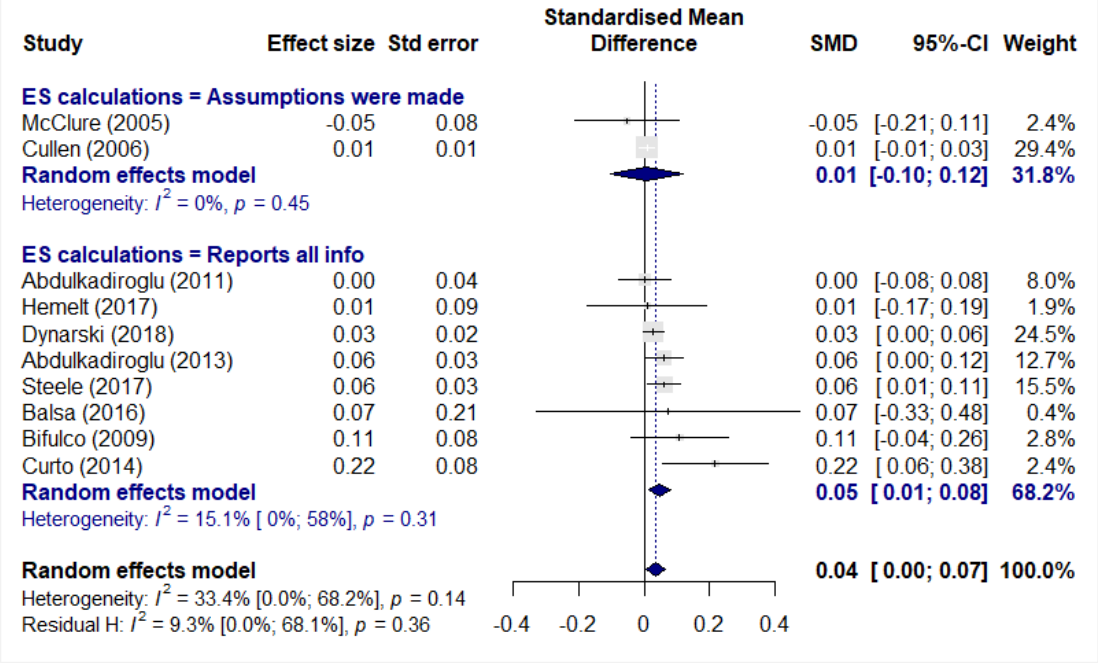
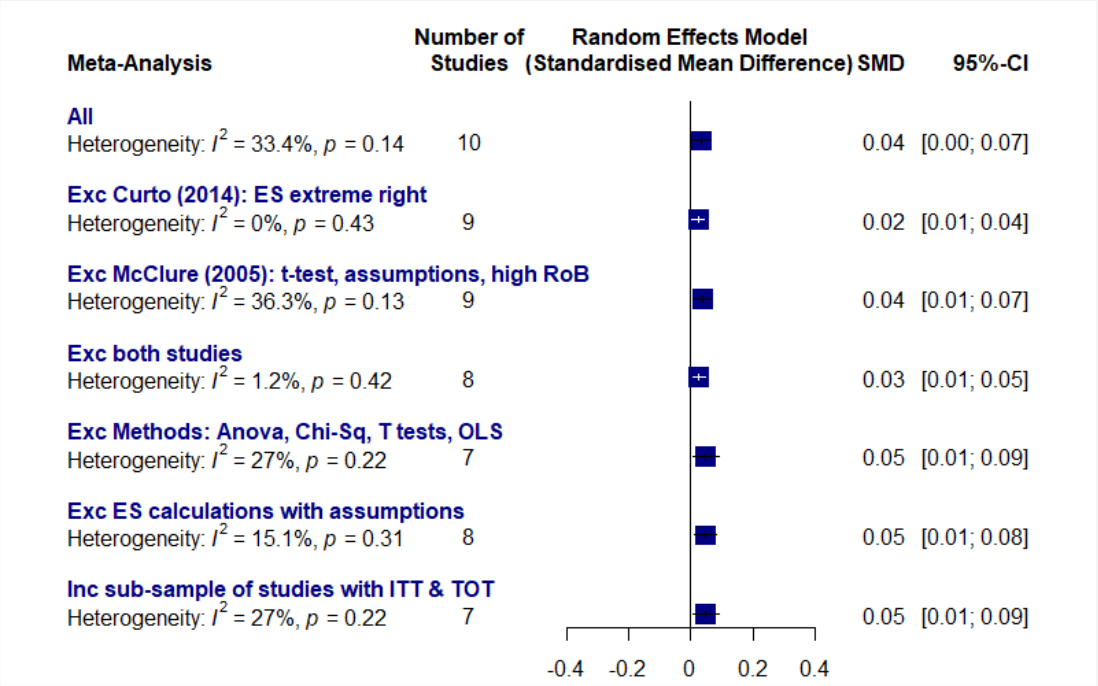


Figure 2.10 Sensitivity analysis for math, ITT



Note: see section *2.2.7* for more details on the rationale for these analyses.

For the synthesis of TOT estimates on math scores, we identify subgroup differences in the policy level of the review's intervention (test for subgroup differences p-value=0.02), in the assumptions made when calculating the effect sizes (p-value=0.02), and regarding the studies locations (p-value=0.04). *Figure 2.11* shows that studies focused on particular schools that use randomised admissions in contexts where other admission mechanisms are in place have an effect size of 0.04 (95% CI [-0.05, 0.13], I2=0%), which is smaller than the pooled effect of studies where the whole school system is based on admission lotteries (0.11, 95% CI [0.04, 0.18], I2=87%). However, the meta-regression analysis indicates that the study intervention cannot serve as a predictor of the effect size (p-value=0.38), which seems reasonable given the low weight the school policy group has in the subgroup analysis.

A similar pattern can be identified in the subgroup analyses by effect size assumptions and study location (*Figure 2.12* and *Figure 2.13*), in which the pooled effect is largely influenced by the group of studies reporting all the necessary information to calculate effect sizes and the group of studies based on known locations. The non-statistically significant results of the meta-regressions for these variables sustain this interpretation (p-value=0.13 and 0.18, respectively). More importantly, *Figure 2.14* presents the sensitivity analyses for the effect size in math, TOT, which highlights the consistency of its main results against a range of different specifications.

Figure 2.11 Forest plot for math, TOT by subgroups of intervention (policy level)

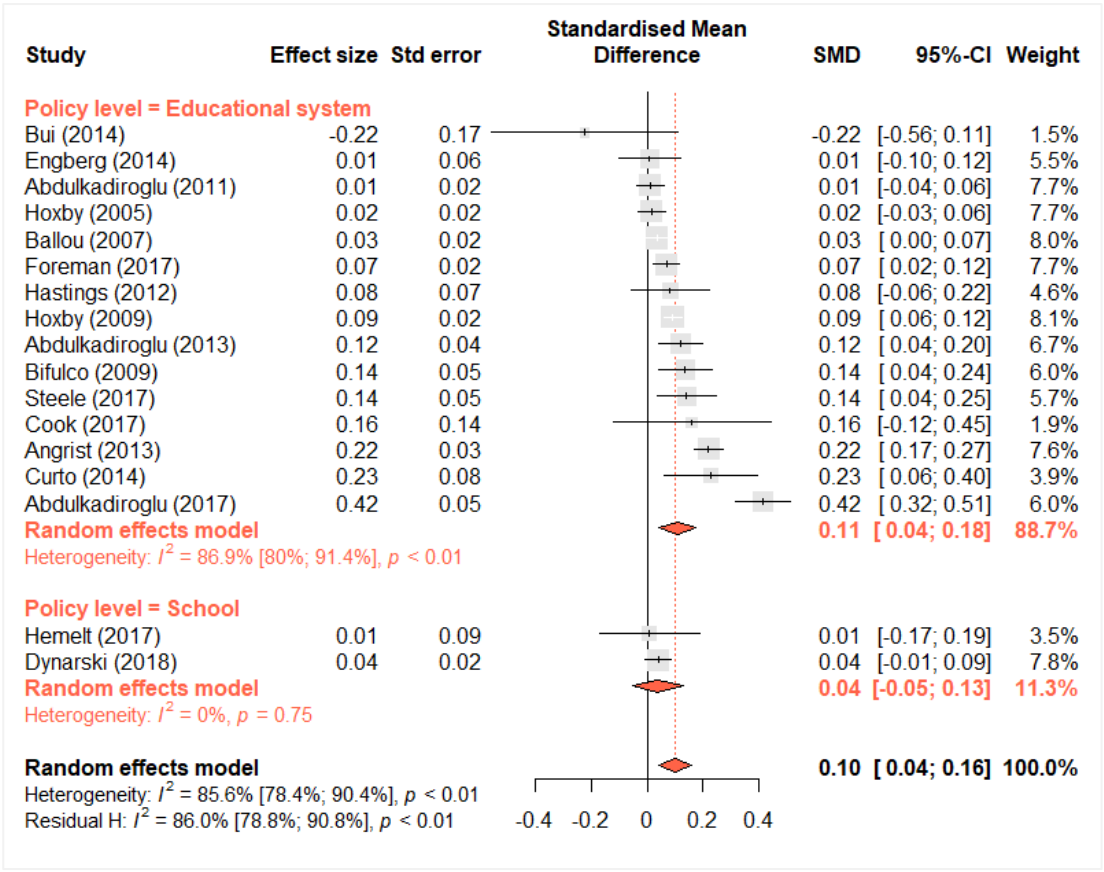


Figure 2.12 Forest plot for math, TOT by subgroups of effect size assumptions

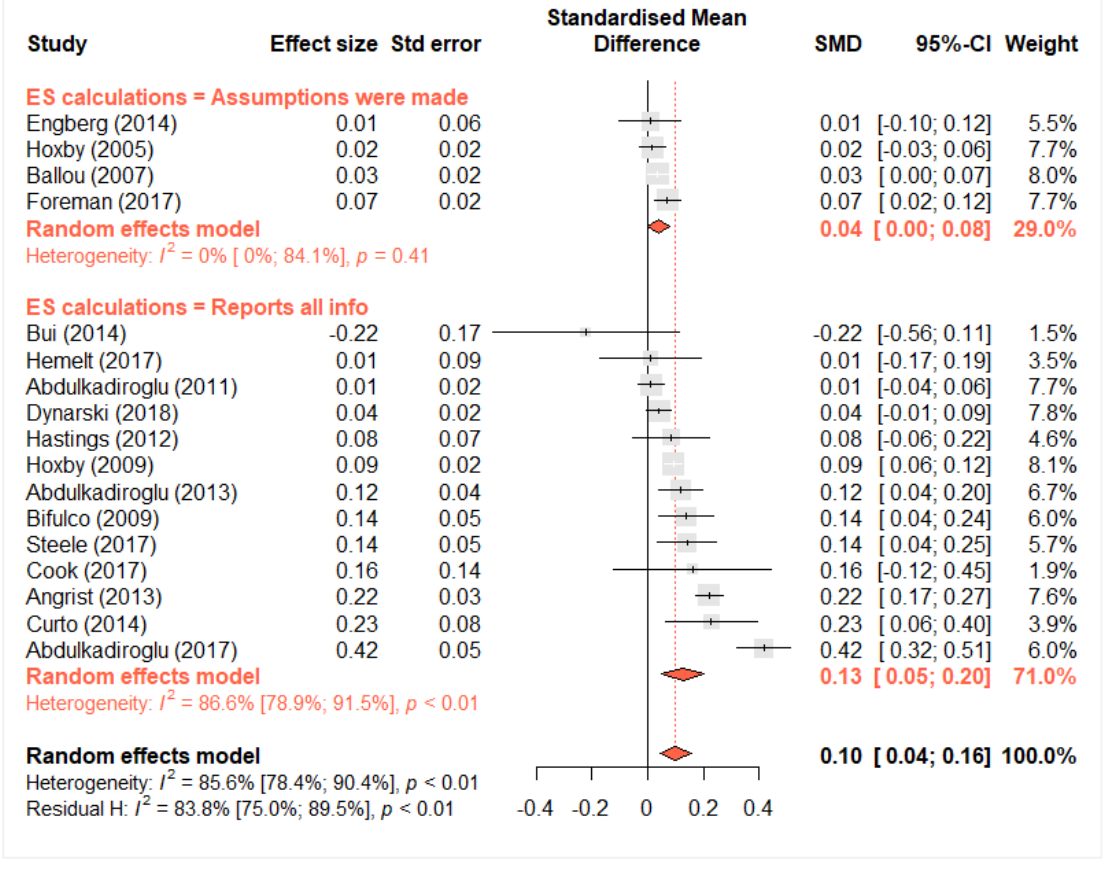


Figure 2.13 Forest plot for math, TOT by subgroups of study location

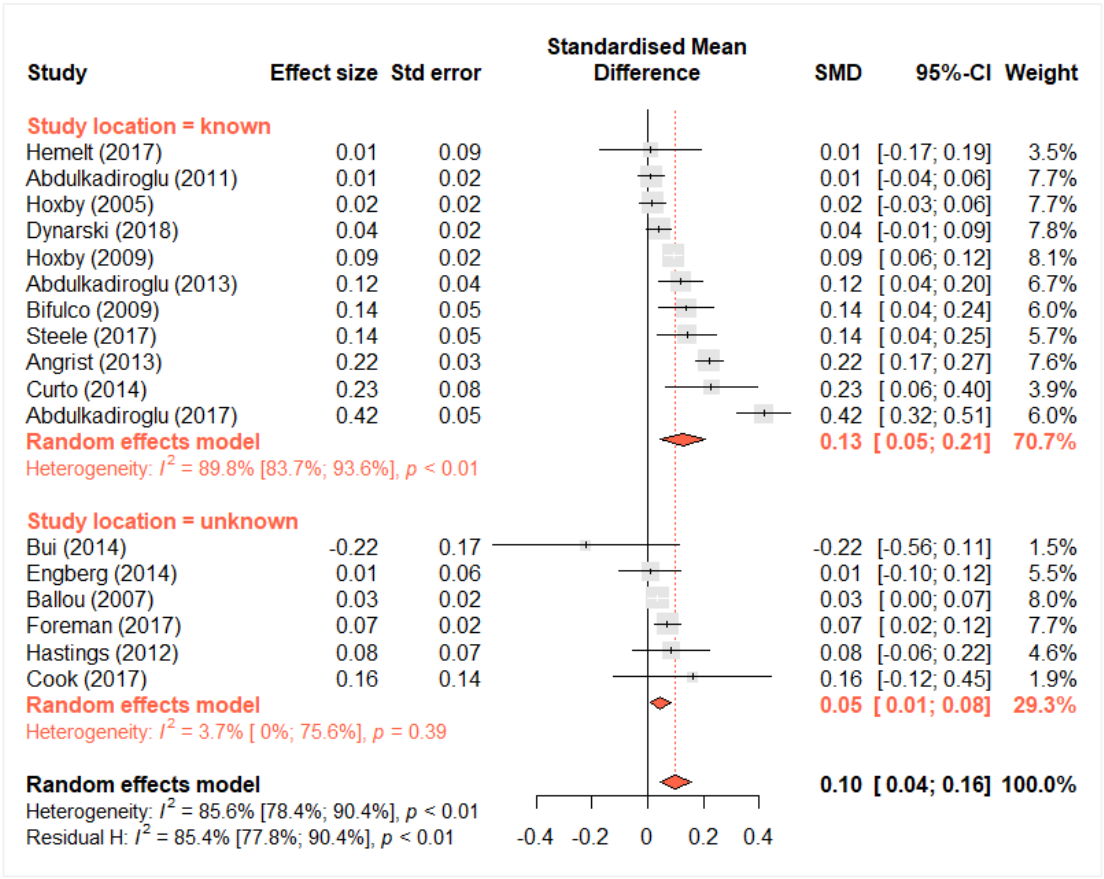
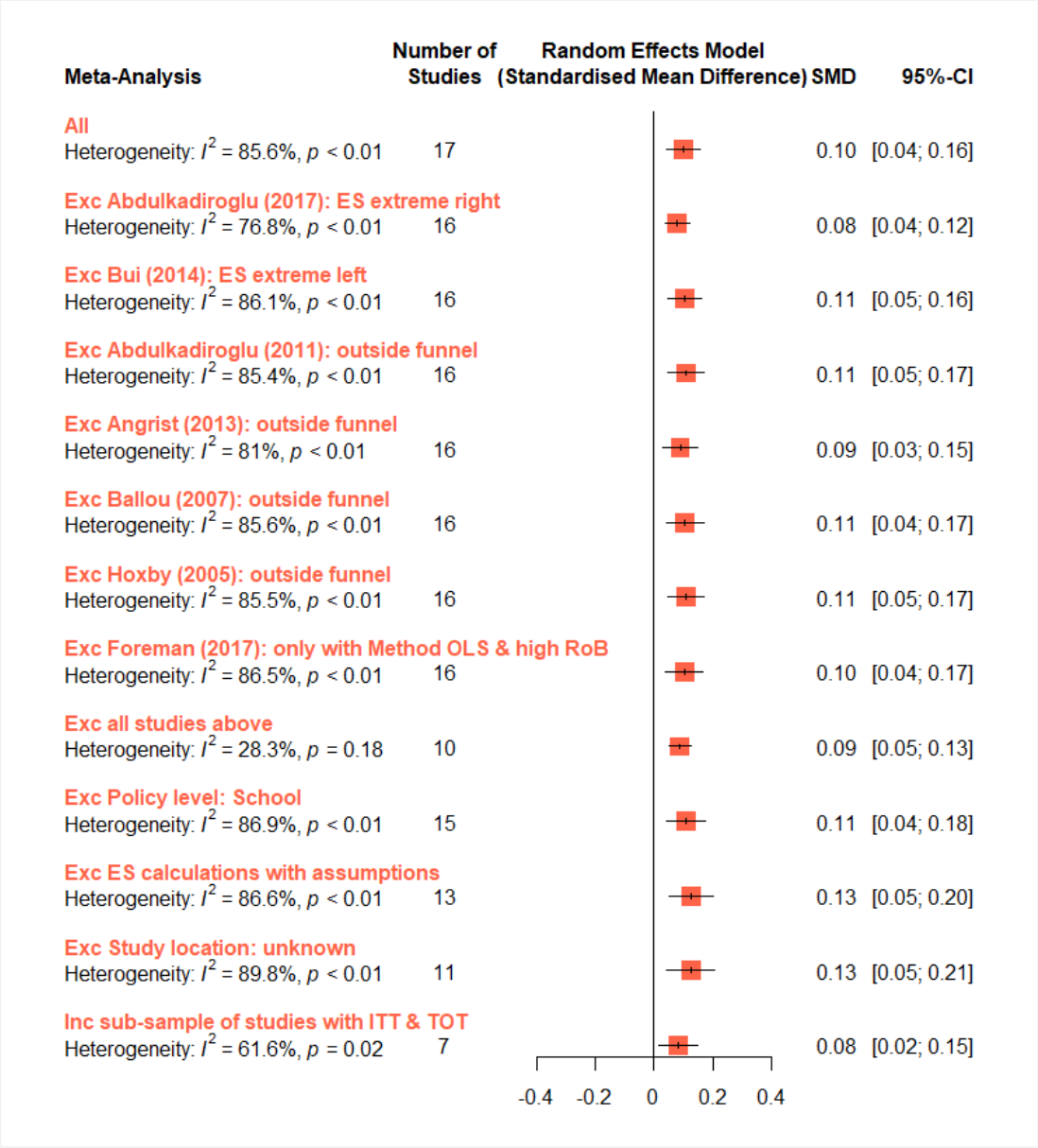


Figure 2.14 Sensitivity analysis for math, TOT



Note: see section *2.2.7* for more details on the rationale for these analyses.

Reading scores

The synthesis of ITT estimates on reading scores is the only analysis where we are able to detect a statistically significant moderator of the effect size, which is given by the school sample size of the studies. Firstly, the subgroup analysis, shown in *Figure 2.15*, excluded the only study with an unreported sample size. From the rest, we can see that studies with smaller school sample sizes have significantly larger effect sizes (0.16, 95% CI [0.01, 0.32], I^2=28%), which is also observed in the meta-regression result (p.-value=0.05). Although studies based on a higher number of schools seem to identify more conservative results on reading, *Figure 2.16* shows that excluding this set of studies from the main analysis does not yield a substantively different effect size.

Figure 2.15 Forest plot for reading, ITT by subgroups of school sample size

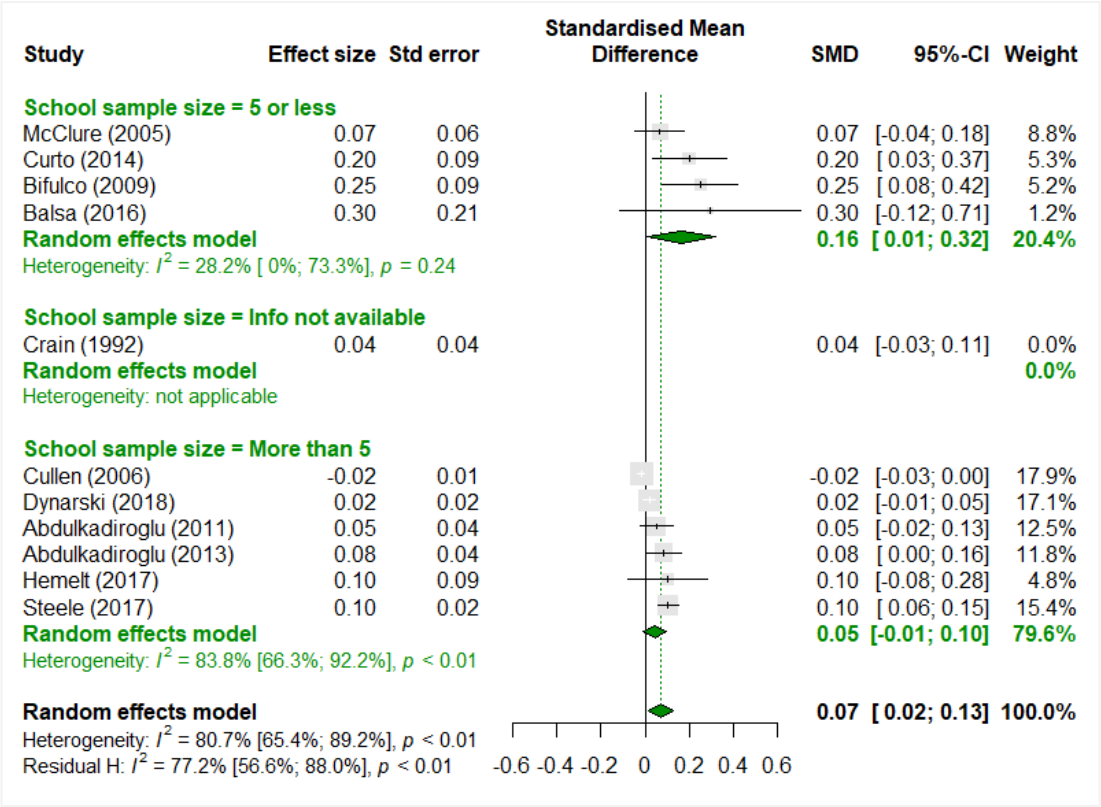
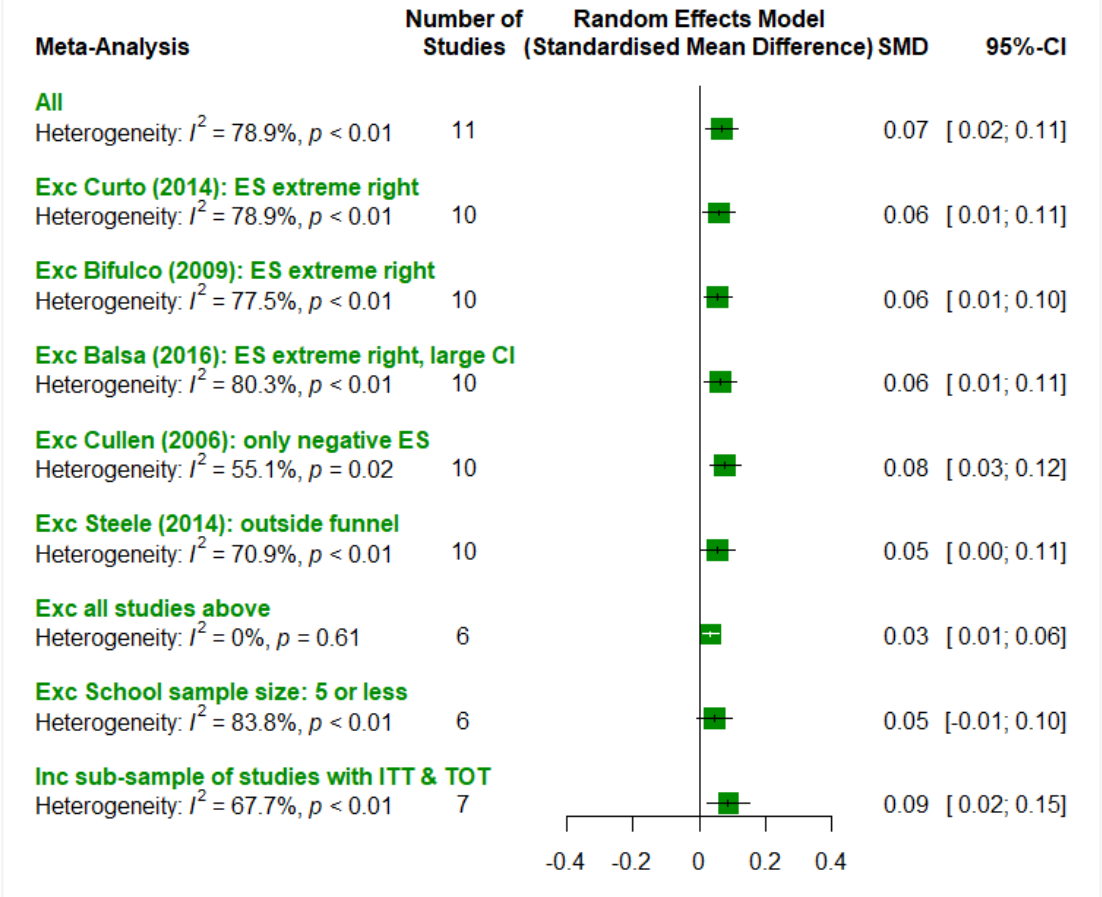


Figure 2.16 Sensitivity analysis for reading, ITT



Note: see section 2.2.7 for more details on the rationale for these analyses.

Figure 2.17 Forest plot for reading, TOT by subgroups of intervention (policy level)

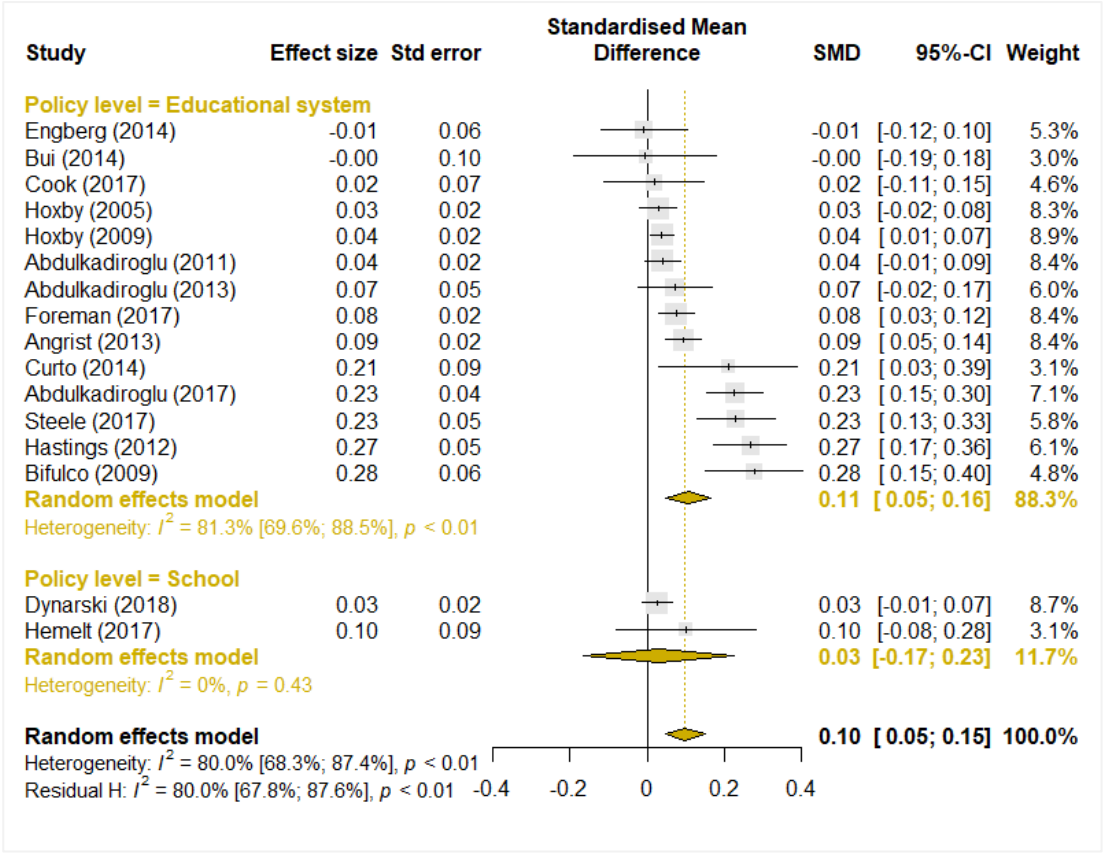
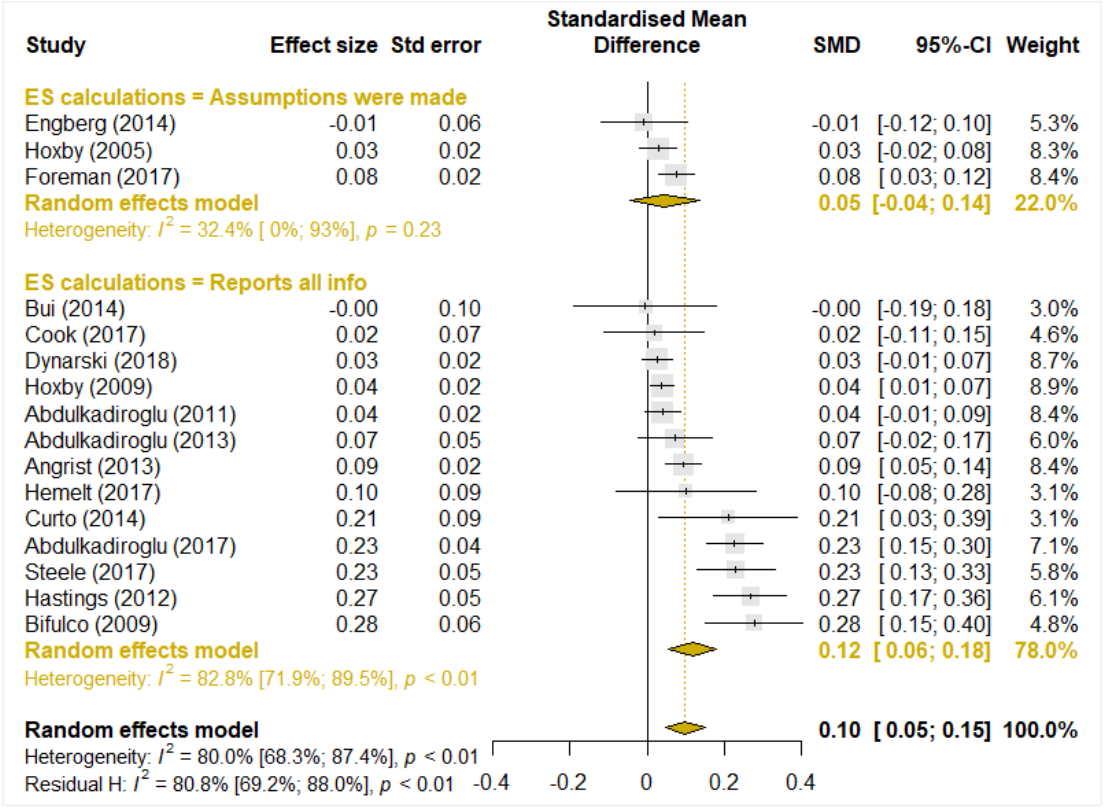


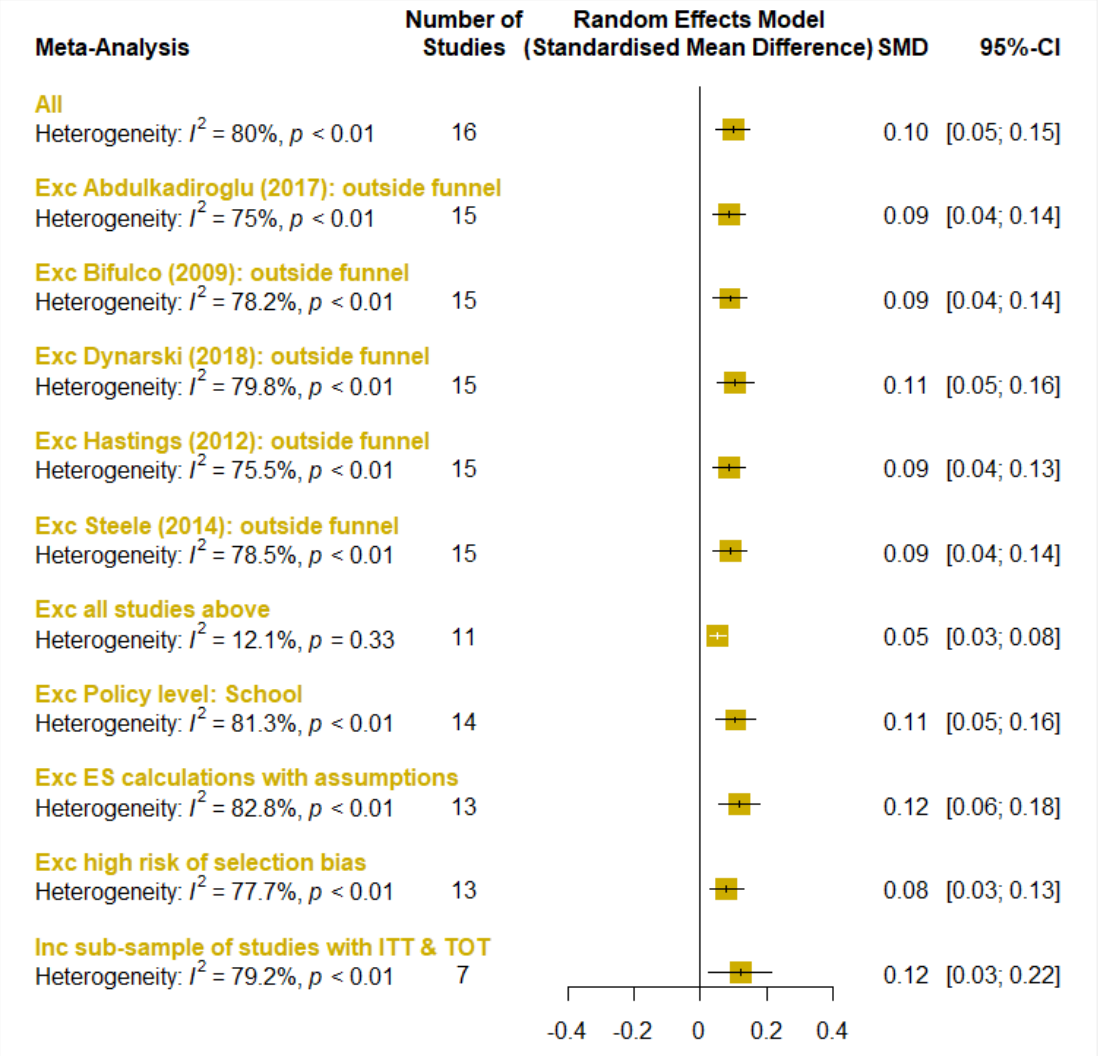
Figure 2.18 Forest plot for reading, TOT by subgroups of effect size assumptions



For the synthesis of TOT estimates on reading scores, we see that there are subgroup differences regarding the intervention evaluated in the review (test for subgroup differences p-value=0.01), and the information available to calculate the effect sizes (p-value=0.04). Both analyses follow a similar pattern in that the group that differs most with the overall effect size is also the group with the lowest weighting, suggesting that the group itself does not largely contribute into this overall pooled effect size. *Figure 2.17* shows that studies focused on particular schools that use randomised admissions have an effect size of 0.03 (95% CI [-0.17, 0.23], I^2=0%), while *Figure 2.18* shows that studies for which our team had to make certain assumptions to calculate the effect sizes have a pooled effect of 0.05 (95% CI [-0.04, 0.14], I^2=32%). In both cases, the meta-regression analysis indicates that these characteristics cannot predict the effect size (p-value=0.44 and 0.18, respectively).

Akin to the previous analyses, *Figure 2.19* illustrates that the main effect size in reading, TOT is quite robust against different specifications.

Figure 2.19 Sensitivity analysis for reading, TOT



Note: see section *2.2.7* for more details on the rationale for these analyses.

Finally, as an additional sensitivity analysis, we assessed if there are systematic differences in the subsample of seven primary studies that estimated both evaluation strategies. As shown in the last section of each sensitivity plot, we find that these pooled effects follow the same overall trend and are consistent with the main effect sizes. This would suggest that the differences we have identified in the main analyses are unrelated to characteristics of this subsample of primary references.

Overall, we can see that there is little evidence of systematic differences in the main effect sizes as characteristics of our primary references could not generally account for their statistical heterogeneity. The subgroup and sensitivity analyses performed suggest that the effect sizes have a high level of heterogeneity but are also highly consistent.

2.3.6. Meta-biases across included studies

To explore the potential presence of publication bias in the review evidence, we first conducted subgroup analyses comparing published references (journal articles) and grey literature (working papers, institutional reports, and dissertations). As shown in detail in section *8.14* of the Appendix, no systematic differences were found for any of the subject/analysis strategy outcome combinations. Secondly, we estimated the correlation between the effect sizes of included studies that were part of the quantitative synthesis and their school sample size. For both analysis strategies on reading, we identified weak, negative, and statistically insignificant correlations (r=-0.38 and p-value=0.27 for ITT; r=-0.22 and p-value=0.48 for TOT). The measures for ITT on math were extremely weak and negatively correlated (r=-0.09 and p-value=0.81), while for TOT on math, the correlation was still weak but positive (r=0.22 and p-value=0.46). These two correlations for math estimates were not statistically significant. The results indicate that, while these measures are mainly negatively correlated, small effect sizes would not be statistically associated with large school sample sizes in our studies. Hence, there would be no evidence to suggest that studies with small effect size and small sample size would not be available due to publication bias. When considering both analyses, and while we cannot definitely reject it, there seems to be no basis to support the presence of potential publication bias in the review's set of literature.

Additionally, we analysed if the critical appraisal of included references in the quantitative synthesis could account for some of its results. First, we evaluated if the risk of bias assessment could individually moderate the effect sizes by conducting meta-regressions with each of these categories. Section *8.14* of the Appendix provides further details on these results, which indicate that, overall, there are no systematic differences between studies with high or low risk of biases. The only exception is seen in the risk of selection bias for the TOT estimate on reading, suggesting that studies with low risk of bias would show lower effect sizes (p-value=0.04).

However, it shows a high between-study variance that is lightly explained by the model (I^2=75% and R^2= 28%). Moreover, the sensitivity analyses for this outcome (*Figure 2.19*) illustrates that, even when excluding the studies with a high risk of selection bias (n=3), the overall effect size is still consistent. Secondly, we considered the overall risk of bias assessment for each included study, both in their original categories (high, moderate and low concern of bias) and aggregated into two categories (moderate-to-high, and low concern of bias) given the small number of studies with high/moderate risk of bias. Subgroup analyses indicate there would be no systematic differences in the effect sizes according to the studies' quality appraisal. Overall, these analyses suggest that the differences we observe between the studies included in the in-depth analysis stem from sources not related to their assessed quality.

2.4. Discussion

2.4.1. Summary of evidence

Main results

This review had two main objectives. First, it aimed to map and systematise the evidence on the impact of school settings using randomised admissions. Second, it intended to synthesise evaluations of the effect of these educational contexts using lottery admissions on student academic performance and school socioeconomic composition. To address these objectives, we conducted a systematic search of literature, from which we presented a descriptive map of such evidence, a narrative synthesis of its outcomes, and a meta-analysis of its results.

While all outcomes evaluated in the primary studies were accounted for and discussed in the narrative synthesis, we were only able to perform a quantitative synthesis of effect sizes for the student academic performance. It was not possible to do the same for the other primary outcome (school socioeconomic composition) and all other secondary outcomes due mainly to the small number of studies measuring these outcomes.

Our main results show positive but small effects on both math and reading outcomes, and under both analysis strategies, intention-to-treat and treatment-on-the-treated. The overall effect size on math scores using ITT estimates is 0.04 (95% CI [0.00, 0.07], I^2=33%) while using TOT estimates is 0.10 (95% CI [0.04, 0.16], I^2=86%). Likewise, the overall effect size on reading scores using ITT estimates is 0.07 (95% CI [0.02, 0.11], I^2=79%) and using TOT estimates is 0.10 (95% CI [0.05, 0.15], I^2=80%).

These results of the quantitative synthesis also show high levels of heterogeneity, indicating that the primary studies we are comparing have differences that cannot be explained in the context of this review. This was expected in that the intervention defined for this review

was the schools' randomised admission process and, since this is not necessarily the primary intervention in each study, the overall effect will be confounded by other factors that extend beyond the scope of the review; namely, the particular educational project of each study. This review tried to get the best estimate possible of randomised school admissions, but this does not strictly mean that it will achieve a "pure" effect. While we acknowledge the differences between the educational projects in our primary studies, we tried to take into account these differences by conducting subgroup analyses. We initially see some differences by subgroups based on the methods, sample size, intervention, location, and information available to calculate effect sizes for each primary reference. However, the main effects are consistently positive against different robustness and sensitivity analyses.

To think about these results in the wider context of education policy, we highlight two main points. First, that at the very least, by aggregating these studies which we know are different from each other, we can see that randomised admission systems do not seem to harm academic results. This would have the potential to challenge the idea that by having greater heterogeneity of students, schools would lower their quality, when in fact, the evidence suggests that this does not necessarily have to be the case. Second, to emphasise an idea made in previous research (Stasz & Von Stolk, 2007), there seems to be some disconnection between the purpose of using randomised admissions and how these school contexts have been evaluated in the literature. The intention of using lottery admission is to provide equal opportunities in the access to education by making the school admission a fair and non-discriminatory process. On one hand we may think how compatible is measuring academic performance with the purpose of such admission policy, and subsequently, which would be the most appropriate outcomes to evaluate lottery admissions. On the other hand, and given the small overall effect on academic performance, we may also question whether providing a fair entry process is enough to provide equal educational opportunities (Cullen & Jacob, 2009).

We conclude by assessing the review's evidence in terms of its consistency and quality. First, the review's evidence is conceptually consistent as all primary studies have in common that they focus on school settings using lottery admission systems. However, this set of literature also shows a high level of heterogeneity, both in terms of the educational projects and the evaluation approaches of such school settings. Secondly, our results suggest there is a robust, small and positive effect of these school contexts using lottery admissions on measures of academic performance. This statistical consistency of the evidence is mainly supported through the sensitivity analyses conducted for each subject/analysis strategy. Finally, regarding the quality of the evidence, we can see that the majority of the primary studies have low risks of bias. More importantly, our analyses indicate that detecting a higher risk did not lead to a statistical bias in the results.

Quality and relevance of the review

In order to evaluate the quality and relevance of this review, we need to take into consideration three main aspects. First, the review was deemed to be executed following the principles of rigour and transparency to facilitate its potential replicability. Second, the review aimed to address several aspects of the intervention of interest. While its design allowed us to discuss the main features of the research questions, there was a certain limitation of the empirical studies to address the question about the purpose of using randomised admissions. Inadvertently, some primary studies took this discussion for granted and did not explicitly address why they use school lotteries. Although this was only one question of the review, the inclusion criteria were not completely aligned with the objectives of the review. Third, the review was able to identify a suitable range of studies focused on the intervention of interest and cover a variety of educational contexts that helped address the review questions.

According to these assessments we finally present an overall judgement of the evidence. The review adhered a robust process, which allowed to address its research questions in a generally satisfactory way. The explicit set of rules by which the literature search and analyses were conducted, along with the critical appraisal the evidence, both individually and as a set, provide confidence in the review's findings. However, one of these questions, the one exploring the purpose of using randomised school admissions, could not be entirely answered. Further research could complement this evidence with a revision of substantive literature to depict the political, legislative, and academic discussion behind the implementation and public appropriation/reaction of such school admission policies.

2.4.2. Limitations

Although the search strategy initially captured a wider range of school contexts at the international level, the studies finally included in this systematic review cover a diverse range of educational contexts, which is also confirmed with the heterogeneity indicators in the meta-analysis. However, the review does not include some contexts where we know that school randomised admissions are used (e.g. New Zealand). Hence, the original aim of having an international perspective is in some way diluted. We took measures to try to ensure that our search strategy was broad, but there is always the possibility that for some other reason we could not capture other studies. It may be that our language criteria (English and Spanish) restricted this identification, or that in certain contexts there have simply been no quantitative evaluations focused on these school admission systems, or that having random admissions is not a relevant or pressing issue of education policy because these are taken for granted or perhaps do not present challenges to these educational communities.

In addition, since we divided the analyses by evaluation strategy (ITT and TOT) and subject (math and reading), the sample of studies included in the meta-analysis could be underpowered (Borenstein et al., 2009). On one hand, the number of studies used in each quantitative synthesis did not always allow to conduct subgroup analyses. On the other hand, regarding the context of the review process, we had to exclude a number of included studies from the set of master records in order to avoid double counting school samples. Then, it is possible that there are systematic differences in the effect sizes that this review could potentially account for, but the small number of studies did not allow for these differences to be detected. We made the explicit judgement that double-counting schools in overlapping studies would pose more of a risk to confidence in the pooled estimate than potentially excluding some schools. Under the circumstances of this review, we took reasonable and most importantly consistent measures to carry out the review process.

2.4.3. Conclusions

This review aimed to map and systematise the evidence on the impact of school settings using randomised admissions and synthesise evaluations of the effect of these educational contexts. For this purpose, we conducted a systematic literature search under rigour standards and present the results for a descriptive map of the evidence, a narrative synthesis of its outcomes, and a meta-analysis of its results.

The review's evidence does not present quality concerns as it is largely exempt of risks of bias and shows a degree of conceptual consistency among its primary studies. The results of the quantitative synthesis show a positive but small effect on math and reading performance and high levels of heterogeneity. While we tried to account for these differences across studies, our results seem statistically robust.

2.5. Contributions

R1 is the guarantor. R1 drafted the manuscript and mainly developed all sections of the review. R2 contributed significantly to the conception of the review, its eligibility criteria and analysis strategy. R3 contributed to the development of the review proposal, and the screening, coding, and data extraction process. All authors read, provided feedback, and approved the final manuscript.

2.6. Funding or sponsor support

This project was part of R1's PhD dissertation, which was jointly funded with scholarships from ESRC through the UBEL Doctoral Training Partnership, and CONICYT Becas Chile 72150434. The use of the online management tool EPPI-Reviewer 4 was supported by the EPPI-Centre of the UCL Institute of Education, University College London, UK.

2.7. Conflict of interest

The authors declare no potential conflicts of interest.

3. School choice or school handpicking? The effect of selective school admissions using Chilean panel data

3.1. Introduction

Since the 1980s, the Chilean school system had been based on a universal school choice scheme, which grants a subsidy to all students to attend any publicly funded school that families choose (Mizala & Torche, 2012). One of the characteristics of the Chilean educational system was that schools were not highly restricted to select students, as each school had the autonomy to define their own admission process. Before the School Inclusion Act (2015), private schools (which do not receive public funding) had no restrictions for selecting students at any grade and by any criteria, whereas schools that receive public funding (traditional public and private-subsidised schools) could select students under any criteria from 7th grade (students aged 12-13). Up to such grade, these schools were not allowed to select by ability or socioeconomic background. However, as shown by previous evidence and the present research, this was not entirely followed by schools. Therefore, student selection was a widespread practice across Chilean schools.

This paper aims to explore how selective admission practices have an effect on schools, both in terms of their academic performance and socioeconomic composition, and also evaluate if there are differences in the effect of selective admissions by school type and grade. The analysis focuses specifically on the use of entry tests and parent interviews as archetypes of student selection by individual ability and family background.

In the international context, there seems to be a consensus that student selection practices can be detrimental to school systems. Countries with selective systems do not appear to drive general academic improvements for the whole school system, but instead, show higher levels of academic and social segregation, especially if student selection is done in early stages (Gorard & Smith, 2004; OECD, 2010). Evidence from Chile indicates that the education system is highly segregated both in academic and social terms, compared to other countries with similar characteristics (Bellei, 2013; Valenzuela, Bellei, & De los Rios, 2009). Yet, there is little empirical evidence that directly associates student selection practices to academic and social segregation in the Chilean context. Exploring this relationship is especially relevant in the context of education policy in Chile and the school reform recently passed. The School Inclusion Act (2015) introduces structural changes in the school system to promote inclusion and equal educational

opportunities. Along with abolishing the co-payment that schools could charge to families and forbidding for-profit schools, the reform also regulates the admission process of schools by eliminating student selection and creating a centralised admission system for all publicly-funded schools. Then, it is crucial to explore the mechanisms under which student selection worked before the reform and the implications that such evidence may have in light of this national policy change.

I use a flexible difference-in-differences approach, which exploits differences in the use of admission mechanisms by following a panel of schools and using a linear regression model with school and time fixed effects. The panel was created linking administrative data at the school- and student-level from 4th and 8th grades (10-year-olds and 14-year-olds, respectively) for the academic years between 2004 and 2013. With these data, I can account for the schools' use of admission mechanisms over time and estimate the effect of student selection by comparing changes in outcomes for schools with different types of use of admission mechanisms, with those schools that have never used these admission mechanisms.

Overall, selective schools show small but higher academic outcomes, suggesting that, on average, these schools would not be having a substantial academic benefit from selecting by ability or socioeconomic background. Results for 4th grade indicate that always selective private and private-subsidised schools seem to be driving the academic effects, while private schools would be able to attract families with higher socioeconomic status. Moreover, the few public schools that always select in 8th grade show considerable academic gains and seem to attract similar students from higher socioeconomic status.

This study is not exempt of certain limitations, which are mainly due to data availability and the identification strategy. However, this research also contributes to the literature on education policy in several ways. First, by using panel data, I assess the effect of selective admission practices across time. Second, the identification of selective admissions discriminates between schools that always select, schools that never do, and schools that have changed their admission policies. Third, it evaluates all school types and two school grades, one on primary and one in secondary education. Finally, this paper evaluates the effect of selective admissions on both academic and socioeconomic outcomes.

The paper is structured as follows: Section 2 describes the Chilean school system and examines some relevant literature on student selection and socioeconomic segregation. Section 3 provides details on the data used in the research, on the categorisation of the use of admission mechanisms, and on the construction of the socioeconomic composition index. Section 4 describes the identification strategy of the research; while Section 5 presents the research findings and robustness checks. Finally, Section 6 discusses the results and policy implications of the research.

3.2. Background

3.2.1. The Chilean school system

In the 1980s, under dictatorship, many social services in Chile were restructured towards market systems. This generated significant changes in the education sector, in which a school choice scheme was established nationwide. The ultimate goal of this system is to give all students the opportunity to access education and choose the best school for them to study (Barrera-Osorio & Patrinos, 2009). The school choice system works under the assumption that governments could not efficiently provide a school place for each student; therefore, the expansion of the private-subsidised sector into education would be essential for increasing school coverage. This way, the educational offer would grow in variety and flexibility, and the competition between schools would promote the improvement and efficiency of the whole school system. On the demand side, families would be able to voice their preferences by choosing the school that fits their values and necessities (Friedman & Friedman, 2002).

Consequently, the privately-run sector in education funded by the state developed swiftly, and the Chilean system was consolidated under three major types of schools[1]: public, private-subsidised, and private. Public schools are run by the municipality's education unit and are entirely funded through state subsidies. Private-subsidised schools are owned and run by private individuals, bodies or foundations, which manage from a single to a network of schools. They may have mixed funding; that is, along with receiving state funding, these schools are also allowed to charge an additional fee to families. Finally, private schools are run by private individuals, bodies or foundations, and have curriculum and management independence from the Ministry of Education. These schools are funded solely by families' private means (i.e., these schools do not receive public funding). Currently, private-subsidised schools account for just over half of the schools, while the private sector represents around 5% of the total number of schools in the country.

The Chilean school choice system is based on a student subsidy (a voucher) that the Ministry of Education transfers directly to schools once students are enrolled. According to Gallego and Sapelli (2007), while many school choice systems use vouchers, its implementation varies across contexts. Based on a classification model (Blaug, 1980), the authors provide an overall picture of the Chilean voucher under four main characteristics. First, it is *unlimited* as the subsidy can be used in any school funded by the state, whether it is for-profit or non-profit, religious or secular, etc. Only private schools are not able to receive this voucher. Notably, schools that receive state vouchers and are also for-profit have a long history in Chile. In 2009,

[1] There is a small number of schools (around 70; less than 1%) that correspond to a different category regarding their administrative body. These schools were not considered in the research.

at least a third of students in the country attended for-profit schools (Elacqua, Martinez, & Santos, 2011). Second, it is *supplemental* as along with the per-student state voucher, all private-subsidised schools and secondary public schools can charge an additional fee to families. By 2010, nearly 80% of primary and secondary private-subsidised schools, and 25% of secondary public schools asked for this co-payment to families (Elacqua, Montt, & Santos, 2013). Third, it is *not-uniform* since not all students receive the same amount of subsidy. For many years, the school system was based on a flat voucher, disregarding the students' background and needs. However, the Preferential School Subsidy Act (2008) introduced an increased voucher for "priority students"[2]. Finally, until the School Inclusion Act (2015) that reformed the school admissions system, the Chilean voucher was *unrestricted*, as the General Education Act (2009) allowed for schools to select their students from 7th grade (13-year-olds). However, as discussed in this paper, schools also applied different student selection mechanisms before such age, which undermined the assumptions of a school choice system and, instead, put the focus on the selective practices that schools use in their admission processes.

3.2.2. Selective admissions in Chile

In most countries, access to publicly funded schools is based on residential criteria and catchment areas (Manzi, 2007; Wylie, 1998). Although more commonly used in secondary education, student selection is unusual in earlier school stages. In OECD countries, schools start selecting students by academic ability, on average, at age 14. Moreover, student selection in earlier stages and high academic selectivity are more common in school systems with a higher number of educational programmes available for students (OECD, 2013).

In contrast, student selection was a widespread school practice in Chile, both in primary and secondary education, especially within private-subsidised and private schools, and in schools with higher socioeconomic composition. Moreover, the admission mechanisms that schools most often used not only focused on student ability, but also on the socioeconomic background of their families (Carrasco, Gutierrez, & Flores, 2017; Dante Contreras, Sepúlveda, & Bustos, 2010; Godoy, Salazar, & Treviño, 2014; Parry, 1996).

The literature available on Chile highlights three features of the school context in which student selection is framed to help understand the extent of these practices. As described below, the General Education Act (2009) was somewhat ambivalent regarding the delimitation

[2] According to the Preferential School Subsidy Act, priority students are "those for whom the socioeconomic situation of their homes hinders their ability to cope with the educational process". This classification is given to the 40% most vulnerable students, determined annually by the Ministry of Education based on social data from other public entities. As an example of its funding, in 2012, the regular monthly subsidy was around £53, while the preferential subsidy could amount up to £90; that is, approximately up to 70% more (Ministry of Education, 2012a).

of the use of these mechanisms and the range of these practices. Second, the education system itself also seemed to drive this type of selection. Third, the use of student selection mechanisms by schools appeared to be socially accepted and legitimised (Daniel Contreras, 2010; Mena & Corbalán, 2010; Valenzuela, Bellei, & De los Rios, 2010).

The General Education Act (2009) prevented the selection of students up to 6th grade (12-year-olds), both based on academic performance or socioeconomic background. This meant that schools could select students under any criteria from 7th grade (13-year-olds), and there was also a consent –by omission– to select students up to 6th grade according to other standards not explicitly mentioned (for example, religious affiliation). Also, the Act allowed each school to define its own admission process, regardless of the level of subscription, requiring only that such procedures are clear and known to the whole community. This hindered the control of selective admission mechanisms by authorities. Moreover, the General Education Act only applied to schools receiving public funding, which meant that private schools had no restrictions regarding the admission mechanisms they could use. Hence, the education law was not sufficiently rigorous or clear to prevent arbitrary discrimination against students.

Schools also had structural incentives to select their students. A potential contradiction between the Chilean school choice system and student selection was early exposed by Parry (1996). Under the theory of co-production applied to education, in which students do not only receive an education but are also part of the production process, schools would have strong incentives to choose only the best partners to make this production more efficient. More generally, MacLeod and Urquiola (2009) developed a similar rationale under a market reputation model. This indicates that schools would base their survival on their reputation, which in turn, depends on the schools' value-added and the quality of its student body. Given that it is easier to access and process information about a school's composition than about its value-added, schools would work towards consolidating its student body rather than on strengthening its educational value to achieve a good reputation. The authors conclude that the education market would be efficient only when schools are not able to select their student body.

An example of these structural incentives is related to the financing of schools in Chile. The voucher subsidy received by schools is subject to accountability systems and student academic testing results, which puts pressure on schools, from principals to teachers, and forces them to find effective ways to meet the standards (Godoy et al., 2014). From this perspective, selecting the best students could also be understood as a practical strategy to obtain state funding.

Finally, although publicly funded schools are not permitted to select students before 7th grade based on ability or socioeconomic background, they seemed to have no issues in declaring their use of selection mechanisms in their admission processes. Previous research indicates that

Chilean schools choose to select students even when they are not oversubscribed, and school principals are open about the use of these practices for discriminatory purposes (Carrasco et al., 2017; Parry, 1996). It would appear that schools knew this information was valued by families and was well considered when applying to schools. If a school declares extensive admission requisites, families seemed to interpret this as a signal of good quality rather than categorise the school as "too selective" (Godoy et al., 2014). Ultimately, this suggests that there were hardly consequences for schools that apply admission mechanisms.

3.2.3. Student selection and educational segregation

After being in place for more than three decades, the Chilean school choice system has not yielded the expected benefits in education, either in academic or socioeconomic terms. Chile has similar levels of expenditure on education than other OECD countries (Ministry of Education, 2013), yet it has not been able to show high academic performance in international standardised tests such as PISA or TIMSS (Bos, Ganimian, & Vegas, 2014; Ministry of Education, 2012b). Even the national student assessment results have stagnated in the last years (Daniel Contreras, 2010; Ministry of Education, 2014). Moreover, evidence indicates that the education system is highly segregated, both academically and socioeconomically. This segregation is observed in primary and secondary education, and is particularly high in the private-subsidised sector (Bellei, 2013; Dupriez, 2010; OECD, 2004; Valenzuela et al., 2009).

International evidence shows that student academic performance is more correlated with the aggregated socioeconomic context of the school than with the student's own socioeconomic background (OECD, 2010). This also seems to hold for the Chilean context (Daniel Contreras, 2010; Mizala & Torche, 2012). Additionally, compared to other OECD countries, a higher percentage of the variation in academic performance in Chile is accounted for by the socioeconomic status of the students (OECD, 2014), and jointly accounted for by the demographic background and the student selection practices of the schools (OECD, 2010). This relationship between academic performance and the socioeconomic background of students and schools suggests that if schools manage to shape and improve their socioeconomic composition, they could also affect their academic performance.

As described earlier, the selection of students in Chile is more prevalent in the private and private-subsidised sectors; then it could be hypothesised that selective school admissions are indeed associated with the levels of segregation in the education system. The more the schools can shape their student body, the more segregated the students will be, which in turn is also associated with higher socioeconomic inequalities in the education system (OECD, 2010). International evidence suggests that countries with more selective systems show higher levels of academic and social segregation, compared to comprehensive or non-selective systems

(Boeskens, 2016; Gorard & Smith, 2004), and that student selection in early stages (before ages 14-15) increases academic inequality in the education system (Hanushek & Wößmann, 2006). Parry (1996) indicates that the school choice system may improve education quality; however, the author detects a conflict between this scheme and equality in education, anticipating that due to student selection practices, quality and equity could not be developed to the same extent in the Chilean school system. Moreover, studies from Chile and Argentina suggest that once controlling for the school's selection practices, the difference in academic performance between public and private-subsidised schools disappears (Dante Contreras et al., 2010; Quiroz, Dari, & Cervini, 2018). Following the reputation model mentioned before (MacLeod & Urquiola, 2009), this would indicate that an academic difference by school type would be due more to the capacity and motivation of the school to select its student body than to its educational value.

Socioeconomic segregation in education is a contentious topic in Chile, yet, it has rarely been empirically connected to student selection practices. Furthermore, beyond theoretical discussions or descriptive data, there is little empirical evidence on student selection and its effects in the Chilean school context. Most of the quantitative research available is descriptive, where the external validity of the results is one of their main shortcomings. The present paper intends to develop this evidence by examining the effect of student selection practices on academic and socioeconomic composition outcomes.

For this, I explore two hypotheses. On one hand, I assess if there is an effect of student selection on academic performance and its within-school variation. The scarce evidence available is mixed, finding no effect to moderate positive effects on academic outcomes; however, these are based on descriptive analyses (Parry, 1996), cross-sectional data (Dante Contreras et al., 2010), or only focused on a specific type of schools (Allende & Valenzuela, 2016; Manríquez, 2016). Hence, by using panel data of all schools and by accounting for their selection practices over time and other unobserved characteristics, I aim to use a more comprehensive data framework to estimate this effect. Then, the first hypothesis is that selective practices will have a positive effect on student academic performance.

On the other hand, I evaluate if student selection practices affect the school socioeconomic composition and its dispersion. This has not been measured before in the Chilean context using panel data, but some preliminary descriptive evidence suggests that student selection practices are negatively associated with school social heterogeneity (Carrasco et al., 2017). The second hypothesis, then, is that selective practices will have a positive effect on the socioeconomic composition of the school, and a negative effect on its variation. This means that selective schools would show socioeconomically higher and more homogeneous social compositions. If both hypotheses hold, it would suggest that the Chilean school choice system

"led schools to compete on selectivity rather than productivity in the generation of skill" (MacLeod & Urquiola, 2009, p. 32).

Finally, and in line with previous research (Dante Contreras et al., 2010), the research assumes that the direction of the relationship is such that selective schools show higher academic performance and socioeconomic composition, and not the other way around. It could be argued that schools with a good reputation receive many applicants and, therefore, would be able to select students. However, evidence indicates that Chilean parents base their school choice on different criteria: from quality measured in test scores to location, socioeconomic composition, or the additional co-payment charged to families (Gallego & Sapelli, 2007). From these, the residential criteria would be the predominant (Schneider, Elacqua, & Buckley, 2006). Hence, it is more likely that, by selecting students, schools can develop a good reputation and increase their academic performance and socioeconomic composition. This is also aligned with the perception of families towards selective schools; namely, that the use of admission mechanisms would be a good signal (Godoy et al., 2014).

3.3. Data

The study uses a panel of school- and student-level data covering the academic years between 2004 and 2013[3]. The panel follows public, private-subsidised, and private schools, and concentrates on two grades: 4th (10-year-olds) and 8th grade (14-year-olds).

The data comes from two sources, the Ministry of Education and the National Education Quality Assessment System (SIMCE, for its acronym in Spanish[4]). Administrative data from the MoE include total school enrolment and several student-level academic indicators such as special educational needs status, annual attendance rate, academic status, and grade retention. Data from SIMCE consist of school-level information (school type, grade, and mechanisms used in the admission processes) and student-level information (household characteristics collected from parent's questionnaires, and individual test scores collected from student assessments). Both the MoE and SIMCE provide the student's gender, which was compared across time as a consistency indicator. Section *9.1* of the Appendix presents the description of the main variables used in the analyses.

The panel includes all schools – and students within those schools – for which there are official records available for 4th and 8th grades between 2004 and 2013. *Table 3.1* shows the total

[3] The panel covers all years for which data is available and comparable. Inconsistencies in the parent's questionnaire across evaluations prevent the extension of the school panel.

[4] SIMCE is based on student assessments and other instruments, such as school profiles and questionnaires for teachers and parents, to contextualise the learning environment of each student. Teachers' questionnaires are not consistent across years, hindering their use for panel data analysis.

number of schools and students in each year included in the analyses. There are several reasons for the panel to be unbalanced: schools may open or close during the years included in the study, as well as students may choose to change schools at any time. In order to maximise the number of complete cases, when linking datasets at the student-level, missing data was imputed with the grade mean of each school[5].

Table 3.1 Summary of the research sample, by year

Year	Number of schools	Number of students	Data for 4th grade	Data for 8th grade
2004	5,177	301,464		Yes
2005	6,588	289,970	Yes	
2006	6,758	289,411	Yes	
2007	7,238	613,345	Yes	Yes
2008	7,096	278,178	Yes	
2009	7,319	532,561	Yes	Yes
2010	7,325	274,416	Yes	
2011	7,488	539,135	Yes	Yes
2012	7,295	268,496	Yes	
2013	7,770	562,487	Yes	Yes
Total	8,237	3,949,463		

3.3.1. Admission mechanisms data

Since 2004, the SIMCE parent's questionnaire includes a section about the schools' admission processes, asking which mechanisms were requested from them or the student when applying to the school[6]:

1. The school requested the student to take an entry test
2. The school requested the student to attend a play session
3. The school requested a preschool evaluation from the previous school/nursery
4. The school requested a transcript from the previous school
5. The school requested the parents to attend an interview
6. The school requested a civil marriage certificate
7. The school requested a baptismal and/or religious marriage certificate
8. The school requested a salary certificate from one of the parents

[5] Student-level variables imputed include test scores, SEN, attendance, academic status, retention, computer, internet and books at home, parents' educational level, and household income. Section *9.2* of the Appendix show that the main results are robust when using complete cases.

[6] From a total of ten mechanisms included in the SIMCE parent's questionnaire between 2004 and 2013, these eight are comparable across the panel. Birth certificates are not included as virtually all schools request them to check that students are applying to the correct grade, and psychological/behavioural reports were included in the questionnaire from 2013.

This section of the questionnaire is framed in retrospect, as it asks about the moment when parents were applying to the school, which could differ for each student. However, the most regular entry levels for students is Pre-Kinder (5-year-olds) and 1st grade (7-year-olds); hence, I assume that parents' answers would be accurate and comparable indicators. To use these responses as a school indicator, the proportion of parents who declared that they had been asked for each of these admission mechanisms was calculated for each year, school and grade. Following previous research (Dante Contreras et al., 2010) and initial analyses on the data[7], a cut-off point of 50% was used to categorise each school: if at least half of the parents per cohort reported that the school requested a mechanism, then the school was considered to use such admission mechanism.

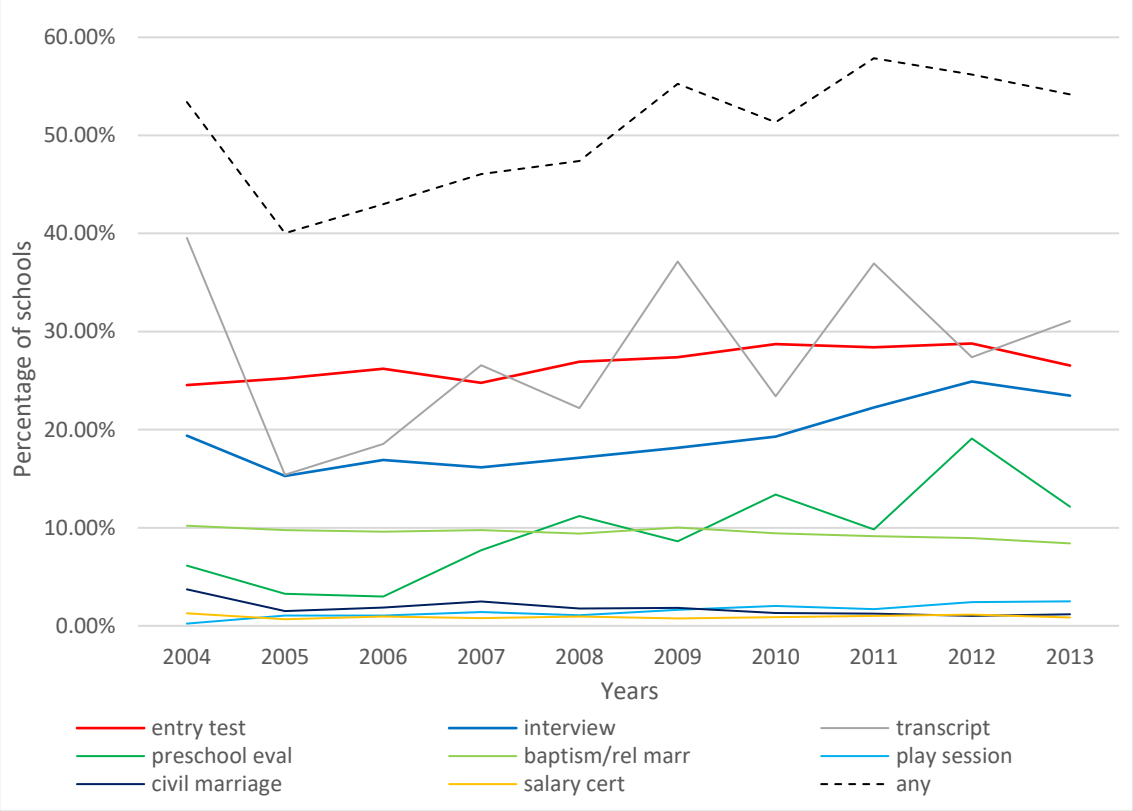
Additional validation analyses were conducted in order to assess the appropriateness of the parent's questionnaire data as accurately reflecting schools conduct. First, the parent's questionnaire data was compared to school self-reported data on their admission processes. The admission requirements declared by parents from SIMCE 2013 was matched to administrative school profiles available from the Ministry of Education, which included information about schools up to April 2014. Data matching was tested under different cut-off points to categorise schools, namely 60, 50, 40, and 30% thresholds. For the use of entry tests, there was a high agreement between the admission data reported by parents and schools (87% of agreement with a 50% threshold). In the case of parent interviews, the agreement was lower (55% of agreement with a 50% threshold); hence, I conducted a more detailed analysis to identify if schools tended to under- or over-report the admission mechanisms used. Results suggest an over-reporting of schools compared to the parent's answers, discarding the scenario where schools would be understating their admission process. A possible explanation for this is that schools could "reserve the right" to apply interviews but not necessarily end up conducting them. For example, when an applicant's sibling is already enrolled, schools may decide not to re-conduct an interview to the parents. Agreement rates were fairly consistent across different cut-off points.

Second, I compared the average number of parent's questionnaires available to the number of student assessments per school, grade and year. If the parent's questionnaire response rate is low, the data would not be as representative of the schools' admission processes. For the data used in this research, the questionnaire had an average of 85% response rate across years and grades (87% in 4th grade and 83% in 8th grade). Therefore, both exercises suggest an overall appropriateness of using parents' answers as school data.

[7] Descriptive analyses indicate that a 50% threshold would discriminate adequately between parents' responses as these are clearly grouped above and below the cut-off point. Only a small number of schools (less than 5% per year) show a response ratio between 45% and 55%.

Figure 3.1 illustrates the use of admission mechanisms by schools over time. Between 40-60% of schools use any (i.e. at least one) of these mechanisms, indicating an upward trend through the years. Play sessions, civil marriage certificates and salary certificates are the mechanisms with the lower level of use over time, as less than 5% of schools ask them. Around 10% of schools appear to ask for baptism/religious marriage certificates in their admission processes. Preschool evaluations show a slightly uneven and upward trend, rounding a 10% of average use across years. The three admission mechanisms that are most commonly used by schools are entry tests, parent interviews, and transcripts from the previous school. Between 25-29% of schools apply entry tests in their admission process; while 15-25% of schools use parent interviews, showing a slight upward trend over time. However, the use of transcripts is more irregular across years, where between 15-40% of schools appear to use them[8].

Figure 3.1 Percentage of schools using admission mechanisms across panel years



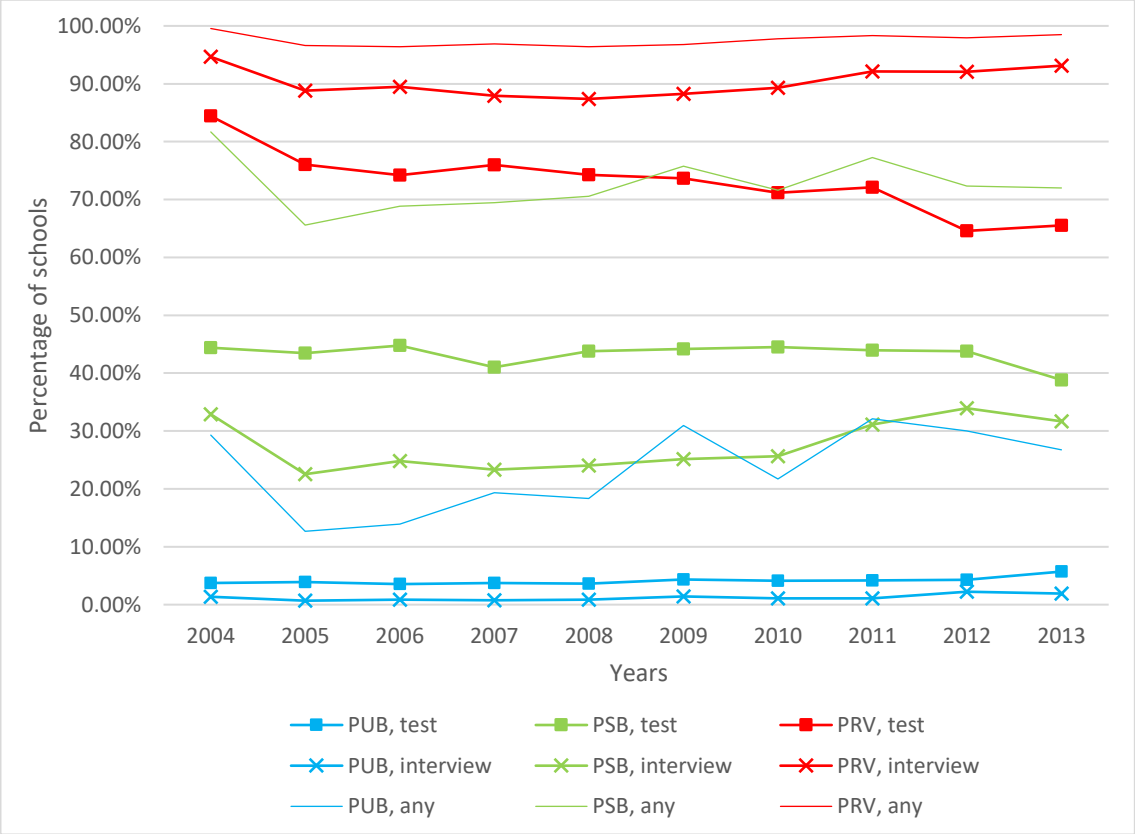
Notes: *any* indicates the use of at least one admission mechanism from the SIMCE parent's questionnaire. Schools using an admission mechanism are those in which at least 50% of parents declare that they were asked such a mechanism in the admission process. The research sample includes a total of 8,237 schools.

This research focuses on entry tests and parent interviews since they reflect different dimensions of student selection. On one hand, the purpose of applying entry tests is to measure

[8] This range of use is probably related to the entry-level assumed for the data. The use of this mechanism would not be applicable if students start school in PK or 1st grade. For secondary schools that start in 7th grade, the use of transcripts may be of more relevance.

the academic skills of the students. On the other hand, conducting parent interviews aims to gauge the family context of the students. While the former is a mechanism to discriminate individual ability, the latter discriminates by socioeconomic background. When applying these mechanisms, schools are trying to identify the ablest or the most advantaged students, and tacitly, the easier ones to serve in the educational process. Exploring these archetypes of admission mechanisms across time would provide a clearer picture of how student selection works and its effect on the school system.

Figure 3.2 Percentage of schools using Entry Test, Parent Interview, or *any* admission mechanism across panel years, by school type



Notes: The first part of each legend refers to the school type, where PUB=public, PSB=private-subsidised, and PRV=private schools. The second part refers to the school admission mechanisms, where test=use of entry tests, interview=use of parent interviews, and any=use of at least one mechanism from the SIMCE parent's questionnaire.

Figure 3.2 provides details on the use of entry tests, parent interviews and any of the admission mechanisms from the SIMCE parent's questionnaire, by school type. Considering all years, only a small proportion of public schools (between 1-6%) use entry tests or parent interviews in the admission process, while 13-32% of public schools use at least one of these admission mechanisms. Less than half of private-subsidised schools use either of these two mechanisms (23-34% use parent interviews and 39-45% use entry tests), although between 66-82% of these schools use any of the mechanisms in the admission process. Finally, the majority

of private schools use parent interviews and entry tests (87-95% and 65-84%, respectively) and practically all private schools (96-99.6%) use at least one of these admission mechanisms.

Notably, both public and private-subsidised schools constantly use more entry tests than parent interviews, whereas private schools seem to use parent interviews more across all years included in the study. This would suggest that schools receiving public funding, by focusing more on academic ability, would base their student selection on merit; while private schools seem to be more interested in selecting students based on social grounds.

Table 3.2 Descriptive statistics by admission mechanism use (years 2004-2013)

	All schools (n=8,237)		Schools using Entry Test				Schools using Parent Interview			
			Ever (n=2,152)		Never (n=6,085)		Ever (n=3,375)		Never (n=4,862)	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Reading	257.0	48.4	274.2	46.8	245.7	46.0	274.0	47.7	248.4	46.4
Math	252.5	49.2	272.6	47.7	239.4	45.6	271.8	49.4	242.8	46.1
Male	0.51	0.50	0.50	0.50	0.52	0.50	0.50	0.50	0.51	0.50
SEN	0.03	0.16	0.01	0.11	0.04	0.18	0.02	0.13	0.03	0.17
Attendance	0.89	0.19	0.91	0.16	0.88	0.20	0.91	0.16	0.89	0.20
Passed	0.93	0.24	0.95	0.20	0.92	0.26	0.95	0.21	0.92	0.25
Failed	0.03	0.15	0.02	0.14	0.03	0.16	0.02	0.14	0.03	0.16
Left	0.04	0.19	0.03	0.15	0.05	0.21	0.03	0.16	0.05	0.20
Retention	0.01	0.09	0.01	0.07	0.01	0.11	0.01	0.07	0.01	0.10
Computer	0.60	0.45	0.79	0.37	0.48	0.45	0.77	0.39	0.51	0.45
Internet	0.39	0.44	0.58	0.45	0.27	0.39	0.59	0.46	0.29	0.40
No. of books	2.84	0.90	3.21	0.92	2.60	0.81	3.21	0.96	2.65	0.81
Father's ed	3.62	1.50	4.46	1.38	3.07	1.31	4.48	1.51	3.19	1.29
Mother's ed	3.59	1.51	4.44	1.35	3.04	1.33	4.45	1.49	3.16	1.32
HH income	3.93	3.13	5.64	3.82	2.82	1.88	5.99	4.10	2.90	1.75
Transcript	0.29	0.45	0.35	0.48	0.25	0.44	0.38	0.49	0.24	0.43
Preschool	0.09	0.29	0.21	0.40	0.02	0.14	0.22	0.41	0.03	0.17
Play session	0.02	0.12	0.04	0.19	0.00	0.05	0.05	0.21	0.00	0.01
Marriage	0.02	0.13	0.04	0.20	0.00	0.03	0.05	0.22	0.00	0.03
Baptismal	0.09	0.29	0.22	0.42	0.01	0.10	0.26	0.44	0.01	0.10
Salary	0.01	0.09	0.02	0.15	0.00	0.02	0.03	0.16	0.00	0.03
Enrolment	784	650	1011	763	637	14	819	591	766	677
Public	0.47	0.50	0.10	0.30	0.71	0.46	0.10	0.30	0.65	0.48
Priv-subsid	0.46	0.50	0.74	0.44	0.28	0.45	0.70	0.46	0.34	0.48
Private	0.07	0.25	0.15	0.36	0.01	0.09	0.20	0.40	0.00	0.03
Grade4	0.64	0.48	0.65	0.48	0.64	0.48	0.65	0.48	0.63	0.48
Grade8	0.36	0.48	0.35	0.48	0.36	0.48	0.35	0.48	0.37	0.48

Note: Mean differences between schools that have ever/never used an admission mechanism were compared using t-tests. For all the characteristics shown, these differences are statistically significant at the 5% confidence level.

Table 3.2 presents summary statistics for the main variables used in the analyses for the overall sample of schools, as well as for schools that have used entry tests or parent interviews

in their admission processes. In the overall sample of schools, 47% are public, 46% are private-subsidised, and 7% are private schools. 51% of the students are males, approximately two-thirds are in 4th grade, and their average SIMCE scores are 253 in math, and 257 in reading[9]. Around 3% of the students have officially reported having some Special Educational Need; they attend, on average, 89% of the school year; 93% passes and 1% of them are asked to repeat the grade. Regarding the household characteristics of students from all schools, 60% of them have a computer, 39% have an Internet connection, and they have on average between 10 and 50 books at home. The mean educational level of their parents is secondary education completed, and their monthly household income is typically between £300 and £400.

As expected, schools that use entry tests or parent interviews differ from schools that do not use them. Compared to schools that have never used entry tests or parent interviews, schools that have ever used them are predominantly private-subsidised and private schools and show higher test scores, a greater proportion of students passing the academic year, higher attendance rates and total enrolment. Moreover, schools that ever use these mechanisms show better household characteristics and also tend to use other selective admission mechanisms. Considering all these characteristics, the differences between schools that have ever used entry tests or parent interviews and schools that have never used them are statistically significant. Hence, the analyses include these controls to account for the different school profiles.

3.3.2. Categorisation of the use of admission mechanisms over time

The school indicator of the use of admission mechanisms only reflects the use for a particular year, without considering the school's previous admission policy. Therefore, on its own, this indicator does not discriminate between schools that have changed their admission policy in a given year from schools that have not. Moreover, the decision to use an admission mechanism one year should be considered dependent on the decisions made by the school in previous years (e.g. a school that has always used a selective mechanism is more likely to use it again than a school that has never used it before). By having a school panel, this previous information can be incorporated, and schools can be better identified according to their admission policies over time.

To account for and discriminate between different types of use, *Table 3.3* details a set of new categories created by complementing two variables: the use of the admission mechanism the year that students enrolled in the school, and the use before such year. Four mutually exclusive indicators were constructed for each grade and year. Therefore, for any particular year, a school that has always used an admission mechanism is going to be differentiated from

[9] The original scale for each SIMCE evaluation has a mean of 250 and a standard deviation of 50 points.

a school that is using it for the first time. The first category identifies schools that applied the selective mechanism in enrolment year and have always used it before. The second category applies to schools that used the mechanism in the admission process of a particular year but have not always used it before; that is, schools that have started or resumed using the admission mechanism. The third category of use classify schools that did not use the admission mechanism in enrolment year, but have used it at least once before; namely, schools that have stopped using the mechanism. The last category refers to schools that have never used entry tests or parent interviews in their admission processes.

Table 3.3 Categories of admission mechanisms use

Did the school use the admission mechanism…?		Categories created
…the year students enrolled?	…before that year?	
Yes	Always	Schools that have always used it
Yes	Not always	Schools that have started/resumed using it
No	At least once before	Schools that have stopped using it
No	Never	Schools that have never used it

Across all years included in the study, 26% of schools have used entry tests, and 41% have used parent interviews at least once. When compared by grades, the distribution of these categories is relatively similar. *Table 3.4* shows that, within schools that have ever used these admission mechanisms, most of them have changed their policy over time: between 16-18% of schools have either started or stooped using entry tests, while a third of schools have changed their use of parent interviews in the admission process.

Table 3.4 Categories of use of Entry Tests and Parent Interviews, by grade

	Entry Tests		Parent Interviews	
	4th grade	8th grade	4th grade	8th grade
Always used selective admission	8%	10%	8%	9%
Changed admission policy	18%	16%	33%	32%
Never used selective admission	74%	74%	59%	59%

Note: for each admission mechanism and grade, this table shows the proportion of schools that have always, changed, or never used them. The research sample includes a total of 8,237 schools.

3.3.3. Construction of the socioeconomic index

As a proxy for the cultural capital and socioeconomic context of the students' families, a socioeconomic (SES) index was generated for each school and grade. This index was created using principal components analysis based on polychoric correlations[10] of six categorical variables of the SIMCE parent's questionnaire: access to computer and internet at home, number of books at home, highest educational level of the student's mother and father, and monthly average household income. After creating an individual-level SES index, an aggregated measure was calculated using the mean for each grade in a school. All variables are originally in the same direction, so a higher score in the index reflects a cohort with higher average socioeconomic status. As a validation exercise, I calculated mean statistics for a range of relevant variables according to three levels of the SES index. Appendix section *9.3* show further details of these results, which indicate that the socioeconomic measure would accurately capture different social contexts of the students in the sample.

3.4. Methods

To estimate the effect of selective admissions, I take advantage of the fact that, before the admissions reform, each school defined which mechanisms (if any) to apply in their admission process. Mainly, I exploit the changes in the use of admission mechanisms using a flexible difference-in-differences approach and a two-way fixed effects panel data model. The flexible approach allows for schools to have different timings in the use of selective mechanisms, and the fixed effects allow me to exploit two sources of variation: between schools, and across time. Therefore, the main effect is identified by comparing the change on the outcomes in schools with different categories of use of admission mechanisms, to that in schools that have never used an admission mechanism.

The advantage of using panel data in this research is that it allows accounting for unobservable features that could, otherwise, be biasing the estimates. Section *9.4* of the Appendix shows that estimates from pooled cross-sectional models are of similar magnitude, suggesting that even when controlling for a range of observable and unobservable characteristics, the effect of selective admissions is detectable.

By using school and year fixed effects, I assume that these unobserved features are correlated with at least one of the observable characteristics included in the models[11].

[10] The first component of the polychoric PCA, explaining 69% of the variance, was used as the SES index.

[11] As an additional validation exercise, Sargan-Hansen tests were conducted to assess over-identification and the pertinence of using fixed effects under the assumption of heteroscedasticity with unbalanced panels. For all models, the null hypothesis that the differences between RE and FE estimates are not systematic was rejected.

Specifically, school fixed effects are included to account for omitted variables that are particular to each school and constant over time. Examples of these could be the school educational project, the local educational context in which the school is situated, or school inputs such as leadership, climate or infrastructure. Additionally, under the hypothesis that schools serve and specialise in capturing a certain profile of families (e.g. through the use of admission mechanisms), this relatively constant pool of families that apply to a school would also be accounted for by the school fixed effects included in the models.

On the other hand, year fixed effects would account for unobserved characteristics that vary over time but are common to all schools. These could represent, for example, the effect of the 2008 Preferential School Subsidy Act, which gives a higher subsidy to the most vulnerable students. With this national policy, schools could have opted to stop selecting students by their academic or socioeconomic background and attract those most vulnerable.

This paper investigates two main outcomes of interest: standardised test scores for math and reading as measures of academic performance, and a school socioeconomic index as a measure of the cohort socioeconomic composition. The variables of interest are the categories of use of entry tests and parent interviews in the schools' admission processes. Additionally, the effect of selective admissions is estimated conditional on other observable characteristics of students and schools. I control for observable student features such as gender, several academic indicators to account for the student educational context (grade, Special Educational Needs, annual school attendance rate, academic status by the end of the year, and retention), and other family and household indicators to control for the student socioeconomic background (access to computer and internet, and number of books at home; highest educational level of mother and father; household average monthly income). Observable school characteristics relate to the use of other admission mechanisms to control for additional ways of selective admissions, and total enrolment to account for the school size each year (see section *9.1* of the Appendix for more details on these variables).

For all models, the standard errors were clustered at the school level to allow for within-school correlation. The baseline linear regression model with fixed effects is formally specified as follows:

$$Test_{igst} = \alpha + \beta_1 \mathrm{Adm}_{gst} + \delta \mathrm{X}_{igst} + \theta \mathrm{Z}_{gst} + \mu_s + \gamma_t + \varepsilon_{igst} \quad (1)$$

$$SES_{gst} = \alpha + \beta_1 Adm_{gst} + \delta \mathrm{W}_{igst} + \theta \mathrm{Z}_{gst} + \mu_s + \gamma_t + \varepsilon_{gst} \quad (2)$$

Equation (1) estimates the effect of selective admissions on academic performance, where $Test_{igst}$ is the standardised test score for student *i* in grade *g* of school *s* in year *t*. Adm_{gst}

is the variable of interest: the cross-sectional use of entry tests or parent interviews in the admission process for grade *g* of school *s* in year *t*; X_{igst} represents the vector of student characteristics described above for student *i* in grade *g* of school *s* in year *t*; Z_{st} represents the vector of school characteristics mentioned above for grade *g* of school *s* in year *t*; μ_s is the unobservable school effect; γ_t is the unobservable year effect; and ε_{igst} is the idiosyncratic error term. Similarly, equation (2) identifies the effect of selective admissions on the cohort socioeconomic composition measure, where SES_{gst} is the socioeconomic index for grade *g* of school *s* in year *t*. The only difference with the first specification is that W_{igst} represents a second vector of student characteristics excluding the socioeconomic background covariates (now part of the outcome measure).

An additional model is specified to discriminate between selection patterns over time. Following *Table 3.3,* I include categories that reflect the use of admission mechanisms in both enrolment year and before that year.

$$Test_{igst} = \alpha + \beta_2 \mathrm{Use}_{gst} + \delta \mathrm{X}_{igst} + \theta \mathrm{Z}_{gst} + \mu_s + \gamma_t + \varepsilon_{igst} \quad (3)$$

$$SES_{gst} = \alpha + \beta_2 \mathrm{Use}_{gst} + \delta \mathrm{W}_{igst} + \theta \mathrm{Z}_{gst} + \mu_s + \gamma_t + \varepsilon_{gst} \quad (4)$$

Equations (3) and (4) show these specifications, where the variable of interest, Use_{gst} is the vector of dummy variables identifying the categories of use of admission mechanisms for grade *g* of school *s* in year *t*. The reference category refers to schools that have never used the admission mechanism. For those schools that did not use the admission mechanism in enrolment year, a second indicator estimates the additional effect of having used it at least once before (i.e. schools that have stopped using the mechanism). A third dummy variable is the baseline effect of using the admission mechanism in enrolment year, but for schools that have not always used it before (i.e. schools that have started or resumed using it). A fourth indicator specifies the additional effect of always using the admission mechanism (i.e. in enrolment year and always before).

In a second stage of analysis, the effect of selective admissions is linked to the levels of segregation in schools, measured by a within-grade variation indicator for both academic and socioeconomic outcomes. The coefficient of variation was calculated for each grade-school combination per year, in order to create a comparable measure of the distribution of the academic and socioeconomic composition, mainly focusing on the dispersion of these indicators. In order to calculate the coefficient of variation, the SES index was re-scaled to range from zero to one, so for interpretation purposes, smaller coefficients of variation indicate less disperse

distributions. The formal specification of these models remains the same, where $Test\_Var_{igst}$ indicates the coefficient of variation of the math or reading test scores, and SES_Var_{gst} is the coefficient of variation of the SES index for grade *g* of school *s* in year *t*.

Finally, to address potential differences in the use of selective admissions by grade (i.e. when it is permitted, or not, by the General Education Act), I estimate heterogeneous effects for schools that always use the admission mechanisms, compared to schools that do not always use them, according to their school type and grade. In this analysis, the comparison group would be schools that either never use these admission mechanisms or that have used them but not every year included in the data. Therefore, I exploit two sources of variation to estimate these heterogeneous effects: first, from schools that change their administrative type (e.g. from public to private-subsidised), and second, from the student composition for each school-year combination (i.e. student-level data on gender, academic indicators, and family and household indicators, which change for each school-year cohort).

$$Test_{igst} = \alpha + \beta_3 \text{Always}_{gst} + \lambda Type_{st} + \varphi\left(\text{Always}_{gst} * Type_{st}\right) + \delta \text{X}_{igst} + \theta \text{Z}_{gst} + \mu_s + \gamma_t + \varepsilon_{igst} \quad (5)$$

$$SES_{gst} = \alpha + \beta_3 \text{Always}_{gst} + \lambda Type_{st} + \phi\left(\text{Always}_{gst} * Type_{st}\right) + \delta \text{W}_{igst} + \theta \text{Z}_{gst} + \mu_s + \gamma_t + \varepsilon_{gst} \quad (6)$$

Equations (5) and (6) show these specifications, where φ and ϕ indicate the additional effect of always using entry tests or parent interviews in the admission process, compared to the rest of schools, on the academic and socioeconomic outcomes by school type. The grade level is accounted for by conditioning the model to each grade subgroup included in the data.

3.5. Results

This section presents the results in the following subsections: First, main results for the effect of selective admissions on academic and socioeconomic indicators and their variation measures are shown according to the categories of use, where schools that never use entry tests or parent interviews are the reference. Second, these categories are analysed by school type and grade to address the use of selective admission processes in different contexts of the General Education Act. Finally, I test the identification strategy for these analyses using a placebo difference-in-differences.

3.5.1. Main results

Overall, results indicate that the use of entry tests and parent interviews has a small positive effect on academic performance. *Table 3.5* shows that schools that always use these admission mechanisms show higher test scores and higher SES index, both with less disperse variation at the cohort level. Even when the use is intermittent, that is, schools that have started or stopped using admission mechanisms throughout the years of the observed panel, there is a smaller but positive effect on academic performance. However, an occasional use of selective admissions has a negative effect on the grade-level SES composition. This would suggest that only selecting students consistently by ability and family background would help schools have a higher overall socioeconomic composition.

Table 3.5 Effect of selective admissions

	Use of Entry Tests			Use of Parent Interviews		
	Reading (1)	Math (2)	SES index (3)	Reading (4)	Math (5)	SES index (6)
Panel A: Standardised measures						
Always users	0.05**	0.09**	0.02*	0.06**	0.06**	0.03**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Started / Resumed	0.01	0.05**	-0.00	0.02**	0.03**	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Stopped	-0.01	0.03*	-0.02*	0.03**	0.03**	-0.02**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Panel B: Variation measures						
Always users	-0.02	-0.07*	-0.02	-0.12**	-0.06	-0.05**
	(0.03)	(0.03)	(0.01)	(0.04)	(0.03)	(0.02)
Started / Resumed	0.05*	0.01	0.05**	-0.05*	0.02	0.01
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
Stopped	0.07**	0.05*	0.05**	-0.02	0.05*	0.03*
	(0.03)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
N of students	3,300,108	3,300,354	3,303,583	3,300,108	3,300,354	3,303,583
N of schools	8,114	8,113	8,156	8,114	8,113	8,156

Notes: Panel A shows the effect of using entry tests and parent interviews in the admission process on reading and math test scores (columns 1, 2, 4 & 5) and on the socioeconomic composition of the cohort (columns 3 & 6). Panel B shows the effect of selective admissions on the grade-level variation measures of tests scores and the SES index. Robust standard errors in parentheses. ** p<0.01, * p<0.05.

Specifically, schools that always select students by taking an entry test or having parent interviews show higher (5-9% of a standard deviation) and less disperse (7-12% of a sd) reading and math test scores. This could indicate that selective schools would be able to translate the use of these admission mechanisms into a relatively better and more homogenous academic performance, compared to schools that never select.

The effect on academic performance seems to persist in schools that have had student selection but not in a consistent way through time. Having used entry tests or parent interviews at least one year has a smaller but positive effect on reading and math test scores (2-5% of a sd increase). However, the dispersion of these academic effects is mixed: while schools that have used entry tests show higher dispersion of test scores (5-7% of a sd), schools that have used parent interviews show a rather inconsistent pattern.

Schools that always use entry tests or parent interviews show a positive effect (2-3% of a sd) on the socioeconomic composition of the cohort and, simultaneously, a negative effect (2-5% of a sd) on the variation if this socioeconomic measure, indicating that selective schools manage to have student bodies with higher and more homogeneous socioeconomic status, compared to schools that never select. On the contrary, starting or stopping using these admission mechanisms has the inverse effect, signalling that by not selecting students in some year(s), the socioeconomic composition is lower and more heterogeneous than in schools that never select students.

3.5.2. Heterogeneous effects by grade and school type

As discussed in section *3.2.2*, schools receiving public funding (traditional public and private-subsidised schools) were not permitted to select students in 4th grade (10-year-olds) but were allowed to select by ability on 8th grade (14-year-olds). This section discusses the results for heterogeneous effects by grade and school type and is structured by outcomes.

Academic outcomes

Table 3.6 shows that in 4th grade, private-subsidised and private schools seem to drive the positive effect on test scores: compared to schools that either never use parent interviews or have used them sporadically, schools that have always had parent interviews in the admission processes show an effect of between 10-19% of a standard deviation increase in reading scores, and also a positive effect on the variation of math test scores (44-39% of a sd). This would suggest that the use of parent interviews would be a more effective way for these schools to gain an academic benefit from student selection. Schools that always use entry tests do not seem to yield an additional academic benefit from this selective pattern.

In turn, for public schools, always using parent interviews does not translate into test scores gains, but it does seem to make their dispersion significantly smaller. So, on average, these schools would increasingly gather students with similar academic achievement but would not show an overall higher academic performance.

Table 3.6 Effect of always using selective admissions, by school type and grade

	Use of Entry Tests			Use of Parent Interviews		
	Reading (1)	Math (2)	SES index (3)	Reading (4)	Math (5)	SES index (6)
Panel A: Standardised measures - 4th grade						
Always users, Public	0.07	0.07	0.01	-0.01	-0.04	0.06
	(0.05)	(0.05)	(0.02)	(0.05)	(0.06)	(0.05)
Always users, P-subsid	-0.03	-0.03	0.00	0.10*	0.08	-0.02
	(0.05)	(0.05)	(0.02)	(0.05)	(0.06)	(0.05)
Always users, Private	0.04	0.04	0.12**	0.19**	0.11	0.04
	(0.05)	(0.06)	(0.02)	(0.07)	(0.08)	(0.06)
Panel B: Variation measures - 4th grade						
Always users, Public	0.04	-0.03	0.05	-0.45**	-0.57**	-0.06
	(0.12)	(0.11)	(0.04)	(0.13)	(0.15)	(0.17)
Always users, P-subsid	-0.07	-0.01	-0.07	0.22	0.44**	-0.03
	(0.12)	(0.11)	(0.04)	(0.14)	(0.15)	(0.17)
Always users, Private	-0.25	-0.26*	-0.26**	0.21	0.39*	-0.12
	(0.13)	(0.11)	(0.04)	(0.15)	(0.18)	(0.18)
N students	2,191,454	2,191,469	2,194,394	2,191,454	2,191,469	2,194,394
N schools	7,878	7,877	7,920	7,878	7,877	7,920
Panel C: Standardised measures - 8th grade						
Always users, Public	0.28*	0.37**	0.16**	0.21	0.27	0.29**
	(0.11)	(0.12)	(0.05)	(0.17)	(0.22)	(0.02)
Always users, P-subsid	-0.26*	-0.31*	-0.12*	-0.17	-0.27	-0.19**
	(0.11)	(0.12)	(0.05)	(0.17)	(0.22)	(0.03)
Always users, Private	-0.16	-0.27*	0.02	-0.12	-0.25	-0.01
	(0.11)	(0.12)	(0.05)	(0.20)	(0.24)	(0.03)
Panel D: Variation measures - 8th grade						
Always users, Public	-0.26	-0.48	-0.12	-0.56	-0.75*	-0.44**
	(0.24)	(0.26)	(0.08)	(0.40)	(0.30)	(0.03)
Always users, P-subsid	0.30	0.35	0.08	0.45	0.72*	0.23**
	(0.25)	(0.26)	(0.08)	(0.41)	(0.31)	(0.03)
Always users, Private	0.38	0.19	-0.15	0.48	0.99*	0.07
	(0.28)	(0.27)	(0.08)	(0.51)	(0.41)	(0.06)
N students	1,108,654	1,108,885	1,109,161	1,108,654	1,108,885	1,109,161
N schools	6,072	6,072	6,072	6,072	6,072	6,072

Notes: Panels A and B show the effect of always using selective admissions on standardised and variation measures of test scores and SES index for 4th grade. Panels C and D show the same group of coefficients for 8th grade. Robust standard errors in parentheses. ** p<0.01, * p<0.05.

In 8th grade, where student selection is allowed, the results on academic achievement show more inconsistent patterns. Against schools that either never or sometimes use selective admissions, public schools that always apply selective admissions show considerable test scores gains of 21-37% of a standard deviation, although these are only significant for the use of entry tests, while also showing less disperse distributions of these academic effects. Conversely, private-subsidised and private schools that always ask for entry tests in their admission process show a decrease in their test scores (26-31% of a sd), suggesting that, compared to schools that do not show this constant pattern, these schools cannot seem to translate the selection of students by ability into an academic gain.

Socioeconomic composition outcome

In 4th grade, private schools that always select by ability show higher levels and lower dispersion of the cohort socioeconomic composition, compared to schools that have not always used entry tests in their admissions. This way, by selecting their intake by ability, private schools would be setting-up particular student bodies, composed of similar families with higher SES. In 8th grade, the small group of public schools that always use entry tests or parent interviews show higher (16-29% of a sd) cohort socioeconomic composition with lower dispersion. That is, the same relationship seen in 4th grade for private schools can be recognised in 8th grade for public schools. The inverse is identified for private-subsidised schools as they manage to attract a wider range of students in socioeconomic terms, thus showing a negative effect on the overall cohort composition. Finally, when compared to not-always selective schools, private schools that always apply selective admissions show no significant effects on their cohort socioeconomic composition.

3.5.3. Model specification check: placebo test

Difference-in-differences estimations are mainly based on the parallel trends assumption (Angrist & Pischke, 2009), which in the context of this research would state that the trends for the outcome of interest among schools would be the same in the absence of selective admission practices. A way to test this assumption would be to perform an additional estimation using treatment groups that are known to be unaffected by this admission policy so that it would be expected for these effects to be equal to zero (Gertler, Martinez, Premand, Rawlings, & Vermeersch, 2016). Therefore, to test the model specification of this study, I conducted a placebo test by manipulating the treatment groups and artificially changing the use of the selective admission mechanisms at a different time point.

The analysis compares schools that have ever used entry tests or parent interviews in their admission processes (the placebo treatment group), against schools that have never used any

of these two mechanisms (the placebo control group). The objective is to "activate" the use of selective admission mechanisms synthetically. Following the data availability for each grade (see *Table 3.1*), the first two years for 4th grades and the first year for 8th grades are *turned off* and, this way, all schools start as non-selective. Then, the placebo treatment schools are *turned on* artificially in years where they have not used the admission mechanisms yet, and they stay *on* until the year these schools started applying the admission mechanism in reality, at which point they are dropped from the analysis. That is, they are provided with a placebo treatment until they actually start using the selective admission mechanisms.

Because the placebo test aims to manipulate admission policy changes, it means that schools that have always used these admission mechanisms are excluded from this validation analysis since they would be always *on*. This also implies that the last year of data is also dropped, as there are no schools left that have not used the selective mechanism yet. By using this placebo treatment, I am able to estimate yearly effects using interaction terms, as well as an overall placebo effect combining all years included in the study.

Results are shown in the following figures for the use of each admission mechanism and grade. These graphs display the same outcomes considered in the main analyses: academic performance in reading and math, and the cohort socioeconomic composition measure. Additionally, point estimates and their corresponding confidence interval are shown for each of the outcomes. The yearly effects are shown in solid lines, whereas the overall placebo effects are shown in dashed lines and are the estimates of interest for this section.

Figure 3.3 and *Figure 3.4* show the placebo treatment effects for the use of entry tests and parent interviews in 4th grade, respectively. For the reading test scores and socioeconomic composition outcomes, these figures show what was expected as the placebo effects are statistically indistinguishable from zero. This would suggest that there are no unobservable variables driving differences in these outcomes between the placebo treatment and control groups, and that these results would yield unbiased estimates of schools selective admissions. However, the placebo test of the use of entry tests on academic achievement in math shows effects different from zero. This would indicate the presence of confounding factors inducing the differences in trends for this outcome between these groups of schools.

The results for 8th grade show a similarly inconsistent pattern. As illustrated in *Figure 3.5* and *Figure 3.6*, the placebo test of the use of parent interviews on academic outcomes shows statistically zero effects. But for the use of entry tests, this is only true for reading test scores. Moreover, for the socioeconomic composition outcome, the placebo test shows effects different from zero, suggesting that the differences in trends for this outcome would be affected by confounding factors.

These results should then be interpreted with caution as the parallel trends assumption would not be completely satisfied for these schools in the absence of the selective admission policy (Gertler et al., 2016).

Figure 3.3 Placebo test effects for the use of entry test in 4th grade

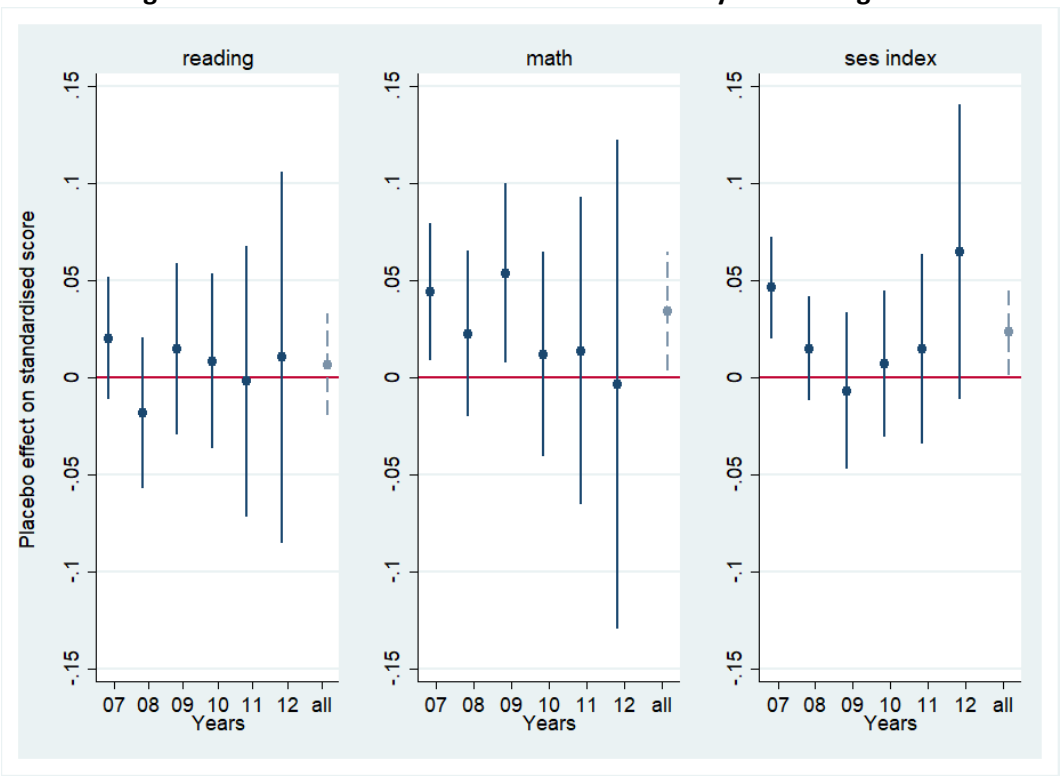


Figure 3.4 Placebo test effects for the use of parent interview in 4th grade

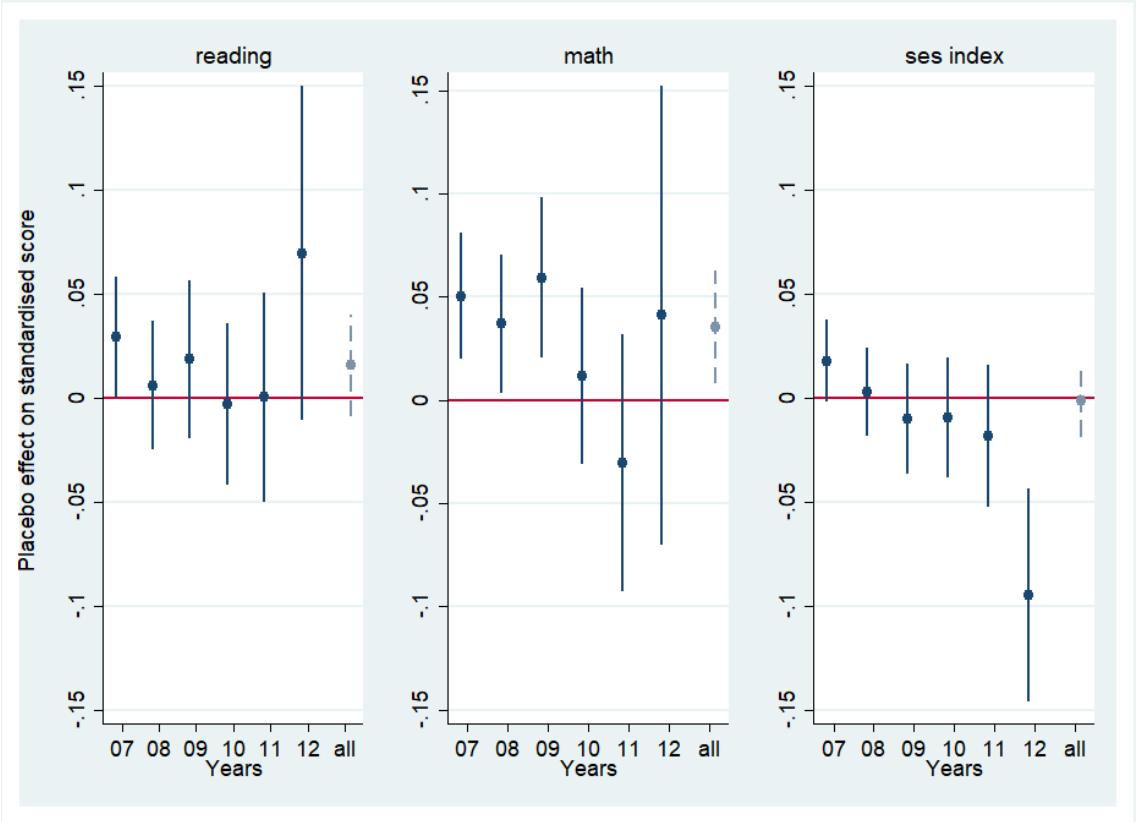


Figure 3.5 Placebo test effects for the use of entry test in 8th grade

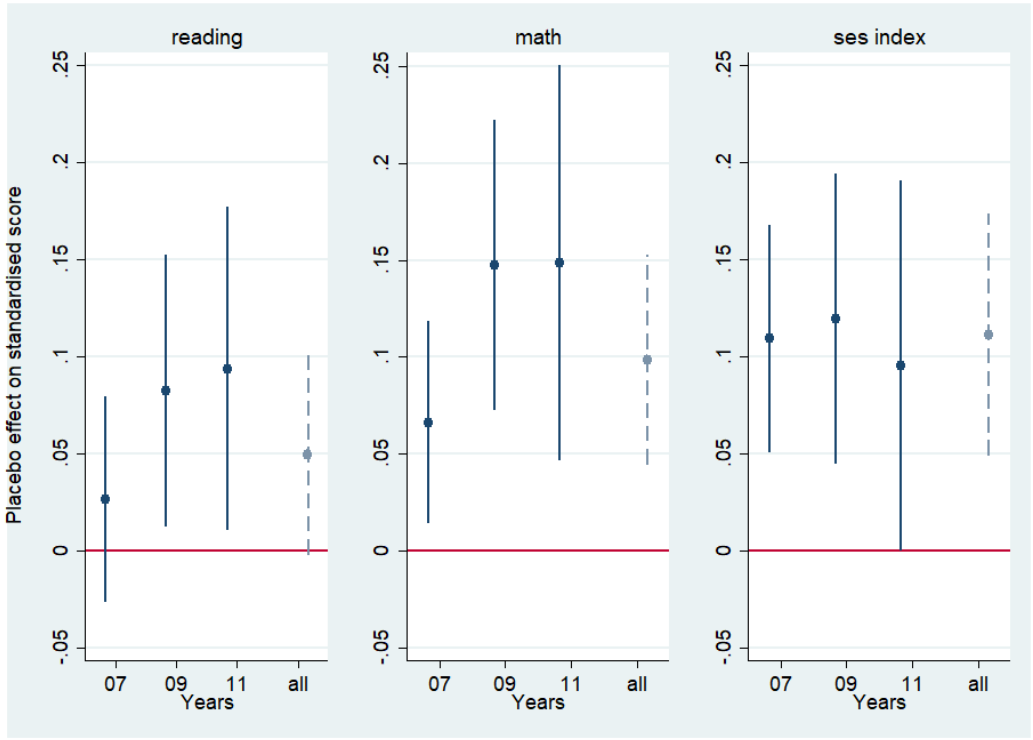
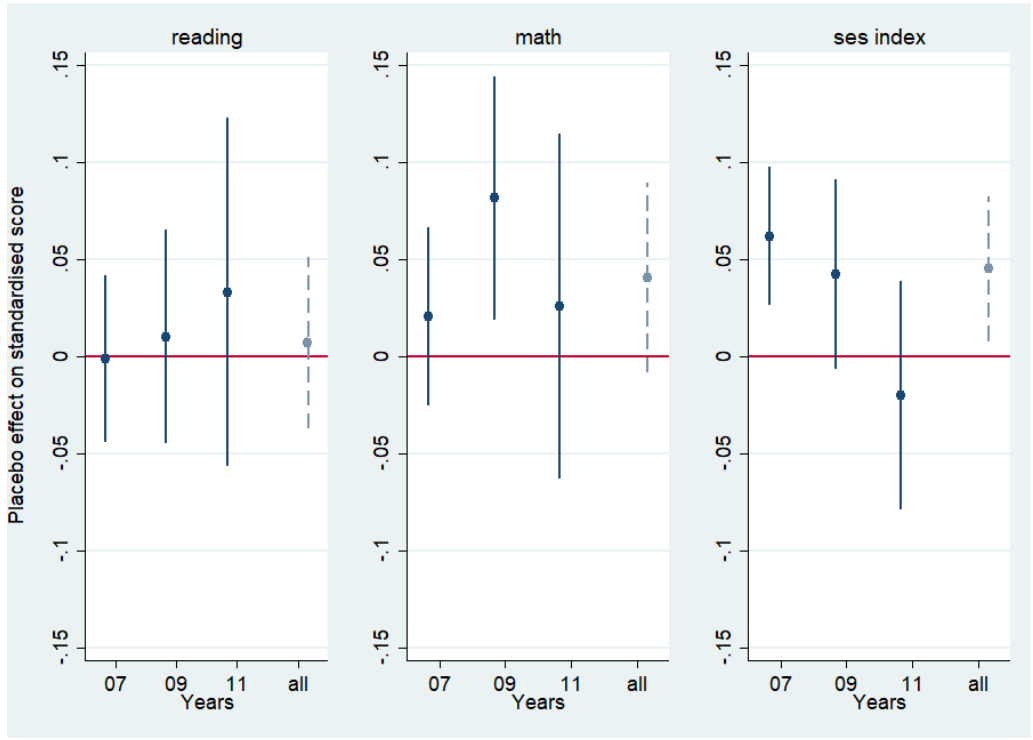


Figure 3.6 Placebo test effects for the use of parent interview in 8th grade



3.6. Discussion

This research examines how selective admission practices affect schools, both in terms of their academic performance and socioeconomic composition, and also test for heterogeneous effects by school types and grades. The analysis focuses specifically on the use of entry tests and parent interviews in the school admission processes as archetypes of student selection by individual ability and family background. Based on data from parents, the analysis focuses specifically on the use of entry tests and parent interviews in the school admission processes as archetypes of student selection by individual ability and family background. I use a flexible difference-in-differences approach and panel data models with school and time fixed effects to exploit the changes in the use of these school admission policies.

Primary results indicate that, overall, students at selective schools show smaller and also more similar reading and math test scores compared to non-selective schools. This would suggest that, on average, these schools would not be having a substantial academic benefit from selecting students by ability or family background.

When looking at the effects by grade and school type, results show that in 4th grade (10-year-olds), always selective private and private-subsidised schools seem to drive the positive academic effects, and selective private schools are able to attract similar families with higher socioeconomic backgrounds. Then, although private schools are exempt from student selection regulations, early on the school trajectory, these schools would be creating educational opportunities for just a small group of families.

In turn, the few public schools that always select in 8th grade show considerable test scores gains and manage to gather more similar students from higher socioeconomic backgrounds. This results would be in line with previous research focused on Public High Schools of Excellence, which are highly selective and attractive to families. Although most socially disadvantaged students attend public schools, the authors show that these schools' intake is mainly based on students from higher socioeconomic status (Allende & Valenzuela, 2016).

Results appear to go in the direction of the two hypotheses considered in this research, suggesting that the Chilean school choice system had been tacitly placing a central role in the schools' ability to select a particular profile of students. As described in section *3.2.3*, academic performance in Chile is highly correlated with the socioeconomic background of students and schools; hence, the student handpicking of these selective schools would make them more likely to increase their academic performance. There is evidence of this sorting effect for the Chilean case (Hsieh & Urquiola, 2006; Mizala & Torche, 2012); however, this is the first study using panel data that finds an effect of student selection practices on sorting.

Moreover, sorting in school choice systems is usually understood as the shift of the best students from public schools to private-subsidised schools (Gallego & Sapelli, 2007). This research offers an additional perspective to this effect by focusing on the schools' selection. Even if considering that parents make the first choice when deciding to apply to the school, the school's selection of these applicants shows to be also relevant. While student selection does not explain completely the high levels of socioeconomic segregation in Chile, the fact that this relationship has been measured using panel data for the first time in the Chilean context is an advancement for the further development of this topic. For future research, it would be interesting to test if the effect of selective practices on academic performance is statistically mediated by the school socioeconomic composition.

These results are especially relevant for public schools, particularly in 8th grade. When selecting by ability, they show higher gains in academic performance, while when selecting by socioeconomic background, they show a higher increase in the school socioeconomic composition, compared to non-selective schools. In general, these schools are the least likely to use selective mechanisms: 24% of them use *any* admission mechanism. Moreover, 9% of the schools that always use entry tests, and 15% of the schools that always use parent interviews are public, which corresponds to approximately 136 and 265 schools nationwide, respectively. Yet, those public schools that regularly use entry tests or parent interviews seem to be particularly effective in their use and appear to be able to achieve the greatest benefit from it. In a school choice system that is rather unbalanced for school competition, and despite being a small group, these selective public schools manage to excel and compete with private and private-subsidised schools. This could be related to the fact that this sector is less selective. Assuming that the general profile of students that attend each sector is relatively fixed (Elacqua et al., 2013), it may be that when the use of selection mechanisms is widespread, its effect becomes more ineffective.

By contrast, private schools are those that most frequently use selection mechanisms in their admission processes (98% use *any* type of admission mechanism), and that continuously use entry tests and parent interviews in particular (72% and 90%, respectively). When always using entry tests in the admission process, these schools show socioeconomically higher and more homogeneous student bodies; however, they barely show differential effects on academic performance. These schools typically outperform other schools in standardised tests (OECD, 2014), hence, the fact that in this research they show no academic benefit compared to non-selective schools could indicate that the reputation of private schools would be based on student handpicking rather than on a strong school value-added. Then, these schools could be using admission mechanisms as a mere signal of quality. For further research, it would be interesting

to explore the motives and actual instruments that these schools use in the admission processes in order to better understand their purpose and effect.

The research also provides some indications on the importance of addressing the selection of students as a matter of educational policy. As this paper tries to show, student selection affects socioeconomic segregation in education, and this is particularly relevant in the context of the current educational reform. Along with focusing on the immediate results of the new policies, future research could explore the changes in student selection practices, and measure how the new centralised school admission system affects the academic performance and socioeconomic composition of schools. Student selection would be expected to decrease in the short term since schools would no longer be able to define their admission processes. Then, schools would need to focus on their value-added in order to attract students. Additionally, a main concern about the School Inclusion Act (2015) is that it only applies to publicly funded schools (that is, public and private-subsidised schools); hence, private schools are not required to adhere to these new regulations. It would be of particular interest to follow how this sector continues with their student selection practices given that all other schools would be under the centralised admission system.

Finally, there are three main reasons that restrict the causal interpretation of these results. First, and as it has been mentioned before, the variables of interest in this research –the use of entry tests and parent interviews– are based on parent's questionnaire data and are asked in retrospect. Even though several measures were adopted to address this issue accordingly, the idea that results may have some measurement error cannot be entirely discarded. Second, the specification test results show that for some of the models used, the identification strategy could be confounding other factors that affect the outcomes. Since these confounding factors are formally embedded in the error term, it would be difficult to propose a valid rationale for these results.

Third, while this research sought to include all relevant information on student selection, it does not include data on the additional co-payment fees that some schools charge to families. These data are only publicly available for one of the ten years the research covers. The information is relevant, given that the extra fee can be understood as another strategy to select students or families. Thus, a school could potentially charge these additional tuition fees but not use entry tests or parent interviews. If such fees are constant across time, then its effect would be accounted for by the school fixed-effects. If I assume, however, that the extra fees are time-varying, then the lack of this information could potentially bias the results. Evidence for the Chilean case indicates that there would be no relationship between these extra fees and academic performance, once the school's socioeconomic composition is accounted for (Mizala & Torche, 2012). Yet, there is also evidence indicating that the extra fees have an effect on

school segregation (Elacqua et al., 2013; Valenzuela et al., 2009). Measuring the effect of student selection on academic performance without extra fees data would, in first instance, generate no further concerns. However, the estimated effect of student selection on the school socioeconomic composition could be more precise if the models accounted for the use of extra tuition fees. Incorporating this information in further research is, then, desirable.

4. School lotteries and equal opportunities: Evaluating the impact of a case study in the context of a school admissions reform

4.1. Introduction

The Chilean school system is currently introducing a new admissions system, where schools with public funding are no longer in charge of this process and transfer its management to the Ministry of Education. This centralised system defines certain priority criteria, after which all students are sorted and assigned according to a random algorithm. Therefore, the new admissions regulation aims to provide equal opportunities to those who apply to a school (Ministry of Education, 2015).

The objective of this chapter is to inform this ongoing reform by evaluating the experience of a new school using random admission before the centralised system was in operation in that municipality. It intends to do so by, first, contextualising what happens when a new school with non-selective admission practices opens in an area; second, understanding the extent to which there is evidence to translate this admission mechanism in an effect on student-level outcomes; and third, analysing the implications these effects may have regarding the new policy scenario that the admissions reform entails. Methodologically, there is a value in evaluating a case like this since new, relevant, and rigorous evidence is provided on school random admissions. This research intends to make an empirical contribution on whether school lotteries are a feasible way to address education inequality. Public policy is better made when it is evidence-based; hence, this paper intends to shed some light on the education reform and the debate around it.

With the new admission system, any new school would be in a scenario similar to that of the school of this research since all publicly funded schools would be strictly non-selective. This regulation could potentially make it harder for schools to generate educational value, particularly for those schools that had selective admission practices. Moreover, starting a new school without controlling its intake could be a challenge, especially in the higher grades, as students arrive at the new school with educational skills that may be difficult to overcome if they are deficient. Overall, this paper explores the question of whether equal access to education can be translated into equal learning opportunities. Therefore, this paper proposes to contribute to the literature on school lotteries, and more broadly to that on admission processes, by evaluating the effect of using school randomised admissions in the Chilean context.

The research focuses on a new private-subsidised school offering primary and secondary education that used a lottery system to allocate its entire student body. The school was highly attractive to families due to its educational project, which is a combination of high academic expectations and integral education, and its access regulations, being a fee-free school and without student selection. This led them to receive more applications than the number of places it had available. To address issues of selection bias, I exploit the school lottery data for its first two years of admission to estimate the intention-to-treat effect of being able to enrol in this new school. Taking advantage of these school data is a valuable opportunity to approximate the implications of the reform, as there is currently little data in Chile on this topic, particularly for higher school grades.

Results indicate there is self-selection in the students and families that applied to the school. Moreover, while most schools in the municipality are private-subsidised, this group of families are sufficiently attracted to the school's opening to prompt its oversubscription from the beginning. The analyses on attendance, final mark, test scores, and grade promotion show negative effects for students who enrol in the school compared to students who did not. However, as the school establishes, these differences become negligible. The limitations of the study are mainly related to the quality of the school lottery data. However, even with these "broken data", the results are consistent against several robustness checks, suggesting that the conclusions presented here are reliable.

The rest of the paper is structured as follows: The next section describes the international literature around school lotteries, introduces the school admissions reform in Chile along with the context of the case study. The third section presents the data and particular features of the sample used in the research. Fourth, the methods section specifies the identification and analysis strategies, assumptions, and potential validity issues. The fifth section starts with descriptive statistics to contextualise the primary analyses of the impact of being able to enrol in the school on students' academic outcomes. I close with a final section discussing these results in terms of the previous literature and the implications it could have for the Chilean school admissions system.

4.2. Background

4.2.1. School lotteries

As presented in Chapter two, there are several examples in the international context of evaluations of school systems using lottery admissions, from China to the United Kingdom, and mainly in different areas of the United States. These differ in terms of the school contexts they

focus on, their sample sizes and the range of outcomes measured, and the meta-analysis of these studies showed positive but small effects on academic achievement.

One of the largest studies available using school lotteries in multiple schools contexts included 36 middle charter schools, an equivalent to private-subsidised schools in Chile, across 15 states of the U.S. (Gleason, Clark, Tuttle, Dwoyer, & Silverberg, 2010). The authors find no effect on a variety of student outcomes, such as academic achievement, attendance, promotion and behavioural measures, driven mainly by the considerable heterogeneity of individual results within the sample of schools. However, they do see a significant positive effect on student and parent satisfaction. In contrast, a study looking at 42 charter schools located in disadvantaged areas of New York City (Hoxby & Murarka, 2009) and another that evaluated one boarding charter school for socioeconomically vulnerable students in Washington, DC (Curto & Fryer, 2014) found positive effects on both reading and math performance.

More related to the context of this research, there are also experiences in other South American countries of particular schools that incorporate lottery admissions. While these studies are situated in different school contexts and present contrasting results, they provide a useful framework for the case of this research.

Di Piero (2014) examines the case of an Argentine public high school in the city of La Plata that for decades has used lotteries as part of their admission process. However, this qualitative study suggests that the use of random admissions does not guarantee the access, progression or graduation of students at this school. The equality of opportunities that this school provides through the lottery admission conflicts with a 30% dropout rate. According to the author, this would be due to a "cleaning" process within the school where the remaining students are those who are better aligned with their educational project. Therefore, the school continues to serve upper-middle sectors of the student population.

Another example is a private middle school in Montevideo, Uruguay (Balsa & Cid, 2016), which serves students below the poverty line in an area with a very low high school graduation rate. This impact evaluation of one cohort of the school exploits the oversubscription of applicants to their first grade and the use of randomisation to assign them after a priority criterion for siblings of current or former students. Despite its small sample size, the study finds that after one year, having won the lottery admission reduced significantly the retention and drop-out rates of students, and had a positive effect on their future educational expectations. However, the authors find no effect on academic performance.

In the Chilean context, school admissions via lottery have not been commonly studied. Before the admissions reform, some schools claimed to use lotteries in their admission process. Yet, according to personal interviews and informal contacts with these schools, randomisation was mainly used to allocate a few available places after considering priority admission criteria,

which were determined by each school. This hinders studying these schools for two reasons. First, because of the difficulty of identifying all the schools that use these mechanisms. The Ministry of Education did not fully account for this information as, before the reform, this was the schools' duty. Second, the small number of places commonly allocated by random assignment in each school would not provide enough data and power to produce reliable estimates.

4.2.2. The Chilean new admissions reform

The Chilean school system is mainly based on school choice without any residential restrictions. Then, students can apply to any school regardless of where they live and the location of the school. It is configured by three major school types: public schools, which are funded and run by a public administrator and account for roughly less than half of the schools in the country; private-subsidised schools, which are publicly funded and run by a private administration, accounting for slightly more than half of the schools; and private schools, which are privately run and funded by their own families, and represent just a small proportion of the schools. The Chilean school system is also highly segregated, both at the academic and socioeconomic level, where a key component to it is the schools' admission policies (Ramírez Fernández, 2017). Before the School Inclusion Act of 2015, each school was responsible for its own admission process, making it challenging to control discriminatory practices in these processes. However, this is one of the core issues of that this act aimed to address. Based on the principles of "transparency, inclusive education, universal accessibility, equity, no arbitrary discrimination, and school choice" (Ministry of Education, 2015), this educational reform introduces a centralised admissions system for all schools that receive state funding. Hence, it makes a fundamental shift in the admissions process regulation, from each individual school to the Ministry of Education.

In the new admissions system, each student applies using the same online platform and the regulation specifies that schools need to admit all applicants if they have places available. In case of oversubscription, there are priority criteria mainly related to siblings already enrolled in the school, students of high vulnerability as measured by the Ministry of Education, and children of school staff. Each student is allocated a random number and within the admission process of each school, students are ordered and allocated according to this number. Therefore, even within the priority admission criteria, the probability of being allocated to the school is only determined by the lottery number (Ministry of Education, 2017).

The implementation of this centralised admission system started in 2016 in one region of the country and for specific school grades. It has been phased-in since, covering more regions and grades each year. Because schools are already at full capacity, currently, most places

allocated through this system are between PK-1st grade (5 to 7-year-olds) and 9th grade (15-year-olds). Therefore, it is still in an initial stage, and it will take some time to be implemented at the national level.

For the same reason, the empirical evidence and debate around this reform are also in early stages. There is evidence that this type of school admissions reform generates a public discussion that remains, at least partially, in the ideological sphere (Wang, Chie, & Chen, 2017) and the Chilean context is not an exemption. Mainly because the school system is heavily based on principles such as competition and school choice, some groups have seen this admission policy change as interfering with the "right to choose" of families, or with the "right to provide a particular educational project" of schools (Ramírez Fernández, 2017). However, the assessment of the admissions reform should consider the school system as a whole, particularly since preliminary studies have indicated that the admission reform has proven to be a cultural change for families (Carrasco, Oyarzún, Bonilla, Honey, & Díaz, 2019).

Moreover, a key point in favour of this public discussion is the availability of data regarding the school choice sets of families. Before the reform, each school was responsible for its admission process, so families had to apply individually to each school and, therefore, it was not possible to gather this information collectively. Researchers have recognized that this as a major benefit of the system reform, but they have also identified that these choice sets are still largely unchanged (Carrasco & Honey, 2019; Eyzaguirre et al., 2019).

Following this framework, the contribution of this research is focusing on a new school that allocated the entire student body through a lottery admission and evaluating its effect on student outcomes. Therefore, anticipating a possible scenario within this new school context.

4.2.3. The school

The case of this research is a private-subsidised primary and secondary school that opened in March[1] 2015 in an urban municipality. Its executive board is a non-profit educational foundation, and it is funded by governmental per-student subsidies and private donations. The school was conceived by an individual effort of a small group of people and the educational project was inspired both by the "high expectations" approach of KIPP schools (Clark, Gleason, Tuttle, & Silverberg, 2015) and an education centred on values (joy, excellence, fraternity, and passion) and soft skills (self-control, social intelligence, tenacity, energy). In addition, the school seems to be ahead of the national reform, presenting itself to the community as a school without co-payment or student selection even before the School Inclusion Act was passed.

[1] The school year in Chile runs from March to December.

The school initially offered grades from PK (5-year-olds) to 4th grade (10yo), and 9th grade (15yo). From its second year of operation, the school kept mainly two entry levels, PK and 9th grade, and progressively filled out its places until covering PK to 12th grade (18yo). From its opening, each level until 4th grade was composed of two classes of a maximum of 45 students, except for PK, which had a maximum of 35 students per class. The school's 9th grades consisted of three classes of a maximum of 40 students each. These class sizes are larger than the national average (n=26), though similar schools located in the same municipality had an average class size of 30 in 2015 (Ministry of Education, n.d.-b).

To try to understand why the school attracted so many families, even when it was a brand-new school, is it useful to characterise the educational context where it is inserted. For this purpose, *Table 4.1* compares the distribution of schools and students at the national and municipal levels for 2014, the year before the school opened. If the educational project proposed by the school was different from the actual school offer in the area, then that could have been attractive to families. Conversely, if at the time there were other similar projects in the area, then its popularity may reflect some dissatisfaction with the school offer; then, the fact that it was a new school may have been seen as an opportunity to try another educational option.

Table 4.1 National and municipal context one year before the school opened

	National	Municipality
N schools	7,546	76
Secular school	51%	47%
Free school	73%	52%
School average SES groups (4th grade)		
Low	32%	17%
Med Low	33%	24%
Med	21%	34%
Med High	9%	18%
High	6%	7%
Academic performance: reading (4th grade)		
Insufficient	36%	37%
Basic	30%	31%
Adequate	34%	32%
Academic performance: math (4th grade)		
Insufficient	44%	48%
Basic	37%	33%
Adequate	19%	20%

The year before the school opened, there were 76 primary and secondary schools in the municipality where the school is located. By 2015, one school closed, and this was the only new school that opened in the municipality that year. At the national level, more than half of primary

and secondary schools were public, while at the municipality level, 55% were private-subsidised schools. In terms of selective admission practices within schools in the municipality, roughly 20% asked for either a preschool evaluation or mark records from the previous school of the student, applied an entry test to students, or requested a parent's interview as part of the admission process. Moreover, this municipality had less secular and fee-free schools than the national average.

Additionally, the municipality where the school is located served relatively less deprived students compared to the national level according to the socioeconomic categorisation assigned to each school by the Education Quality Agency. However, the students' academic performance does not seem to differ much from the national average. In this sense, the school inserts itself in a context with much opportunities to improve the academic performance of students in the area.

In summary, compared to the national level, the school opened in a municipality with a vast school offer; however, half of these schools were similar to the school of this research in terms of being private-subsidised, secular, and fee-free. Therefore, this school seems to represent, to some extent, a different educational alternative, which may have provoked its oversubscription from its opening.

4.3. Data and sample

As discussed in previous literature (Foreman, Anderson, Ritter, & Wolf, 2017; Tuttle, Gleason, & Clark, 2012), perfect lotteries are uncommon and, therefore, dealing with perfect data is also rare. Before the national admissions reform, each school was in charge of its own process, so accessing these raw primary data was a unique opportunity provided by the school. This also means that while there is enough data to produce reliable estimates for this educational project, the data linkage with administrative records from the Ministry of Education and the Education Quality Agency faced several challenges to create the input for this research.

From the school's data for its first two years of operation (2015 and 2016), I obtained information about those students who applied. This included the national ID for each applicant, basic demographic and family contact information, and two questions about how the applicant's families knew the school and what they expected from it. Additionally, the Ministry of Education's research centre generates masked IDs to follow students across all their publicly available datasets anonymously. Following their confidentiality requirements for data linkage [2], I was able to get the masked IDs for all students who applied to the school and merge this list to

[2] These requirements prevented me from linking unmasked students with any other data.

several datasets covering their academic information and school trajectories. One of the strong points of the Chilean MoE's data is that it registers information for all students, regardless of the type of school they attend. Consequently, and unlike other studies that use public data, this expands the comparison group for this research, as I can follow students at any other school.

Table 4.2 presents the overall school sample, pooling the two cohorts included in the research (see Appendix section *10.1* for full details). Column (1) shows the total number of applicants per grade, excluding those with ID missing or duplicated in the school lists[3]. Column (2) indicates the total number of linked observations between the school lists and the MoE's data. The difference with column (1) derives from observations not found in the MoE's data, observations matched, and observations identified only in the Ministry's datasets[4]. Column (3) illustrates the number of students that attended the school and column (4) shows the number of students that applied but did not attend the school. These will be considered as the treatment and controls groups, respectively.

Table 4.2 School sample from applicants list to enrolment, per grade

Grade	Age (years)	Valid applicants (1)	Linked with MoE's data (2)	Total enrolled (3)	Total not enrolled (4)
PK	5	343	332	136	196
K	6	225	223	102	121
1st	7	250	251	100	151
2nd	8	205	208	97	111
3rd	9	193	186	97	89
4th	10	194	199	102	97
5th	11	71	65	3	62
9th	15	387	398	193	205
10th	16	0	0	0	0
Total		1,868	1,862	830	1,032

Since the school staff entirely managed the admission process, the data is not exempt from certain inconsistencies. First of all, I had access to the school's lists of applicants, but these files did not include the lottery outcome. Once these lists were linked to MoE's data, I was able to identify those applicants that finally enrolled in the school and those who did not. As

[3] Due to a school decision, no lottery was held for 10th grade in 2016. They did not receive applications for such level, and all students came from their previous grade.

[4] Observations may not be found in the official records of the MoE if a student enrols late or if the school does not provide their information on time. A small percentage of cases (4% overall the two cohorts) were identified as enrolled in the school based on the MoE's data. There is no way to know with complete certainty how many of these cases were indeed on the school lists but had an invalid ID, although this may also suggest that I was not provided with the final version of the school lists. I conducted a robustness check by adding a control variable for the source of each observation (the school list or the MoE's data). The results remain mostly unchanged in terms of its statistical and substantive interpretation. See section *10.3* in the Appendix for full details on these analyses.

explained in more detail in the Methods section, some assumptions had to be made to analyse the data, based mainly on the high demand that this brand-new school generated.

Second, the lists provided by the school identified families that submitted applications for more than one child but only for the 2015 cohort. If one of the siblings wins the lottery in the same admissions process, the other siblings would also be offered a place at the school. This has the potential to bias the results as some students would have entered the school by other means than their lottery chance; namely, by the chance of their sibling. However, because I do not have the lottery outcome, there is no information on which sibling (if any) won the lottery. Due to the inability to identify these cases and to avoid introducing additional noise in the data, I did not exclude these cases (around 30% of the 2015 cohort) in the primary analyses. Similarly, I identified five pairs of twins applying to four different grades among this group of siblings. Because these cases applied to the same grade, their probability of winning a place at the school was higher than that of non-twin applicants. Since the number of twins in the data is very small, I did not exclude these cases from the main analysis. Section *10.4* of the Appendix shows the robustness checks conducted for both cases by adding an indicator for siblings and twins. As in previous research (Hoxby & Murarka, 2009), these results remain consistent with the main analysis.

4.4. Methods

Because the application lists from the school do not include the lottery outcome, I am unable to assess compliance in the data directly. However, I will work under the assumption that there was full compliance at the time of the assignment and will estimate the intention-to-treat effects of being able to enrol in the school on academic outcomes. Although this may seem a strong assumption, there is no evidence to suggest that students had the chance to enrol in the school by other ways than the lottery assignment. Given the high oversubscription of the school, I could expect that applicants were sufficiently motivated to enrol if they were offered a place.

Under this initial assumption, I am also able to identify the proportion of enrolled students who stayed in the school and the proportion of not enrolled students who stayed in other schools over the years covered in this research. This measure of "compliance" is high (91% overall), so following previous research (Edmunds et al., 2017), I will not estimate treatment-on-the-treated effects as these would yield quantitatively similar results (Gerber & Green, 2012).

The analyses include four academic outcomes at the student level. First, the percentage of attendance within an academic year. Second, the student's average mark across subjects by the end of the academic year. These marks are measured as a continuous scale from 1 (lowest) to 7 (highest), increasing in decimal units. Third, an indicator of grade promotion (from now on,

passing). These three outcomes are available for all students from 1st grade (7yo students) onwards. Finally, I analyse the scores from the national standardised test for 4th graders (10yo students). As defined by the MoE, in each of these evaluations, the scores are set to have a mean of 250 and a standard deviation of 50 points (Education Quality Agency, n.d.).

To increase statistical precision, each analysis also controls for student- and school-level characteristics, such as gender, a socioeconomic vulnerability indicator created based on the MoE's classification[5] (Ministry of Education, 2008), the grade, attendance, final mark and progression status of the student the year before the opening of the school, and the rurality and type of school the student attended the year before the school started functioning.

I conducted a short-term analysis looking at outcomes in the first until the third year of the school's operation. When available[6], the analysis for year zero (the year before the school opened) is also shown to compare enrolled versus not enrolled students before they applied to the school. Equation 1 shows the general identification strategy to estimate the intention-to-treat effect on different academic outcomes at the student-level:

$$Y_s = \alpha + \beta T_s + \delta Student_s + \varphi School_c + \varepsilon_s \quad (1)$$

Where Y_s is the academic outcome of interest for student *s*; T_s is an indicator equal to one if student *s* enrolled in the school the first year he/she applied (and hence, is considered part of the lottery treatment group); $Student_s$ and $School_c$ are vectors of covariates in year zero at the student and school level; and ε_s is the error term. In each model, β represents the effect of being able to enrol in the school the year student s applied. In all models, standard errors are clustered at the school level.

As described in section *4.3*, the number of cases lost in the data management and linkage process is low. Moreover, without the lottery outcome, these dropped-out observations cannot be compared between enrolled and not enrolled applicants. There may, however, be a potential attrition bias related to the availability of data to conduct the analyses if the level of data missingness is high and, most importantly, if there is differential attrition between treatment and control groups. For this purpose, I conducted two analyses, which are fully presented in section *10.5* of the Appendix. First, I evaluated attrition levels considering the total number of observations in the sample, and second, I replicated the analysis considering the maximum number of observations with outcome data for each year. The difference between these two

[5] Assuming that the vulnerability condition of students would not change in the period of time of this research, I identified a student as vulnerable if in any year he/she was categorised as such by the MoE.
[6] The national standardised test evaluates specific grades yearly. However, since the school offered up to 4th grade its first year, there are no test scores data for the student sample the year before it opened.

approaches derives, for example, from students in PK and K, for which the outcomes are not measured. Lastly, I checked these attrition levels against the What Works Clearinghouse thresholds (WWC, 2017) to assess whether there is a potential risk of attrition bias in the data. The results indicate that there are moderate levels of attrition due to data availability, but the differences between enrolled and not enrolled applicants are consistently low. Thus, I could assume that attrition is not significantly related to the lottery assignment, which would allow yielding unbiased estimates (Gerber & Green, 2012).

Finally, using these lottery data has two additional implications. First, I exploit the fact that the school was highly oversubscribed, which makes it different from other schools (Stasz & Von Stolk, 2007). This could affect the external validity of the analyses; however, under the new admissions reform, it could be expected that other new oversubscribed schools may experience similar conditions. Second, one of the key issues when evaluating the effect on an educational project is selection bias (Abdulkadiroglu, Hu, & Pathak, 2013; Cullen et al., 2006). Precisely because the admission process was based on a random component, I can exploit this variation to account for selection bias by comparing all students who were interested enough in the school to apply.

4.5. Results

4.5.1. Who applied to the school?

Families applying to the school reported that they learned about its opening via two main channels: by local advertising through the press, leaflets, stores, television, and internet; and by word of mouth through relatives, friends, colleagues, other parents, and neighbours. Moreover, at the time of application, families also reported having expectations about the school in terms of providing *quality* education, mentioning academic excellence, discipline, integral education, and values as the main aspects they expected from the school. Therefore, these seem to be motivated families, concerned and active towards the education of their children.

Table 4.3 Comparison of applicants with municipality and regional aggregated levels

Variable	Applicants (1)	Municipality (2)	Region (3)
Male	56.81	51.36	51.48
Vulnerable	85.04	72.17	79.55
Previous academic year: attendance	92.97	92.12	92.67
Previous academic year: final mark	5.87	5.64	5.65
Previous academic year: passing	0.96	0.86	0.87
Previous school: public	27.21	36.37	50.37
Previous school: private-subsidised	70.44	49.27	42.05

Table 4.3 shows descriptive statistics comparing the profile of applicants with mean characteristics at the municipality and regional levels the year before the school opened. These show that male students, who could be classified as socioeconomically vulnerable, applied to the school in a higher proportion. However, although there seems to be no difference with the municipality and regional averages in terms of academic measures, students who applied to the school attended other private-subsidised schools the previous year substantively more.

Therefore, there is a degree of selection in the sample of applicants as they seem to differ in observable characteristics from other students in the municipality and region. More importantly, given these differences, I could not rule out that these applicants would also differ in terms of non-observable characteristics; for example, in the support they get from their motivated families during their school experience. Then, it is valuable to use the lottery data to estimate the school effects, as those who end up enrolled in the school and those who do not will, on average, be expected to be similar on these characteristics (Cullen et al., 2006).

4.5.2. Where did not enrolled students go?

To further understand the context of the school and the families that applied, *Table 4.4* shows a comparison between characteristics of the school and the groups of other schools attended by applicants who did not enrol in the first year of each cohort.

Table 4.4 Comparison between the school and the group of other schools that not enrolled applicants end up attending

	2015		2016	
Variable	The school (1)	Other schools (2)	The school (3)	Other schools (4)
Public school	No	0.40	No	0.32
Private-subsidised school	Yes	0.55	Yes	0.64
Private school	No	0.04	No	0.04
School in rural area	No	0.11	No	0.07
School in the same region	Yes	0.76	Yes	0.68
School in the same municipality	Yes	0.59	Yes	0.40
Secular school	Yes	0.58	Yes	0.53
Free school	Yes	0.45	Yes	0.56
Average class size	40.9	30.1	41.1	26.6
Total school enrolment	618	671.1	781	582.1
Pupil-teacher ratio	22.7	17.6	24.4	15.9
# schools	1	92	1	98

Notes: ** p<0.01, * p<0.05

More than half of the applicants who did not enrol in the school went to other private-subsidised schools in urban areas and stayed predominantly in the same region, although not

necessarily in the same municipality, while around half of them ended up in other secular, free schools. Additionally, applicants who finally did not enrol in the school decided to attend smaller, maybe more personalised schools. However, none of the differences in terms of the average class size, total school enrolment and pupil-teacher ratio is statistically significant. Therefore, these other schools where not enrolled students went do not seem to be particularly different, on average, to the school of this research.

4.5.3. Did the lottery work?

Because the admission process was implemented entirely by the school staff, a balance check was performed to evaluate its implementation (Chiapello, 2018; Gerber & Green, 2012). *Table 4.5* shows that differences between the group of applicants who enrolled and did not enrol in the school are small, yet these are statistically significant for two main variables. First, there is a higher proportion of male students within those who enrolled and, second, applicants who end up attending the school came disproportionately more from private-subsidised schools and significantly less from public schools.

Table 4.5 Balance check for analysis sample

Variable	Not enrolled (1)	Enrolled (2)	Difference (3)	n (4)
Male	0.50	0.55	0.05*	1,862
	(0.50)	(0.49)	(0.02)	
Vulnerable	0.83	0.85	0.02	1,862
	(0.38)	(0.36)	(0.02)	
Previous school: public	0.29	0.24	-0.05*	1,573
	(0.46)	(0.43)	(0.02)	
Previous school: private-subsidised	0.68	0.74	0.06**	1,573
	(0.47)	(0.44)	(0.02)	
Previous school: in rural area	0.05	0.07	0.02	1,573
	(0.22)	(0.26)	(0.01)	
Previous academic year: attendance	92.89	93.18	0.29	1,048
	(7.09)	(6.20)	(0.42)	
Previous academic year: final mark	5.90	5.85	-0.05	1,048
	(0.61)	(0.60)	(0.04)	
Previous academic year: passed	0.97	0.97	0.00	1,054
	(0.17)	(0.16)	(0.01)	
Total observations	1,032	830	1,862	

Notes: Standard deviations are shown in parentheses in columns (1) and (2), and standard errors are shown in parentheses in column (3). ** p<0.01, * p<0.05

On one hand, the school admission process was based on a manual lottery machine with a different ball (number) for each applicant. Hence, while it is a co-educational school, they did not actively enforce equal gender assignment by using, for example, quotas. On the other hand,

the school does not have much control over its pool of applicants. As shown in section *4.2.3*, most schools in this area are private-subsidised, so this imbalance by school type, while substantively minor, is also not surprising. More importantly, there is no anecdotal evidence to suspect manipulation of the lottery assignment by the school.

Since the admission process was random, any differences identified could be due to chance (Hayes & Moulton, 2009), therefore, it could be understood that these differences arise mainly, and randomly, from the implementation of the lottery process. In order to yield unbiased estimates, the primary analyses control for these student and school characteristics.

4.5.4. The school effect on students' outcomes

The following figures illustrate the main analysis estimating the intention-to-treat effects of being able to enrol in the school for the first and subsequent years the school has been in operation. As an additional visual assessment, the analysis for Year 0 shows that, for all outcomes, there are no differences between enrolled and not enrolled students the year before they applied to the school. This confirms the analysis presented in section *4.5.3* and suggests that any effects in subsequent years would be associated with being able to enrol in the school due to the lottery admission process. Full results are included in Appendix section *10.2*, where models with and without covariates are presented.

Figure 4.1 shows the school effect on the percentage of annual attendance, which is slightly but significantly lower for students who enrolled in the school in the first two years, after which the difference becomes negligible. The first year of the school, students who enrolled attended 2 percentage points less annually compared to not enrolled students, which corresponds to four school days according to the Ministry's minimum calendar (2010).

The effect on the final average mark is presented in *Figure 4.2*, showing that in the school's first year, students who enrolled obtained half a point less in the final mark than students who did not enrol in the school. This would suggest that students in the school achieved 8% less of the full range of the marks' measure. However, in the following years, this significant difference seems to disappear. Similarly, when looking at the national test scores for 4th grades (10yo students), *Figure 4.3* shows that enrolled students do slightly worse in both reading and math. Moreover, this negative effect is statistically and substantively significant for reading in the second year, which is equivalent to almost a third of a standard deviation. The results for math are small, corresponding to a tenth of a standard deviation, and non-significant.

Finally, *Figure 4.4* illustrates the school effect on the grade promotion indicator. Similar to the effect on attendance, students who enrolled in the school were significantly less likely to pass in the first years compared to students who did not enrol, although by the third year, this effect becomes non-significant. As shown in *Figure 4.5* and *Figure 4.6*, this result is driven

primarily by the 9th grade cohort (15yo), where students were 12 percentage points less likely to pass, a much larger effect than in lower grades for which data is available. Yet, the difference in the promotion rate for this cohort becomes equivalent to zero in the following years.

Figure 4.1 Intention-to-treat effects on attendance by year

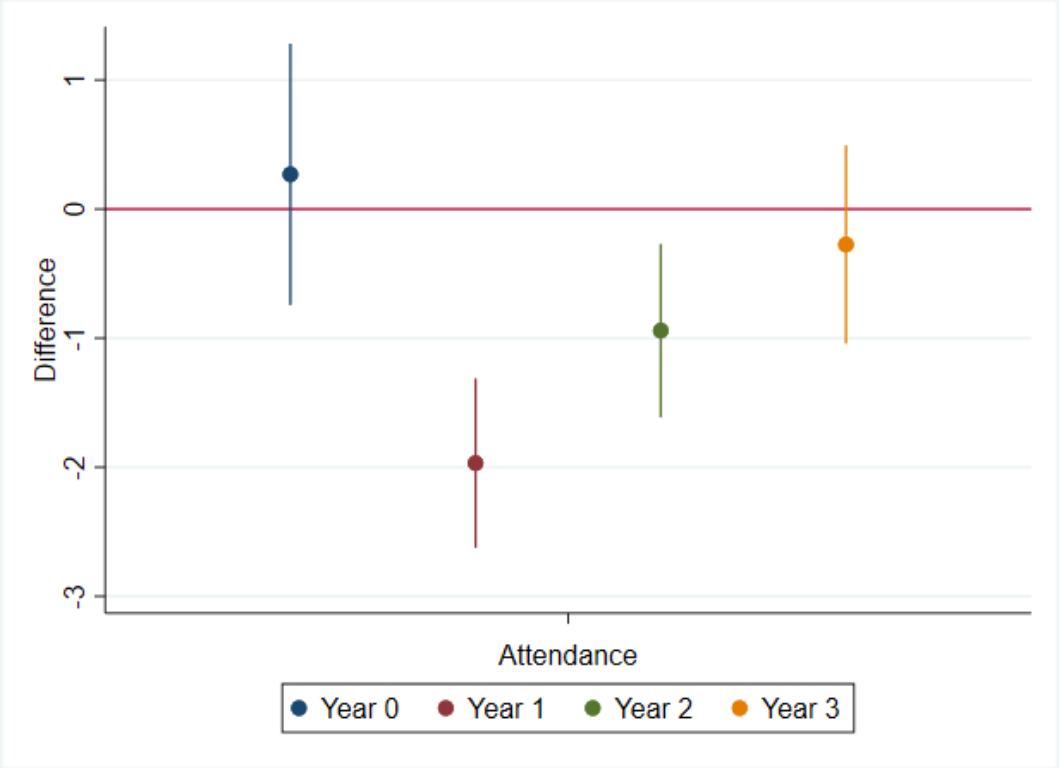


Figure 4.2 Intention-to-treat effects on final mark by year

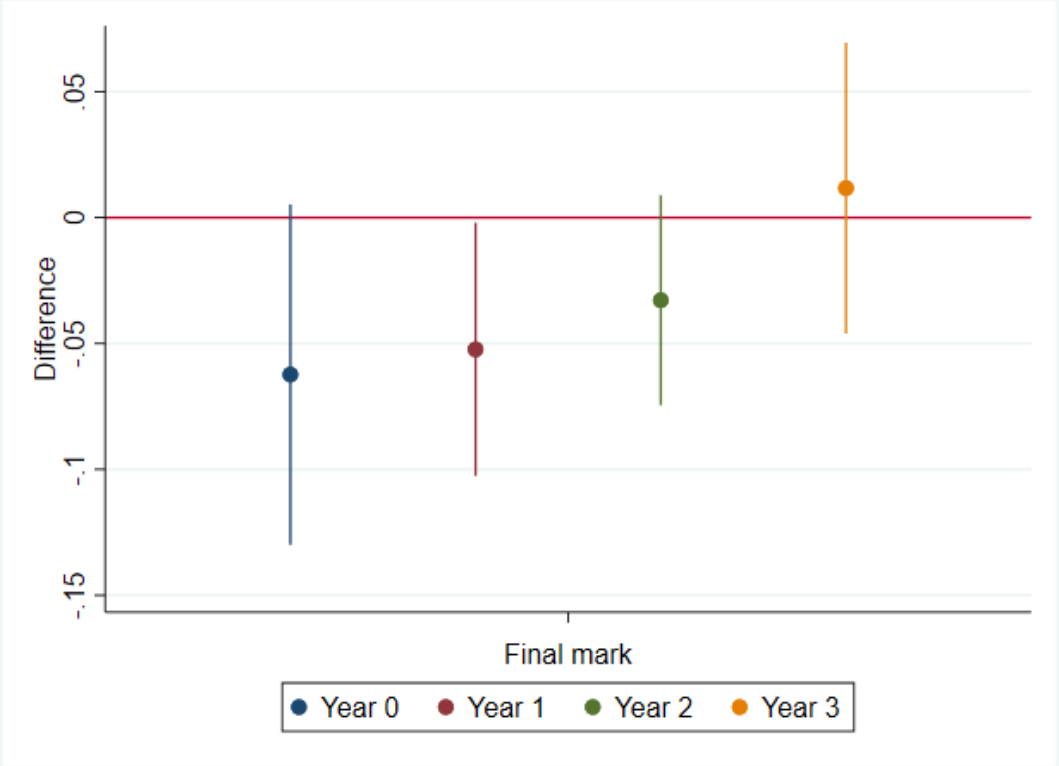


Figure 4.3 Intention-to-treat effects on test scores for 4th grades by year

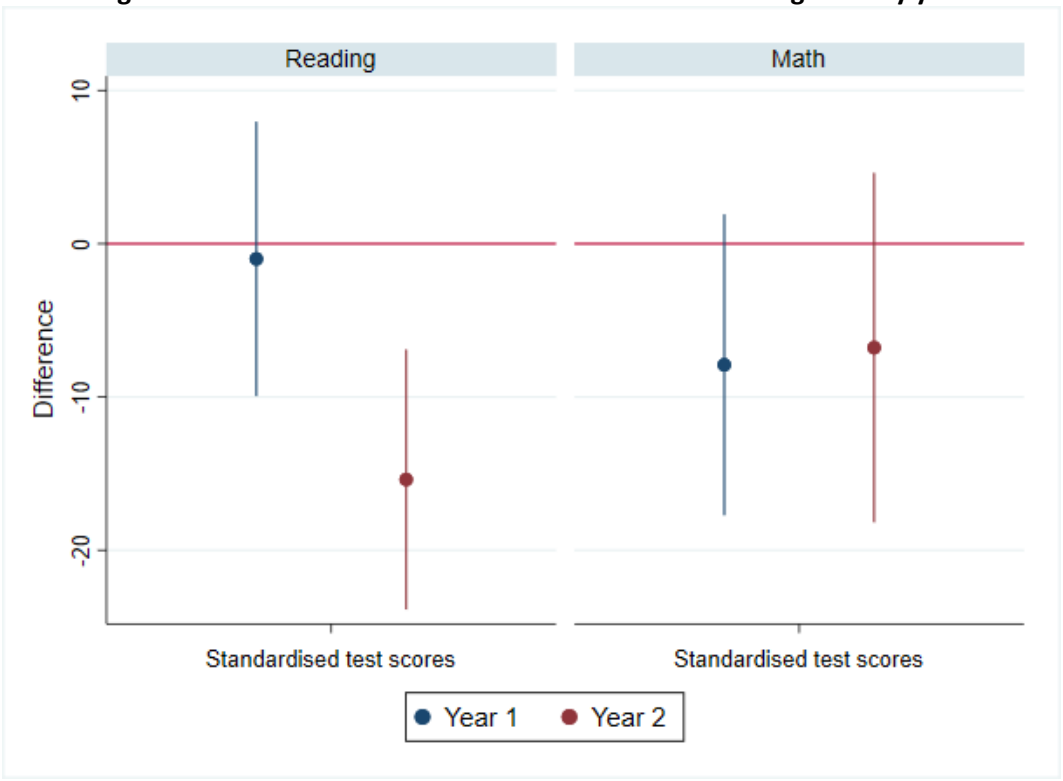


Figure 4.4 Intention-to-treat effects on passing by year

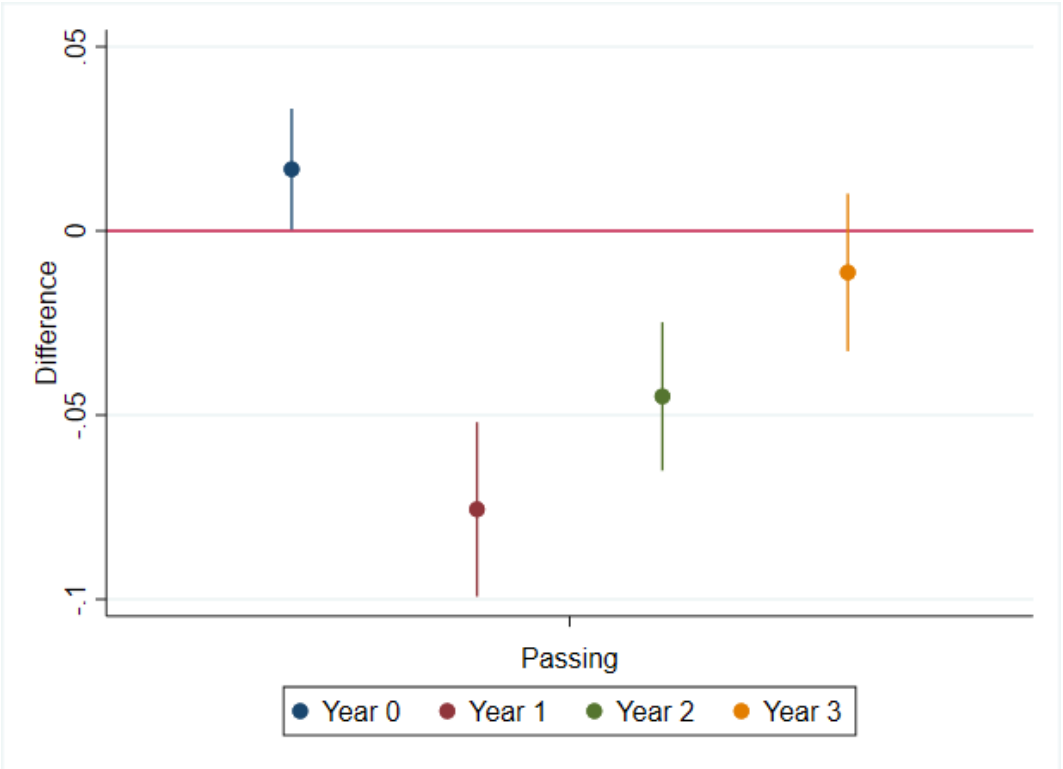


Figure 4.5 Intention-to-treat effects on passing by grade in Year 1

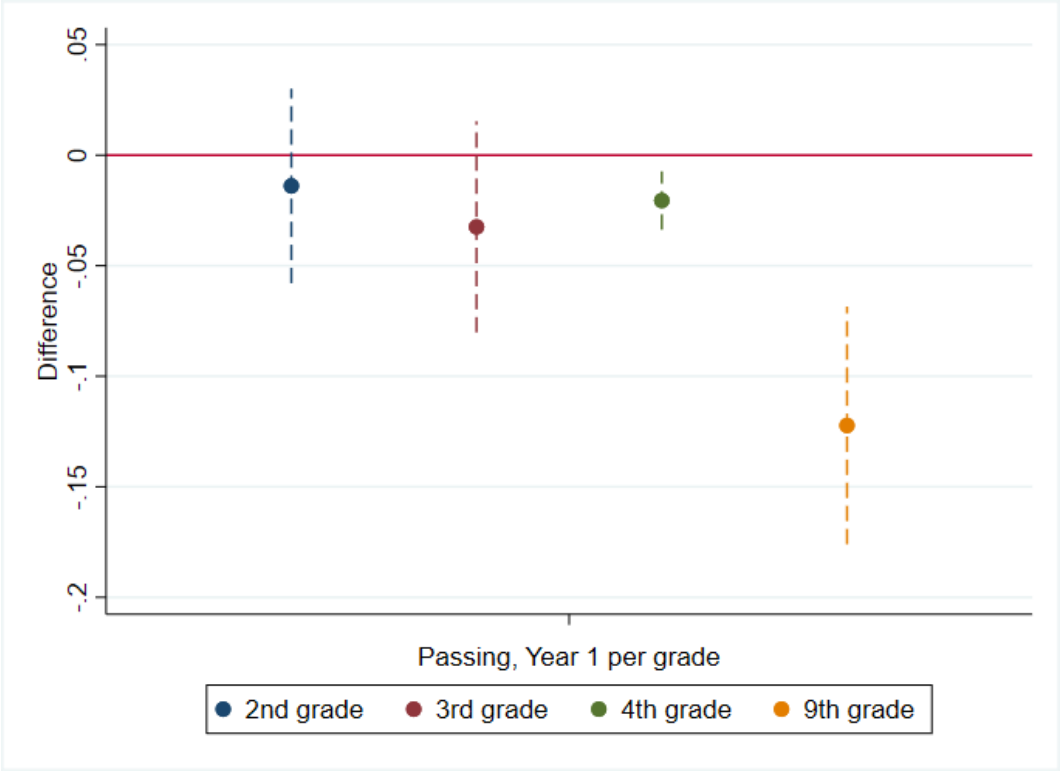
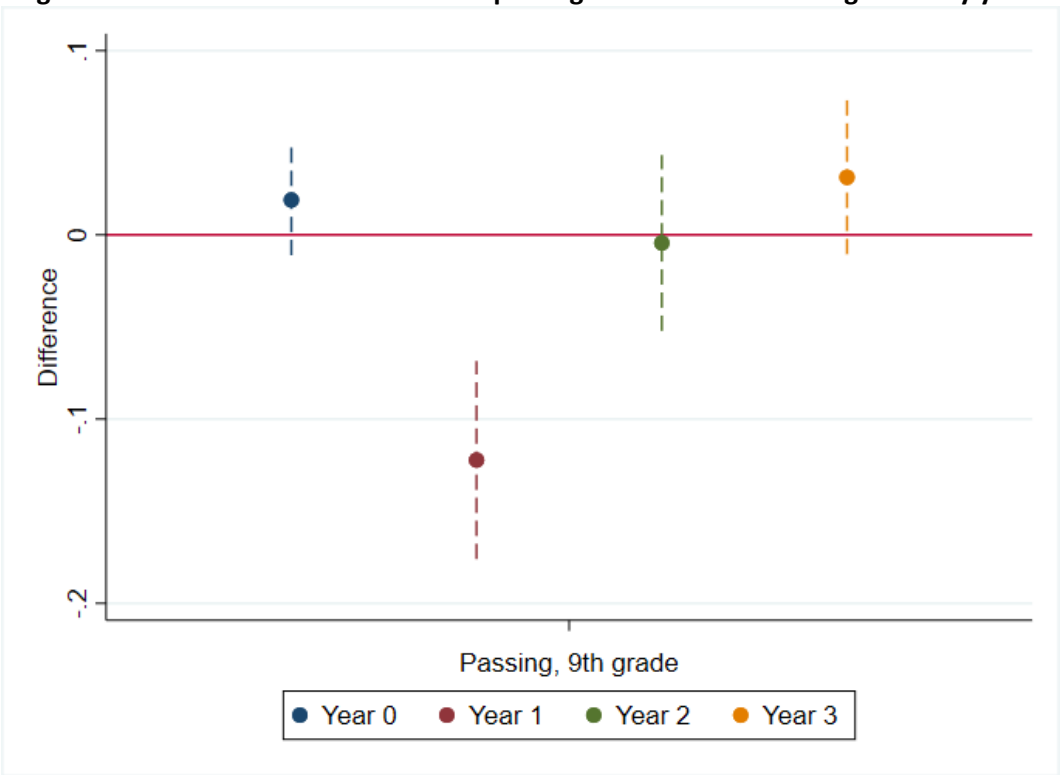


Figure 4.6 Intention-to-treat effects on passing for first cohort of 9th graders by year



4.5.5. Potential mechanisms

As in previous research (for example, Dynarski, Hubbard, Jacob, & Robles, 2018; Knechtel, Coen, Caronongan, Fung, & Goble, 2017) and based on administrative data, I hypothesise about potential channels to help understand and contextualise these results. For this purpose, I use indicators of social and personal development and categories of school performance collected by the Chilean Education Quality Agency, as they provide an overview of the school's quality as measured by the Ministry of Education.

The personal and social development indicators (PSDI) are non-academic student development measures to provide a more comprehensive understanding of school quality (Ministry of Education, 2016). The PSDI include four areas: academic self-esteem and motivation; school climate as a respectful, organized, and safe environment; citizenship as civic participation and sense of belonging; and healthy lifestyle as eating, active life, and self-care habits. These are measured through student questionnaires, which then generate an overall school-level indicator by area ranging from 0 to 100, where a higher value means a better development in that area.

Figure 4.7 Personal and social development Indicators, per grade

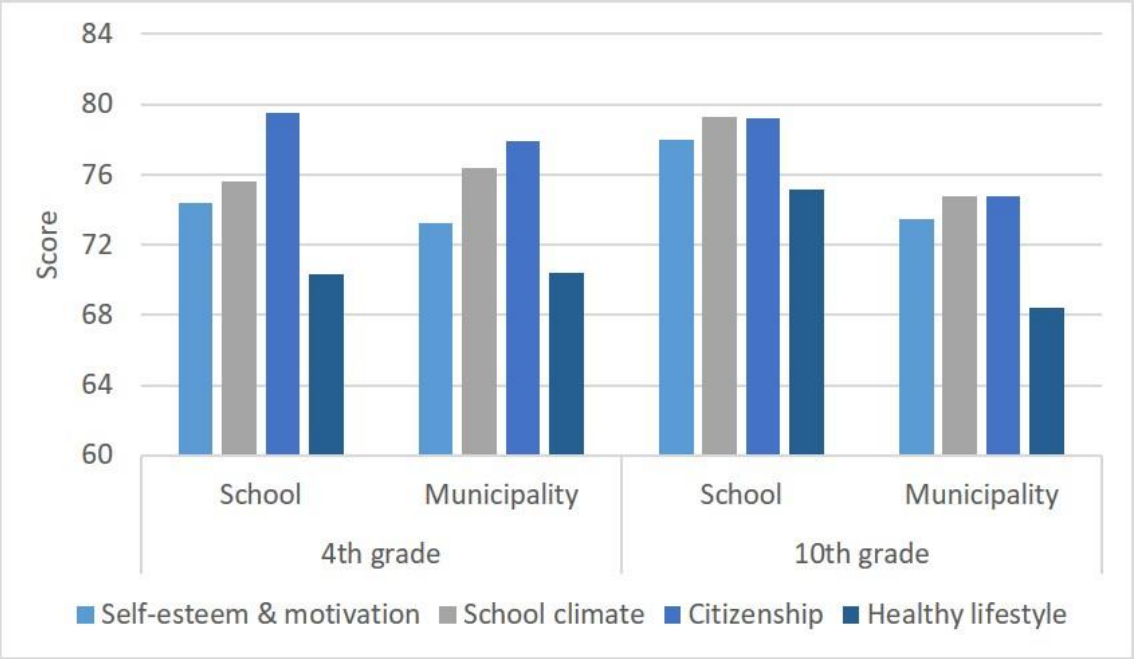


Figure 4.7 shows the average PSDI for 4th and 10th grades (10yo and 16yo, respectively) across all available data from 2015 to 2017. It compares the indicators for the school with the average for all private-subsidised schools in the municipality. The indicators for the school according to 4th graders seem to be at least as high as for the other schools in the same location, whereas 10th graders evaluate the school much better in all four areas compared to other similar schools in the municipality.

In addition, since 2016, the Education Quality Agency evaluates schools by triangulating data on their academic performance measures, their personal and social development indicators, and their teaching context based on the socioeconomic profile of their students. With such information, one of the four *performance categories* is assigned to each school: high, medium, medium-low and insufficient (Ministry of Education, n.d.-a).

The school has been rated as having a medium-low performance in 2017 and 2018[7], which indicates that, given the background of the students, their results are below expectations. In practice, this means that the school has been receiving extra support from the Ministry of Education in order to improve this outcome.

In summary, these data suggest that although the school's academic results are not as high as might be expected, its students would still be satisfied with the work of the school.

4.6. Discussion

In the context of the school admissions reform in Chile, this paper studies potential implications for this new system through a unique school case and looking at how it positions itself in the community, how families behave towards this school opening, and how the school affects the students' outcomes. Using exclusive data provided by a private-subsidised school, which is publicly funded and run by an educational foundation, I am able to trace applicants from the school's first two years of operation. Moreover, by exploiting the school's lottery data and the fact that it was oversubscribed, I estimate intention-to-treat effects of being able to enrol in the school on student academic outcomes.

Results show that there is self-selection in the students and families that applied to the school, as families appear to be very motivated and students not only come disproportionately more from other private-subsidised schools but also turn to this type of schools when they are not able to enrol. Additionally, although the majority of schools in the municipality are private-subsidised, families react to this school's opening by submitting, from the first year, more applications than the places it had available.

The school effects on attendance, final mark, test scores, and grade promotion seem to be worse for students who enrol in the school compared to students who did not. However, as the school increases its years of operation, these differences become statistically indistinguishable. This is not only a common feature of new schools implementing their educational project (Fullan, 2001), but evidence also indicates that this is common in U.S. charter schools, the equivalent to private-subsidised in Chile (Zimmer et al., 2009). Therefore, these

[7] Each yearly report of performance categories is based on data until the previous academic year.

academic effects based on the first years of the school could serve as lower bounds of its full potential.

Moreover, while these results are in line with the medium-low quality categorisation assigned to the school by the Ministry of Education, students seem to have a positive evaluation of the school in terms of non-academic indicators. This suggests that the school may be more effective in developing the integral education component, based on values and soft skills, of its educational project rather than the high academic expectations element.

Nevertheless, these findings may also reflect other educational issues. On one hand, there may have been a lack of preparation from the school and its teaching staff. If lottery admissions provide equal access to education, then it could be expected that enrolled students would have a range of social and economic backgrounds. This has a direct implication for the new admission system in Chile, as attention should be put on how to support schools and teachers to care successfully for all students, especially for schools that have historically received a particular profile of students. This way, the system regulation would provide equality of access and each school would be more able to provide equality of learning opportunities.

On the other hand, the rebound seen in the effects as the school is established over time may also indicate a strict adherence to the high academic expectations for students. In 9th grade, the level of secondary education offered by the school in its first year, 40% of students did not progress appropriately and either left the school or were retained at the school and had to repeat the grade. This is a relevant issue in education policy, as grade retention would be associated with school dropout (Di Piero, 2014; Josephson, Francis, & Jayaram, 2018), which ultimately means a detrimental effect for these students.

There are certain limitations in this research, of which the most prominent is the lack of lottery outcome data due mainly to the school not collecting these data for research purposes. Nonetheless, one of the advantages of the new admissions system is that the risk of having low-quality data would be minimal as now the collection of information and its management is a standardised process for all families and schools. Second, the school data shows some pre-treatment imbalance, yet because of the nature of the random process, this could be due to chance. Moreover, following an early public discussion of potential issues of the new admissions system (Eyzaguirre, 2016), it now takes care of some of the issues identified in this paper. Namely, gender distribution in co-educational schools is now controlled for by the algorithm used in the admissions system. Third, the analysis is based on the sample of students who applied to one school, which restricted the range of analyses that could have been carried out. For example, looking at further heterogeneous treatment effects or working with standardised test scores at other grades would be potentially underpowered. Moreover, with regards to the external validity of the study, the fact that this school was oversubscribed from the beginning

makes it unique. While these results would be less applicable to undersubscribed new schools, these will still be under the same admission application process and potentially diverse student body.

Despite the "broken data" to which I had access, the results are consistent against several robustness checks related to data sources, siblings, and attrition, suggesting that the conclusions presented here are reliable and in line with causal intention-to-treat effects. Moreover, this is a good exercise looking forward to the new admissions system and thinking about key issues that could be addressed with the new data.

The admissions reform in Chile is changing the scenario not only in terms of school regulation but also regarding the data that will be available for research. The transfer of the admission process to the Ministry of Education will provide details that could not have been collected before the reform, which could inform the decision-making regarding this policy. For example, these new data will allow exploring the socioeconomic changes of the schools' intake, or how families change their school choice behaviour after this policy change. As previous literature has discussed (Wang et al., 2017), it is essential to study and incorporate these behavioural adaptations to, first, assess whether the admissions reform is achieving its objective of providing equal access opportunities, and second, identify if there are areas that need yet to be addressed towards this goal.

Finally, since there is no data available yet from the new admissions system for higher grades, under the same rationale of this study, further research could follow this cohort of students through the end of high school and into higher education to assess medium-to-long term school effects.

5. General conclusions

5.1. Summary and synthesis of the findings

This thesis focuses on school admission policies aiming to address its two main questions. First, studying the extent to which different school admission practices are able to foster or obstruct equal access opportunities. Second, examining the extent to which providing equal opportunities in the access to education can also generate equal learning opportunities for students. To address these overarching questions, this thesis offers three empirical studies related to the evidence on school lottery admissions in the international context, and to the evaluation of different admission policies in the Chilean school context before the implementation of its ongoing admissions reform.

The first study, which is co-authored with Gabriel Gutierrez and Alison O'Mara-Eves, presents a review of the evidence on lottery school admissions and synthesises their impact on student achievement. After conducting a systematic search of literature, the range of outcomes evaluated in the primary studies is discussed in the form of a narrative synthesis, including outcomes at the school level (for example, school climate or socioeconomic composition), non-educational outcomes (for example, socioemotional or health measures), other outcomes related to the educational process (for example, high school graduation or educational expectations), and academic performance as the more traditional educational outcome.

Moreover, the results of the meta-analysis on academic performance show small positive effects on both math and reading outcomes, and under the two analysis strategies considered: intention-to-treat and treatment-on-the-treated. The quantitative synthesis also shows high levels of heterogeneity, indicating that the primary studies have differences that go beyond the scope of the review. However, the main effects are consistently positive against a series of robustness and sensitivity analyses. Likewise, the evidence of the review is deemed reliable due to its overall low risks of bias.

The second research chapter explores how using student selection practices in the admissions, specifically entry tests and parent interviews, may have an effect on the students' academic performance and the socioeconomic composition of schools. By identifying different categories of use of these admission mechanisms over the research panel years, I exploit changes in this use to estimate the effect of selective admissions.

Although, on average, selective schools would not seem to show a substantial academic benefit from selecting students, there are some interesting results by school type and grade.

Private and private-subsidised schools that consistently select students seem to be driving the academic effects, while private schools would seem to account for the positive effects on the socioeconomic composition in 4th grades (10-year-olds).

In turn, the few public schools that always select students in 8th grade (14-year-olds) show considerable academic gains and seem to attract similar students from higher socioeconomic status. This finding would be in line with previous research from the Chilean context indicating how selective public secondary schools show higher socioeconomic compositions (Allende & Valenzuela, 2016).

The third research chapter informs the ongoing national admissions reform, which incorporates a random component as a tie-breaker for oversubscribed schools. It does so by studying the case of a new private-subsidised school that used lottery admissions (before the reform) to fill-out its student body. By exploiting its lottery data and the fact that the school was oversubscribed, I estimate the intention-to-treat effect of being able to enrol in the school on different student academic outcomes: attendance rate, final average mark at the end of the school year, standardised test scores, and grade promotion.

Descriptive results show that the families that applied to this new school are different from other families in the municipality, as applicants are disproportionately more male, vulnerable and from other private-subsidised schools than the average municipality and regional levels. Moreover, although the opening of the school attracted many families, students that were able to enrol in the school show lower academic outcomes than students who applied but were not able to enrol. These differences, however, seem to disappear by the third year of the school's functioning. These results would be consistent with previous evidence on a similar type of schools on the U.S. (Zimmer et al., 2009) and would also be in line with the overall quality category assigned to the school by the Chilean Ministry of Education.

As a synthesis of these results, this thesis provides evidence that in the context before the admissions reform in Chile, there seemed to be a culture for schools to select their students, through different mechanisms and to different extents, which the evidence suggests helped enable the high levels of academic and socioeconomic segregation in schools. In the odder case where an oversubscribed school was non-selective, admitting its students through random allocation, the initial disadvantage in their academic performance ultimately disappears over the years. This seems to disagree with the international evidence on educational programmes using random admissions, which shows that at the very least, schools that use lotteries do not harm students in terms of their academic outcomes. The overall connection between these studies seems to be directed to the idea that school admission policies have the power to influence educational segregation but, on its own, they may not be a sufficient condition to provide educational opportunities for all students. The implications of these findings concerning

the Chilean admissions reform and the field of school admissions more broadly are discussed in the next section.

These studies are not exempt from limitations, which are mainly due to issues with the data used in the analyses. For example, potential measurement error issues or underpowered analyses, the lack of availability for certain data, and moderate issues with the external validity of the results are common threats to the studies presented in this thesis. However, the range of robustness checks and sensitivity analyses conducted in all three pieces of research show that, overall, these results are consistent.

5.2. Implications of the findings

The underlying heterogeneity of the results of the random admissions review remains unresolved in the thesis but, on its own, it is still an interesting finding. All variables under which heterogeneity was examined are related to characteristics of how the study was carried out (for example, the methods used for analysing the data or the school sample size), or to features related to the school or admission policy of the study (for example, whether it was a policy at the educational system or the school level, or the type of school assessed). Then, the robustness checks and sensitivity analyses performed were based on more contextual variables rather than on characteristics of the specific educational programmes of these studies. Exploring these programme-level differences could open new research possibilities lo learn more about this admission policy. This task would imply looking into more detail what schools and teachers do within each of these education programmes.

In the context of the overall evidence of this thesis, this finding allows making a distinction between providing equal opportunities to access education and providing equal learning opportunities to all students. The former would be related to the admission policies that are in place in a school system, whereas the latter would be associated with the educational practice performed by these schools. More importantly, as evidenced in the chapter on the school lottery case, guaranteeing equal access does not automatically translate into better learning opportunities for all students. As such, research should help identify the best admission policies to provide equal access to education but also the best practices to provide equal learning opportunities that would benefit all students.

This is one of the major implications that this thesis may have for the ongoing reform in Chile. Because this new policy would open the access to education, the educational value of schools will no longer be conditioned to its intake. Schools will face a transition with the implementation of this new policy, and should be supported to connect this system policy with

appropriate teaching and learning practices, particularly those which had a pattern of student selection.

Moreover, the evidence and implications of this research are also important because school admission policies are becoming an increasing topic of discussion in the international context. For example, in the UK, school admissions are currently being discussed and studied in terms of their ability to provide fairer processes and to promote social mobility. The Sutton Trust has published relevant research (Burgess, Greaves, & Vignoles, 2020; Cullinane, 2020) as part of its current national push for making school admissions fairer. This research emphasises the prevalence of socio-economic segregation in schools and the perception from school staff and parents that this is a problem. To balance the schools' intakes in order to promote social cohesion and help even out teacher quality among schools, the authors offer random ballots as one of the alternative admission policies for oversubscribed schools, along with banding tests, prioritisation of disadvantaged students, and relaxing the religious observance requirement in faith schools. Moreover, they argue that parents seem to perceive positively the use of random allocation in school admissions, compared to the current residential rule that is most commonly used in schools in England. In the same line, when discussing ways to promote social mobility in Britain, Major and Machin (2019) propose four principles under which substantial reforms should be advanced, one of which is related to providing fairer educational opportunities. The authors also endorse the use of random school admissions as an unbiased way to allocate students who are in equal conditions.

In terms of studying this topic further, assessing school admissions by (only) measuring academic outcomes would not provide essential information on their original purpose. As discussed in previous research (Stasz & Von Stolk, 2007), school admissions that explicitly aim to provide equal access opportunities to education need also to be evaluated according to this objective. The evidence from this thesis relies more on academic outcomes; however, attention should also be put into how admission policy changes affect individual school's intakes and the overall segregation in the education system. Similarly, the international empirical evidence on school lottery admissions could be complemented with a revision of substantive literature around the purpose or public response to the implementation of such admission policies.

An additional way to advance the study of school admissions would be by combining the school panel used in this research with new data provided by the centralised Chilean system. This would allow assessing the changes the schools will face when not being able to select students. For example, after identifying patterns of selection in schools, such typification (e.g. always selective) could be used to explore how schools adapt to the new admissions system, and the effects, if any, it could have on student- and school-level outcomes.

Likewise, exploiting the school choice sets of Chilean families would provide new insight into the relationship between school admissions and equality of educational opportunities in this context. Making evidence-based policy is important, and these new data could inform in a timely manner issues in the implementation and the effects and implications of the admissions reform at the country level. In addition, this evidence could also be useful for the international research community as the Chilean context is a compelling national-level school choice case. For example, given that the Chilean school system does not consider residential criteria in their admissions, the relevance of having catchment areas could be explored and contrasted with other school choice systems with such criteria.

Lastly, the data of the school lottery case could be extended to explore longer-term outcomes, such as high school graduation and university enrolment. This evidence could provide further insight into the potential outcomes and the challenges for schools and students of promoting equal access opportunities.

5.3. Final reflections

As part of this PhD process, my main takeaway is that when doing research there is a balance to be achieved between being flexible and adapt to changes that will inevitably come, and staying focused on the original research questions. There are so many possibilities to get distracted in the way that it is challenging not to take in more than what you can or what you should. Thus, I have had to learn how to make decisions related to my research projects based on being reflective and critical, but also practical.

An example of this is related to the research chapter on the school lottery case. I invested a good deal of effort with the school to convince them to let me use their lottery data, and then in accessing and making sense of it (because, of course, this files had not been created for research purposes). So realising that the data was not perfect and that it would not allow me to use the most advanced analysis methods was a turning point. After being close to changing the chapter completely, I ultimately recognised that the data still had value not only as an academic exercise but also as a contribution to the public discussion and to the school itself.

Additionally, I started this research project aiming to explore how school admissions affected equality within school systems. However, now I understand the need to differentiate equality of access from equality of learning opportunities, which is a distinction that I did not start with. This is interesting because, due to my inclination to quantitative research, I had paid more attention to the structural aspects of the school system, I had put more focus on the system policy than on the in-class practice. Then, this thesis process made me realise that school system policies are not necessarily a sufficient condition to provide more and better

opportunities for all students. As hinted in the findings of these research chapters, even under the same admission policy, what happens inside the school and classroom matters. In turn, this allowed me to reflect and challenge my initial assumptions about this topic and open some perspective on the power and limitations of quantitative research in terms of providing evidence to inform education policy.

Finally, and related to the above, my main positioning issue with this research relates to how I value social justice and equality of opportunities. In Chile, it is not uncommon to hear speeches of meritocracy, which in the context of education argue that motivated parents would have the "right" to select, and being selected, by a school of their choice. This new admission system, which tie-breaks oversubscription through a random component, has had to face the rejection from parents who deem it as unfair because it leaves to chance a decision that before they were able to buy (through paying to enter the admission process of a highly solicited school, through paying a fee co-payment, etc.). My motivation with this research was to generate evidence to be able to put forward the random allocation of students as a feasible and fair admission policy because I thought that school lotteries were the epitome of fairness and equality of opportunities. Ultimately, this research has nuanced this perspective and has allowed me to take some distance from this positioning in order to understand the relevance of this research as a whole.

6. References

Abdulkadiroglu, A., Hu, W., & Pathak, P. A. (2013). *Small High Schools and Student Achievement: Lottery-Based Evidence from New York City* (Working paper No. w19576). Cambridge, MA: National Bureau of Economic Research. Retrieved from http://www.nber.org/papers/w19576

Allen, R., Burgess, S., & McKenna, L. (2013). The short-run impact of using lotteries for school admissions: early results from Brighton and Hove's reforms. *Transactions of the Institute of British Geographers*, *38*(1), 149–166. https://doi.org/10.1111/j.1475-5661.2012.00511.x

Allende, C., & Valenzuela, J. P. (2016). *Efectividad de los Liceos Públicos de Excelencia en Chile* (Documento de Trabajo n° 20). CIAE, Universidad de Chile. Retrieved from https://www.researchgate.net/publication/311518805_Efectividad_de_los_Liceos_Publicos_de_Excelencia_en_Chile/citation/download

Angrist, J. D., & Pischke, J.-S. (2009). *Mostly harmless econometrics: an empiricist's companion*. Princeton: Princeton University Press. Retrieved from http://www.dawsonera.com/depp/reader/protected/external/AbstractView/S9781400829828

Balsa, A. I., & Cid, A. (2016). A Randomized Impact Evaluation of a Tuition-Free Private School Targeting Low Income Students in Uruguay. *Journal of Applied Economics*, *19*(1), 65–94. https://doi.org/10.1016/S1514-0326(16)30003-4

Barrera-Osorio, F., & Patrinos, H. (2009). An international Perspectives on school vouchers. In *Handbook of Research on School Choice* (Berends, M., Springer, M. G., Ballou, D., Walberg, H. J. (eds), pp. 339–358). Routledge. Retrieved from https://www.routledgehandbooks.com/doi/10.4324/9780203881781-20

Belfield, C., & Levin, H. M. (2005). Vouchers and Public Policy: When Ideology Trumps Evidence. *American Journal of Education*, *111*(4), 548–567. https://doi.org/10.1086/431183

Bellei, C. (2013). El estudio de la segregación socioeconómica y académica de la educación chilena. *Estudios Pedagógicos (Valdivia)*, *39*(1), 325–345. https://doi.org/10.4067/S0718-07052013000100019

Betts, J. R. (2005). The Economic Theory of School Choice. In *Getting Choice Right* (pp. 14–39). Brookings Institution Press. Retrieved from http://www.jstor.org/stable/10.7864/j.ctt12879sq.4

Betts, J. R., & Tang, Y. E. (2011). *The Effect of Charter Schools on Student Achievement: A Meta-Analysis of the Literature*. Center on Reinventing Public Education. Retrieved from https://eric.ed.gov/?id=ED526353

Betts, J. R., & Tang, Y. E. (2016). *A Meta-Analysis of the Literature on the Effect of Charter Schools on Student Achievement*. Society for Research on Educational Effectiveness. Retrieved from https://eric.ed.gov/?id=ED566972

Blaug, M. (1980). *Education vouchers : A new method of financing education*. University of London, Institute of Education.

Boeskens, L. (2016). *Regulating Publicly Funded Private Schools: A Literature Review on Equity and Effectiveness* (OECD Education Working Papers No. 147). Retrieved from https://doi.org/10.1787/5jln6jcg80r4-en

Borenstein, M., Hedges, L. V., Higgins, J. P. T., & Rothstein, H. R. (2009). *Introduction to meta-analysis*. John Wiley & Sons. Retrieved from https://doi.org/10.1002/9780470743386

Borman, G. D., & Grigg, J. (2009). Visual and narrative interpretation. In *The handbook of research synthesis and meta-analysis* (2nd ed, pp. 497–519). New York: Russell Sage Foundation.

Bos, M. S., Ganimian, A., & Vegas, E. (2014). *Chile en PISA 2012: Logros y desafíos pendientes* (América Latina en PISA 2012). Inter-American Development Bank. Retrieved from https://publications.iadb.org/es/publicacion/13784/america-latina-en-pisa-2012-chile-en-pisa-2012-logros-y-desafios-pendientes

Burgess, S., & Briggs, A. (2010). School assignment, school choice and social mobility. *Economics of Education Review*, *29*(4), 639–649. https://doi.org/10.1016/j.econedurev.2009.10.011

Burgess, S., Greaves, E., & Vignoles, A. (2020). *School places: A Fairer Choice? School choice, inequality and options for reform of school admissions in England*. The Sutton Trust. Retrieved from https://www.suttontrust.com/wp-content/uploads/2020/02/School-Places.pdf

Carrasco, A., Gutierrez, G., & Flores, C. (2017). Failed regulations and school composition: selective admission practices in Chilean primary schools. *Journal of Education Policy*, *32*(5), 642–672.

Carrasco, A., & Honey, N. (2019). *Nuevo Sistema de Admisión Escolar y su capacidad de atenuar la desigualdad de acceso a colegios de calidad: al inicio de un camino largo* (Estudios en Justicia Educacional, N° 1). Santiago, Chile: Centro Justicia Educacional. Retrieved from http://centrojusticiaeducacional.cl/wp-content/ uploads/2019/03/estudios-n1.pdf

Carrasco, A., Oyarzún, J. de D., Bonilla, A., Honey, N., & Díaz, B. (2019). *La experiencia de las familias con el nuevo sistema de admisión escolar: un cambio cultural en marcha* (Estudios en Justicia Educacional, N° 2). Santiago, Chile: Centro Justicia Educacional. Retrieved from http://centrojusticiaeducacional.cl/wp-content/uploads/2019/08/ estudios-n2.pdf

Chabrier, J., Cohodes, S., & Oreopoulos, P. (2016). What Can We Learn from Charter School Lotteries? *Journal of Economic Perspectives*, *30*(3), 57–84. https://doi.org/10.1257/jep.30.3.57

Chandler, J., McKenzie, J., Boutron, I., & Welch, V. (2016). *Cochrane Methods 2016*. Wiley. https://doi.org/10.1002/14651858.CD201601

Cheng, A., Hitt, C., Kisida, B., & Mills, J. N. (2017). "No Excuses" Charter Schools: A Meta-Analysis of the Experimental Evidence on Student Achievement. *Journal of School Choice*, *11*(2), 209–238.

Chiapello, M. (2018). *BALANCETABLE: Stata module to build a balance table and print it in a LaTeX file or an Excel file*. Boston College Department of Economics. Retrieved from https://ideas.repec.org/c/boc/bocode/s458424.html

Clark, M. A., Gleason, P. M., Tuttle, C. C., & Silverberg, M. K. (2015). Do Charter Schools Improve Student Achievement? *Educational Evaluation and Policy Analysis*, *37*(4), 419–436. https://doi.org/10.3102/0162373714558292

Coldron, J., Tanner, E., Finch, S., Shipton, L., Wolstenholme, C., Willis, B., … Stiell, B. (2008). *Secondary School Admissions*. Sheffield Hallam University and National Centre for Social Research. Retrieved from http://shura.shu.ac.uk/183/1/Sec_School_Adms_DCSFRR020.pdf

Contreras, Daniel. (2010). Derecho a la educación, inclusión y selección escolar. In *Ecos de la Revolución Pingüina: Avances, desafíos y silencios en la reforma educacional* (Bellei, C., Contreras, D., Valenzuela, J.P. (eds), pp. 311–330). Santiago, Chile: Universidad de Chile, UNICEF. Retrieved from https://www.unicef.cl/web/wp-content/uploads/doc_wp/Ecos%20de%20la%20revolucion.pdf

Contreras, Dante, Sepúlveda, P., & Bustos, S. (2010). When Schools Are the Ones that Choose: The Effects of Screening in Chile: School Vouchers in Chile. *Social Science Quarterly*, *91*(5), 1349–1368. https://doi.org/10.1111/j.1540-6237.2010.00735.x

Cullen, J. B., & Jacob, B. A. (2009). Is Gaining Access to Selective Elementary Schools Gaining Ground? Evidence from Randomized Lotteries. In *The problems of disadvantaged youth: an economic perspective* (pp. 43–84). University of Chicago Press.

Cullen, J. B., Jacob, B. A., & Levitt, S. (2006). The Effect of School Choice on Participants: Evidence from Randomized Lotteries. *Econometrica*, *74*(5), 1191–1230. https://doi.org/10.1111/j.1468-0262.2006.00702.x

Cullinane, C. (2020). *Fairer School Admissions. Social segregation in schools: the view from parents & teachers*. The Sutton Trust. Retrieved from https://www.suttontrust.com/wp-content/uploads/2020/02/Fairer-School-Admissions-Polling.pdf

Curto, V. E., & Fryer, R. G. (2014). The Potential of Urban Boarding Schools for the Poor: Evidence from SEED. *Journal of Labor Economics*, *32*(1), 65–93. https://doi.org/10.1086/671798

Deming, D. J., Hastings, J. S., Kane, T. J., & Staiger, D. O. (2014). School Choice, School Quality, and Postsecondary Attainment. *The American Economic Review; Nashville*, *104*(3), 991–1013. https://doi.org/10.1257/aer.104.3.991

Department for Education. School admissions code, § 84, School Standards and Framework Act 1998 (2014). Retrieved from https://www.gov.uk/government/publications/school-admissions-code--2

Di Piero, M. E. (2014). *¿Mérito y azar? Nociones de justicia distributiva y selección soft: el caso de una escuela secundaria tradicional frente al mandato inclusor.* (Tesis de Maestría). FLACSO. Retrieved from https://repositorio.flacsoandes.edu.ec/handle/10469/6465

Dupriez, V. (2010). *Methods of grouping learners at school*. Paris: UNESCO: International Institute for Educational Planning.

Duxbury, N. (1999). *Random Justice: On Lotteries and Legal Decision-Making*. Oxford University Press. Retrieved from 10.1093/acprof:oso/9780198268253.001.0001

Dynarski, S., Hubbard, D., Jacob, B., & Robles, S. (2018). *Estimating the Effects of a Large For-Profit Charter School Operator*. National Bureau of Economic Research.

Edmunds, J. A., Unlu, F., Glennie, E., Bernstein, L., Fesler, L., Furey, J., & Arshavsky, N. (2017). Smoothing the Transition to Postsecondary Education: The Impact of the Early College Model. *Journal of Research on Educational Effectiveness*, *10*(2), 297–325. https://doi.org/10.1080/19345747.2016.1191574

Education Quality Agency. (n.d.). *Datasets 2012-2018*. Santiago, Chile. Retrieved from http://informacionestadistica.agenciaeducacion.cl

Elacqua, G., Martinez, M., & Santos, H. (2011). *Lucro y Educación Escolar* (Claves de Políticas Públicas). Santiago, Chile: Instituto de Políticas Públicas, Universidad Diego Portales. Retrieved from http://www.politicaspublicas.udp.cl/publicaciones/detalle.tpl?id=285

Elacqua, G., Montt, P., & Santos, H. (2013). *Evidencias para eliminar -gradualmente- el Financiamiento Compartido* (Claves de Políticas Públicas). Santiago, Chile: Instituto de Políticas Públicas, Universidad Diego Portales. Retrieved from http://www.politicaspublicas.udp.cl/publicaciones/detalle.tpl?id=356

Elliott, J. (2005). *Using narrative in social research qualitative and quantitative approaches*. London: SAGE Publications.

Eyzaguirre, S. (2016). *Urgentes modificaciones al nuevo sistema de admisión escolar* (Puntos de Referencia N° 437). Santiago, Chile: Centro de Estudios Públicos. Retrieved from https://www.cepchile.cl/urgentes-modificaciones-al-nuevo-sistema-de-admision-escolar/cep/2016-09-20/170115.html

Eyzaguirre, S., Hernando, A., Razmilic, S., Blanco, N., Figueroa, N., Tagle, F., & Icaran, R. (2019). *¿Qué explica las diferencias socioeconómicas en las preferencias escolares?* (Puntos de Referencia N° 514). Santiago, Chile: Centro de Estudios Públicos. Retrieved from https://www.cepchile.cl/cep/puntos-de-referencia/puntos-de-referencia-2019/que-explica-las-diferencias-socioeconomicas-en-las-preferencias-escolares

Foreman, L. M., Anderson, K. P., Ritter, G. W., & Wolf, P. J. (2017). Using "Broken" Lotteries to Check the Validity of Charter School Evaluations Using Matching Designs. *Educational Policy*, 0895904817741543.

Friedman, M., & Friedman, R. D. (2002). *Capitalism and freedom* (40th anniversary ed). Chicago: University of Chicago Press.

Fullan, M. (2001). *The new meaning of educational change* (3. edition). New York: Teachers College Press.

Gallego, F., & Sapelli, C. (2007). El financiamiento de la educación en chile: una evaluación. *Pensamiento Educativo. Revista de Investigación Educacional Latinoamericana*, *40*(1), 263–284.

Gerber, A. S., & Green, D. P. (2012). *Field experiments: design, analysis, and interpretation* (1st ed). New York: W. W. Norton.

Gertler, P. J., Martinez, S., Premand, P., Rawlings, L. B., & Vermeersch, C. M. J. (2016). *Impact Evaluation in Practice, Second Edition*. The World Bank. https://doi.org/10.1596/978-1-4648-0779-4

Gleason, P., Clark, M., Tuttle, C. C., Dwoyer, E., & Silverberg, M. K. (2010). *The Evaluation of Charter School Impacts. Final Report* (NCEE 2010-4029). Washington, DC: National Center for Educational Evaluation and Regional Assistance, Institute of Education Sciences, U.S. Department of Education. Retrieved from https://ies.ed.gov/ncee/pubs/20104029/pdf/20104029.pdf

Godoy, F., Salazar, F., & Treviño, E. (2014). *Efectividad de los Liceos Públicos de Excelencia en Chile* (Informes para la Política Educativa Nº 1). Santiago, Chile: Centro de Políticas Comparadas de Educación, Universidad Diego Portales. Retrieved from http://cpce.udp.cl/wp-content/uploads/2016/08/IPE1.pdf

Gorard, S., & Smith, E. (2004). An international comparison of equity in education systems. *Comparative Education*, *40*(1), 15–28. https://doi.org/10.1080/0305006042000184863

Gough, D. (2007). Weight of Evidence: a framework for the appraisal of the quality and relevance of evidence. *Research Papers in Education*, *22*(2), 213–228. https://doi.org/10.1080/02671520701296189

Hanushek, E. A., & Wößmann, L. (2006). Does Educational Tracking Affect Performance and Inequality? Differences- in-Differences Evidence Across Countries. *The Economic Journal*, *116*(510), C63–C76. https://doi.org/10.1111/j.1468-0297.2006.01076.x

Harden, A., & Gough, D. (2012). Quality and relevance appraisal. In *An Introduction to Systematic Reviews* (Gough, D., Oliver, S., Thomas, J. (eds), pp. 153–178). SAGE Publications.

Hargreaves, D. H. (1996). Diversity and Choice in School Education: A Modified Libertarian Approach. *Oxford Review of Education*, *22*(2), 131–141.

Harrer, M., Cuijpers, P., & Ebert, D. (2019). Doing Meta-Analysis in R. *Zenodo*. https://doi.org/10.5281/zenodo.2551803

Hayes, R. J., & Moulton, L. H. (2009). *Cluster randomised trials*. Boca Raton: CRC Press.

Heyneman, S. (2009). International Perspectives on School Choice. In *Handbook of Research on School Choice* (Berends, M., Springer, M. G., Ballou, D., Walberg, H. J. (eds), pp. 79–95). Routledge. Retrieved from https://www.routledgehandbooks.com/doi/10.4324/9780203881781-5

Higgins, J. P. T., Thompson, S. G., Deeks, J. J., & Altman, D. G. (2003). Measuring inconsistency in meta-analyses. *BMJ*, *327*(7414), 557–560. https://doi.org/10.1136/bmj.327.7414.557

Hill, P. T. (2005). Doing School Choice Right. *American Journal of Education*, *111*(2), 141–150. https://doi.org/10.1086/426835

Hill, P. T., & Jochim, A. E. (2009). Political Perspectives on School Choice. In *Handbook of Research on School Choice* (Berends, M., Springer, M. G., Ballou, D., Walberg, H. J. (eds), pp. 3–17). Routledge. Retrieved from https://www.routledgehandbooks.com/doi/10.4324/9780203881781-1

Hoxby, C., & Murarka, S. (2009). *Charter Schools in New York City: Who Enrolls and How They Affect Their Students' Achievement* (Working paper No. w14852). National Bureau of Economic Research. Retrieved from https://www.nber.org/papers/w14852

Hoxby, C., & Rockoff, J. (2005). The impact of charter schools on student achievement. Presented at the RAND Seminars. Retrieved from http://www.rand.org/content/dam/rand/www/external/labor/seminars/adp/pdfs/2005hoxby.pdf

Hsieh, C.-T., & Urquiola, M. (2006). The effects of generalized school choice on achievement and stratification: Evidence from Chile's voucher program. *Journal of Public Economics*, *90*(8–9), 1477–1503. https://doi.org/10.1016/j.jpubeco.2005.11.002

Jafar, A. J. N. (2018). What is positionality and should it be expressed in quantitative studies? *Emergency Medicine Journal : EMJ; London*, *35*(5), 323. https://doi.org/10.1136/emermed-2017-207158

Jann, B. (2014). Plotting regression coefficients and other estimates. *Stata Journal*, *14*(4), 708–737.

Josephson, K., Francis, R., & Jayaram, S. (2018). *Promoting secondary school retention in Latin America and the Caribbean. Lessons from Mexico and Chile*. CAF - Development Bank of Latin America. Retrieved from http://scioteca.caf.com/handle/123456789/1248

Knechtel, V., Coen, T., Caronongan, P., Fung, N., & Goble, L. (2017). *Pre-Kindergarten Impacts Over Time: An Analysis of KIPP Charter Schools*. Princeton: Mathematica Policy Research. Retrieved from https://search.proquest.com/docview/2083834107?accountid=14511

Krowka, S. K., Hadd, A. R., & Marx, R. A. (2017). *"No Excuses" charter schools for increasing math and literacy achievement in primary and secondary education: a systematic review*. The Campbell Collaboration. https://doi.org/10.4073/csr.2017.9

Lipsey, M. W., & Wilson, D. B. (2001). *Practical meta-analysis*. Thousand Oaks, Calif. ; London: Sage.

Lubienski, C. (2016). *Review of "A Win-Win Solution" and "The Participant Effects of Private School Vouchers across the Globe."* Boulder, CO: National Education Policy Center. Retrieved from http://nepc.colorado.edu/thinktank/review-meta-analysis

MacLeod, W. B., & Urquiola, M. (2009). *Anti-Lemons: School Reputation and Educational Quality* (No. w15112). Cambridge, MA: National Bureau of Economic Research. Retrieved from http://www.nber.org/papers/w15112.pdf

Major, L. E., & Machin, S. (2019). *Social Mobility: CEP Election Analysis*. LSE Centre for Economic Performance. Retrieved from http://cep.lse.ac.uk/pubs/download/ea045.pdf

Manríquez, B. (2016, April). *Efectos de los liceos públicos emblemáticos sobre el desempeño escolar el caso del Liceo N.1 de Niñas Javiera Carrera* (Magíster en Análisis Económico). Universidad de Chile. Retrieved from http://repositorio.uchile.cl/handle/2250/138585

Manzi, J. (2007). Selección de estudiantes en el sistema escolar chileno: ¿la buena educación debe ser sólo para los mejores? In *La reforma al sistema escolar: aportes para el debate* (Brunner, J. and Peña, C. (eds), pp. 204–220). Santiago, Chile: Ediciones UDP. Retrieved from https://www.unicef.cl/web/wp-content/uploads/doc_wp/Ecos%20de%20la%20revolucion.pdf

Mena, P., & Corbalán, F. (2010). La selección escolar: una cuestión de libertad, competencia, igualdad e integración. In *Ecos de la Revolución Pingüina: Avances, desafíos y silencios en la reforma educacional* (Bellei, C., Contreras, D., Valenzuela, J.P. (eds), pp. 331–363). Santiago, Chile: Universidad de Chile, UNICEF. Retrieved from https://www.unicef.cl/web/wp-content/uploads/doc_wp/Ecos%20de%20la%20revolucion.pdf

Ministry of Education. Ley de Subvención Escolar Preferencial [Preferential School Subsidy Act], Pub. L. No. 20248 (2008). Retrieved from http://bcn.cl/1uv4p

Ministry of Education. Ley General de Educacion [General Education Act], Pub. L. No. 20370 (2009). Retrieved from www.leychile.cl/Navegar?idNorma=1006043

Ministry of Education. Fija normas generales sobre calendario escolar [Set general rules on school calendar], Decree 289 § (2010). Retrieved from http://bcn.cl/1xi8l

Ministry of Education. (2012a). *Impacto de la Ley SEP en SIMCE: una mirada a 4 años de su implementación* (Serie Evidencias). Ministerio de Educación Chile, Centro de Estudios. Retrieved from http://centroestudios.mineduc.cl/tp_enlaces/portales/tp5996f8b7cm96/uploadImg/File/A1N8_ImpactoSEP.pdf

Ministry of Education. (2012b). *Resultados TIMSS 2011 Chile*. Agencia de Calidad de la Educación, División de Estudios. Retrieved from http://www.agenciaeducacion.cl/wp-content/uploads/2013/02/resultados-timss-18-dic-2012.pdf

Ministry of Education. (2013). *Chile en el panorama educacional internacional OCDE: avances y desafíos* (Serie Evidencias). Ministerio de Educación Chile, Centro de Estudios. Retrieved from http://centroestudios.mineduc.cl/tp_enlaces/portales/tp5996f8b7cm96/uploadImg/File/Evidencias/A2N18_Chile_en_OECD.pdf

Ministry of Education. (2014). *Entrega de Resultados de Aprendizaje 2014, Simce e indicadores de desarrollo personal y social*. Agencia de Calidad de la Educación. Retrieved from http://archivos.agenciaeducacion.cl/resultados-2014/Presentacion_Entrega_Resultados_2014.pdf

Ministry of Education. Ley de Inclusion Escolar [School Inclusion Act], Pub. L. No. 20845 (2015). Retrieved from https://www.leychile.cl/Navegar?idNorma=1078172

Ministry of Education. (2016). Metodología de Construcción Indicadores de desarrollo de personal y social. Agencia de Calidad de la Educación. Retrieved from http://www.agenciaorienta.cl/pdf/Metodolog%C3%ADa%20de%20C%C3%A1lculo%20IDPS_dic2016.pdf

Ministry of Education. (2017, April 26). Manual Bases de datos Sistema de Admisión Escolar 2016.

Ministry of Education. (n.d.-a). Categorías de Desempeño [School performance categories website]. Retrieved from https://www.agenciaeducacion.cl/evaluaciones/categoria-de-desempeno/

Ministry of Education. (n.d.-b). Datos Abiertos [Open access data portal]. Retrieved from http://datosabiertos.mineduc.cl/

Mizala, A. (2007). *La economía política de la reforma educacional en Chile [Political Economy of the Educational Reform in Chile]* (Serie Estudios Socio / Económicos No. 36). CIEPLAN. Retrieved from http://www.cieplan.org/media/publicaciones/archivos/153/Capitulo_1.pdf

Mizala, A., & Torche, F. (2012). Bringing the schools back in: the stratification of educational achievement in the Chilean voucher system. *International Journal of Educational Development*, *32*(1), 132–144. https://doi.org/10.1016/j.ijedudev.2010.09.004

Moher, D., Liberati, A., Tetzlaff, J., & Altman, D. G. (2009). Preferred reporting items for systematic reviews and meta-analyses: the PRISMA statement. *BMJ*, *339*(jul21 1), b2535–b2535. https://doi.org/10.1136/bmj.b2535

Moher, D., Shamseer, L., Clarke, M., Ghersi, D., Liberati, A., Petticrew, M., … PRISMA-P Group. (2015). Preferred reporting items for systematic review and meta-analysis protocols (PRISMA-P) 2015 statement. *Systematic Reviews*, *4*(1). https://doi.org/10.1186/2046-4053-4-1

Musset, P. (2012). *School Choice and Equity: Current Policies in OECD Countries and a Literature Review* (OECD Education Working Papers No. 66) (p. 51). OECD. Retrieved from http://www.oecd-ilibrary.org/education/school-choice-and-equity_5k9fq23507vc-en

OECD. (2004). *Reviews of National Policies for Education: Chile 2004*. OECD. https://doi.org/10.1787/9789264106352-en

OECD. (2010). *PISA 2009 Results: What Makes a School Successful: Resources, Policies and Practices (Volume IV)*. OECD. https://doi.org/10.1787/9789264091559-en

OECD. (2013). *PISA 2012 Results: What Makes Schools Successful: Resources, Policies and Practices (Volume IV)*. OECD. https://doi.org/10.1787/9789264201156-en

OECD. (2014). Chile. In *Education at a Glance 2014: OECD Indicators*. Paris: OECD Publishing. https://doi.org/10.1787/eag-2014-46-en

OECD. (2019). *Balancing School Choice and Equity: An International Perspective Based on Pisa*. Paris: OECD Publishing. Retrieved from https://doi.org/10.1787/e9db7b00-en

Oliver, S., & Sutcliffe, K. (2012). Describing and analysing studies. In *An Introduction to Systematic Reviews* (Gough, D., Oliver, S., Thomas, J. (eds), pp. 135–152). SAGE Publications.

O'Mara-Eves, A., & Thomas, J. (2016). Ongoing developments in meta-analytic and quantitative synthesis methods: Broadening the types of research questions that can be addressed. *Review of Education*, *4*(1), 5–27. https://doi.org/10.1002/rev3.3062

Parry, T. R. (1996). Will Pursuit of Higher Quality Sacrifice Equal Opportunity in Education? An Analysis of the Education Voucher System in Santiago. *Social Science Quarterly*, *77*(4), 821–841.

Quiroz, S. S., Dari, N. L., & Cervini, R. A. (2018). Nivel Socioeconómico y Brecha entre Educación Secundaria Pública y Privada en Argentina. Los Datos de PISA 2015. *REICE. Revista Iberoamericana Sobre Calidad, Eficacia y Cambio En Educación*, *16*(4), 79–97. https://doi.org/10.15366/reice2018.16.4.005

Ramírez Fernández, C. (2017). *Análisis sobre el diseño e implementación del fin de la selección escolar en el contexto de la Reforma Educacional en Chile* (Magíster en Gestión y Políticas Públicas). Universidad de Chile. Retrieved from http://repositorio.uchile.cl/handle/2250/146511

Schneider, M., Elacqua, G., & Buckley, J. (2006). School choice in Chile: Is it class or the classroom? *Journal of Policy Analysis and Management*, *25*(3), 577–601. https://doi.org/10.1002/pam.20192

Schwarzer, G. (2007). meta: An R package for meta-analysis. *R News*, *7*(3), 40–45.

Shakeel, M. D., Anderson, K. P., & Wolf, P. J. (2016). *The Participant Effects of Private School Vouchers Across the Globe: A Meta-Analytic and Systematic Review* (SSRN Scholarly Paper No. ID 2777633). Rochester, NY: Social Science Research Network. Retrieved from https://papers.ssrn.com/abstract=2777633

Shamseer, L., Moher, D., Clarke, M., Ghersi, D., Liberati, A., Petticrew, M., … PRISMA-P Group. (2015). Preferred reporting items for systematic review and meta-analysis protocols (PRISMA-P) 2015: elaboration and explanation. *BMJ*, *349*(jan02 1), g7647–g7647. https://doi.org/10.1136/bmj.g7647

Smithers, A., & Robinson, P. (2010). *Choice and selection in school admissions: the experience of other countries*. The Sutton Trust. Retrieved from https://www.suttontrust.com/our-research/choice-selection-admissions-experience-countries/

Social Market Foundation. (2004). *School Admissions*. Social Market Foundation. Retrieved from http://www.smf.co.uk/publications/school-admissions/

Stasz, C., & Von Stolk, C. (2007). *The use of lottery systems in school admissions*. RAND. Retrieved from http://www.conallboyle.com/lottery/RAND_LEduMay07.pdf

StataCorp. (2017). Stata Statistical Software (Version 15). College Station, TX: StataCorp LLC.

Stone, P. (2008). What can lotteries do for education? *School Field*, *6*(3), 267–282. https://doi.org/10.1177/1477878508095583

Sutton Trust. (2007). *Ballots in School Admissions*. The Sutton Trust. Retrieved from http://www.suttontrust.com/researcharchive/ballots-school-admissions/

Thomas, J., Brunton, J., & Graziosi, S. (2019). *EPPI-Reviewer 4: software for research synthesis. EPPI-Centre Software*. London: Social Science Research Unit, UCL Institute of Education.

Thomas, J., O'Mara-Eves, A., Harden, A., & Newman, M. (2017). Synthesis methods for combining and configuring textual or mixed methods data. In *An Introduction to Systematic Reviews, 2nd edition* (Gough, D., Oliver, S., Thomas, J. (eds), pp. 181–209). SAGE Publications.

Thomas, J., O'Mara-Eves, A., Kneale, D., & Shemilt, I. (2017). Synthesis methods for combining and configuring quantitative data. In *An Introduction to Systematic Reviews, 2nd edition* (Gough, D., Oliver, S., Thomas, J. (eds), pp. 211–249). SAGE Publications.

Tuttle, C. C., Gleason, P., & Clark, M. (2012). Using lotteries to evaluate schools of choice: Evidence from a national study of charter schools. *Economics of Education Review*, *31*(2), 237–253. https://doi.org/10.1016/j.econedurev.2011.07.002

U.S. Department of Education. Elementary and Secondary Education Act, 20 U.S.C. 7221-7221 § 5201 – 5211 (2002). Retrieved from https://www2.ed.gov/policy/elsec/leg/esea02/pg62.html

Valenzuela, J. P., Bellei, C., & De los Rios, D. (2009). *Evolución de la segregación socioeconómica y su relación con el financiamiento compartido: el caso chileno*. Santiago, Chile: FONIDE, MINEDUC.

Valenzuela, J. P., Bellei, C., & De los Rios, D. (2010). Segregación escolar en Chile. In *¿Fin de ciclo? Cambios en la gobernanza del sistema educativo* (Martinic, S. and Elacqua, G. (eds), pp. 209–229). Santiago, Chile: UNESCO, Pontificia Universidad Católica de Chile.

Walford, G. (1996). Diversity and Choice in School Education: An Alternative View. *Oxford Review of Education*, *22*(2), 143–154.

Wang, C. H., Chie, B.-T., & Chen, S.-H. (2017). Transitional student admission mechanism from tracking to mixing: an agent-based policy analysis. *Evolutionary and Institutional Economics Review*, *14*(1), 253–293. https://doi.org/10.1007/s40844-016-0058-x

Wang, J., Herman, J. L., & Dockterman, D. (2018). A research synthesis of magnet school effect on student outcomes: Beyond descriptive studies. *Journal of School Choice*, *12*(2), 157–180.

Wilson, D. B. (n.d.). *Practical Meta-Analysis Effect Size Calculator*. Retrieved from http://www.campbellcollaboration.org/escalc/html/EffectSizeCalculator-Home.php

WWC. (2017). What Works Clearinghouse Standards Handbook (Version 4.0). Institute of Education Sciences (IES), U.S. Department of Education. Retrieved from https://ies.ed.gov/ncee/wwc/Docs/referenceresources/wwc_standards_handbook_v4.pdf

Wylie, C. (1998). *Can vouchers deliver? A review of the literature, with especial reference to New Zealand*. Wellington: New Zealand: Council for Educational Research. Retrieved from http://www.nzcer.org.nz/system/files/5835.pdf

Zimmer, R., Gill, B., Booker, K., Lavertu, S., Sass, T., & Witte, J. (2009). *Charter Schools in Eight States: Effects on Achievement, Attainment, Integration, and Competition*. RAND Corporation. https://doi.org/10.7249/MG869

7. Appendix Ethics Review Procedures

Anyone conducting research under the auspices of the Institute (staff, students or visitors) where the research involves human participants or the use of data collected from human participants, is required to gain ethical approval before starting. This includes preliminary and pilot studies. Please answer all relevant questions in terms that can be understood by a lay person and note that your form may be returned if incomplete.

For further support and guidance please see accompanying guidelines and the Ethics Review Procedures for Student Research http://www.ucl.ac.uk/srs/research-ethics-committee/ioe or contact your supervisor or IOE.researchethics@ucl.ac.uk.

Before completing this form you will need to discuss your proposal fully with your supervisor(s). Please attach all supporting documents and letters.

For all Psychology students, this form should be completed with reference to the British Psychological Society (BPS) Code of Human Research Ethics and Code of Ethics and Conduct.

7.1. Approved ethics form - Chapter 2

Section 1 Project details					
a.	Project title	Are lotteries the students' best chance? A meta-analysis of the effects of school randomised admissions			
b.	Student name	Constanza Gonzalez Parrao			
c.	Supervisor/Personal Tutor	Alison O'Mara-Eves			
d.	Department	Social Science - QSS			
e.	Course category (Tick one)	PhD/MPhil	☒	EdD	☐
		MRes	☐	DEdPsy	☐
		MTeach	☐	MA/MSc	☐
		ITE	☐		
		Diploma (state which)	☐		
		Other (state which)	☐		
f.	Course/module title	Thesis dissertation chapter			
g.	**If applicable,** state who the funder is and if funding has been confirmed.	Bloomsbury DTC and CONICYT-Becas Chile			
h.	Intended research start date	23-08-2016			
i.	Intended research end date	31-10-2018			
j.	Country fieldwork will be conducted in *If research to be conducted abroad please ensure travel insurance is obtained through UCL http://www.ucl.ac.uk/finance/insurance/travel*	No fieldwork is contemplated for this research			
k.	Has this project been considered by another (external) Research Ethics Committee?				

	Yes ☐	External Committee Name:
	No ☒ *go to Section 2*	Date of Approval:

If yes:

- **Submit a copy of the approval letter with this application.**
- Proceed to Section 10 Attachments.

Note: Ensure that you check the guidelines carefully as research with some participants will require ethical approval from a different ethics committee such as the National Research Ethics Service (NRES) or Social Care Research Ethics Committee (SCREC). In addition, if your research is based in another institution then you may be required to apply to their research ethics committee.

Section 2 Project summary

Research methods (tick all that apply)

Please attach questionnaires, visual methods and schedules for interviews (even in draft form).

☐ Interviews	☐ Controlled trial/other intervention study
☐ Focus groups	☐ Use of personal records
☐ Questionnaires	☒ Systematic review ***if only method used go to Section 5.***
☐ Action research	☐ Secondary data analysis ***if secondary analysis used go to Section 6.***
☐ Observation	☐ Advisory/consultation/collaborative groups
☐ Literature review	☐ Other, give details:

Please provide an overview of your research. *This should include some or all of the following: purpose of the research, aims, main research questions, research design, participants, sampling, your method of data collection (e.g., observations, interviews, questionnaires, etc.) and kind of questions that will be asked, reporting and dissemination (typically 300-500 words).*

The present research is part of my doctoral thesis focused on school admission policies. Along with this project, I also study the effects of two school admission policies in the Chilean context: selective admissions by ability and socioeconomic background, and school lotteries.

A lottery is a decision-making process in which the outcome cannot be predicted or influenced by those who apply or implement it. One of the key contributions of lotteries is that they give justice to the process (Duxbury, 1999), but while this mechanism guarantees equality of opportunities, it does not necessarily generate equality in the resources assigned. In the context of education, random-based decisions are usually set on school choice systems and mainly aim to solve the issue of student allocation into schools. Lotteries could be critical to help pursue the goal of equity in education under a school choice system, as they would remove cases of discrimination or handpicking of students in the school admission processes. On the other hand, school lotteries would also eliminate the ability of parents to ensure a school place due to, for example, a housing decision or an interview outcome.

Although I am aware of some individual school programmes or policy evaluations including school random admissions, these show mixed results in terms of student achievement. Moreover, and to the best of our knowledge, there are no systematic reviews or meta-analyses available that specifically address the effectiveness of school lotteries and consolidates this from an international perspective. Given the lack of academic efforts to examine and systematise the evidence on school-randomised admissions, this review emerges as a relevant proposition to build up the literature and inform researchers, policy-makers, government agencies, school systems, and families involved with school lotteries. The review would also be of benefit as I anticipate that our strategy will accomplish a more accurate international perspective that would help to fill the current evidence gap towards school lotteries.

The review aims to, firstly, map and systematise the evidence available on the impact of randomised school admissions on academic performance and socioeconomic composition measures. Secondly, it intends to meta-analyse an overall effect of school lotteries on student achievement, school-level socioeconomic measures, and other available outcomes. The main research question guiding the systematic review is to understand the scope of the evidence available on the impact of randomised school admissions. Secondary research questions focus on (i) where these school admissions are used; (ii) which schools use this type of admission, for what purpose, and how are they implemented; and (iii) how has this type of school admission been evaluated in the available literature.

The review contemplates the production of a protocol (see attachment for more details on the review) and two reports. The first and main document will include a mapping section to address the primary and secondary research questions, the meta-analysis of the data, and a section discussing the policy implications of the review. The second report will have a journal paper format, addressing mainly the primary research question and, therefore, focusing on the meta-analysis process.

Section 3 Participants

Please answer the following questions giving full details where necessary. Text boxes will expand for your responses.

a.	Will your research involve human participants?	Yes ☐	No ☐ ⇨ *go to Section 4*

b. Who are the participants (i.e. what sorts of people will be involved)? Tick all that apply.

☐ Early years/pre-school	☐ Unknown – specify below
☐ Ages 5-11	☐ Adults *please specify below*
☐ Ages 12-16	☐ Other – specify below
☐ Young people aged 17-18	

NB: Ensure that you check the **guidelines** (Section 1) carefully as research with some participants will require ethical approval from a different ethics committee such as the National Research Ethics Service (NRES).

c. If participants are under the responsibility of others (such as parents, teachers or medical staff) how do you intend to obtain permission to approach the participants to take part in the study?
(Please attach approach letters or details of permission procedures – see Section 9 Attachments.)

d. How will participants be recruited (identified and approached)?

e. Describe the process you will use to inform participants about what you are doing.

f. How will you obtain the consent of participants? Will this be written? How will it be made clear to participants that they may withdraw consent to participate at any time?
See the guidelines for information on opt-in and opt-out procedures. Please note that the method of consent should be appropriate to the research and fully explained.

g. **Studies involving questionnaires:** Will participants be given the option of omitting questions they do not wish to answer?
Yes ☐ No ☐

If **NO** please explain why below and ensure that you cover any ethical issues arising from this in section 8.

h. **Studies involving observation:** Confirm whether participants will be asked for their informed consent to be observed.
Yes ☐ No ☐

If **NO** read the guidelines (Ethical Issues section) and explain why below and ensure that you cover any ethical issues arising from this in section 8.

i.	Might participants experience anxiety, discomfort or embarrassment as a result of your study? Yes ☐ No ☐	
	If **yes** what steps will you take to explain and minimise this? If **not**, explain how you can be sure that no discomfort or embarrassment will arise?	
j.	Will your project involve deliberately misleading participants (deception) in any way? Yes ☐ No ☐	
	If **YES** please provide further details below and ensure that you cover any ethical issues arising from this in section 8.	
k.	Will you debrief participants at the end of their participation (i.e. give them a brief explanation of the study)? Yes ☐ No ☐	
	If **NO** please explain why below and ensure that you cover any ethical issues arising from this in section 8.	
l.	Will participants be given information about the findings of your study? (This could be a brief summary of your findings in general; it is not the same as an individual debriefing.) Yes ☐ No ☐	
	If **no**, why not?	

Section 4 Security-sensitive material Only complete if applicable

Security sensitive research includes: commissioned by the military; commissioned under an EU security call; involves the acquisition of security clearances; concerns terrorist or extreme groups.

a.	Will your project consider or encounter security-sensitive material?	Yes ☐ *	No ☐
b.	Will you be visiting websites associated with extreme or terrorist organisations?	Yes ☐ *	No ☐
c.	Will you be storing or transmitting any materials that could be interpreted as promoting or endorsing terrorist acts?	Yes ☐ *	No ☐

** Give further details in **Section 8 Ethical Issues***

Section 5 Systematic review of research Only complete if applicable

a.	Will you be collecting any new data from participants?	Yes ☐ *	No ☒
b.	Will you be analysing any secondary data?	Yes ☐ *	No ☒

** Give further details in **Section 8 Ethical Issues***
*If your methods do not involve engagement with participants (e.g. systematic review, literature review) **and** if you have answered **No** to both questions, please go to **Section 10 Attachments.***

Section 6 Secondary data analysis Complete for all secondary analysis

a.	Name of dataset/s		
b.	Owner of dataset/s		
c.	Are the data in the public domain?	Yes ☐	No ☐ ***If no,** do you have the owner's permission/license?* Yes ☐ No* ☐
d.	Are the data anonymised?	Yes ☐	No ☐
		Do you plan to anonymise the data?	Yes ☐ No* ☐
		Do you plan to use individual level data?	Yes* ☐ No ☐
		Will you be linking data to individuals?	Yes* ☐ No ☐
e.	Are the data sensitive (DPA 1998 definition)?		Yes* ☐ No ☐
f.	Will you be conducting analysis within the remit it was originally collected for?		Yes ☐ No* ☐

g.	**If no,** was consent gained from participants for subsequent/future analysis?	Yes ☐	No* ☐
h.	**If no,** was data collected prior to ethics approval process?	Yes ☐	No* ☐

** Give further details in **Section 8 Ethical Issues***
*If secondary analysis is only method used **and** no answers with asterisks are ticked, go to **Section 9 Attachments.***

Section 7 Data Storage and Security *Please ensure that you include all hard and electronic data when completing this section.*

a.	Confirm that all personal data will be stored and processed in compliance with the Data Protection Act 1998 (DPA 1998). *(See the Guidelines and the Institute's Data Protection & Records Management Policy for more detail.)*		Yes ☐
b.	Will personal data be processed or be sent outside the European Economic Area?	Yes ☐ *	No ☐

*** If yes,** please confirm that there are adequate levels of protections in compliance with the DPA 1998 and state what these arrangements are below.

c.	Who will have access to the data and personal information, including advisory/consultation groups and during transcription?

During the research

d.	Where will the data be stored?
e.	Will mobile devices such as USB storage and laptops be used? Yes ☐ * No ☐ *** If yes,** state what mobile devices: *** If yes,** will they be encrypted?:

After the research

f.	Where will the data be stored?
g.	How long will the data and records be kept for and in what format?
h.	Will data be archived for use by other researchers? Yes ☐ * No ☐ *** If yes,** please provide details.

Section 8 Ethical issues

Are there particular features of the proposed work which may raise ethical concerns or add to the complexity of ethical decision making? If so, please outline how you will deal with these.
It is important that you demonstrate your awareness of potential risks or harm that may arise as a result of your research. You should then demonstrate that you have considered ways to minimise the likelihood and impact of each potential harm that you have identified. Please be as specific as possible in describing the ethical issues you will have to address. Please consider / address ALL issues that may apply.
Ethical concerns may include, but not be limited to, the following areas:

- Methods
- Sampling
- Recruitment
- Gatekeepers
- Informed consent
- Potentially vulnerable participants
- Safeguarding/child protection
- Sensitive topics
- International research
- Risks to participants and/or researchers
- Confidentiality/Anonymity
- Disclosures/limits to confidentiality
- Data storage and security both during and after the research (including transfer, sharing, encryption, protection)
- Reporting
- Dissemination and use of findings

Sampling: To avoid imposing a bias in the results by omitting relevant studies in the analysis, the authors will implement a thorough search strategy. The review has an intended international focus for this same reason, and studies in two languages will be considered (English and Spanish). The search strategy includes an extensive list of 25 databases and search engines that will be considered for the literature search phase, which includes

academic and open sources with different geographical focuses. Additionally, references from systematic reviews found in the search stage will be examined to identify further potentially relevant primary studies.

Confidentiality/Anonymity: This review poses a low risk of confidentiality breach as the findings of existing studies to be included in the analysis are assumed as anonymised primary data. Moreover, these data are already available for the public through academic and open source databases.

Dissemination and use of findings: Since this review intends to have an international perspective in the search for relevant studies, the authors will also apply an extensive dissemination strategy for their final report, trying to make it as publicly available as possible. If the final report cannot be published on an open source platform, the authors will produce a lighter/preliminary version for public availability.

Collection of new data: In case missing data hinders the calculation of the effect size of a study, the first strategy - whenever possible and considering the resource limitations of this review - will be to contact the original research team to gather the relevant supplementary information. This request will not implicate the collection of new data from participants. Moreover, this would expose a low breach potential of participant confidentiality as any data request would be for clarification purposes and at the group-level (not at the individual level), hence it would not be possible to identify individual participants from the study or their characteristics.

Section 9 Further information

Outline any other information you feel relevant to this submission, using a separate sheet or attachments if necessary.

Section 10 Attachments Please attach the following items to this form, or explain if not attached

a.	Information sheets and other materials to be used to inform potential participants about the research, including approach letters	Yes ☐	No ☐
b.	Consent form	Yes ☐	No ☐
	If applicable:		
c.	The proposal for the project	Yes ☒	No ☐
d.	Approval letter from external Research Ethics Committee	Yes ☐	No ☐
e.	Full risk assessment	Yes ☐	No ☐

Section 11 Declaration

	Yes	No
I have read, understood and will abide by the following set of guidelines.	☒	☐

BPS ☐ BERA ☒ BSA ☐ Other (please state) ☐

I have discussed the ethical issues relating to my research with my supervisor.	☒	☐
I have attended the appropriate ethics training provided by my course.	☒	☐

I confirm that to the best of my knowledge:
The above information is correct and that this is a full description of the ethics issues that may arise in the course of this project.

Name	Constanza Gonzalez Parrao
Date	04 Sep 2017

Notes and references

Professional code of ethics

You should read and understand relevant ethics guidelines, for example:
British Psychological Society (2009) *Code of Ethics and Conduct*, and (2014) *Code of Human Research Ethics,* or British Educational Research Association (2011) *Ethical Guidelines, or* British Sociological Association (2002) *Statement of Ethical Practice*

Disclosure and Barring Service checks

If you are planning to carry out research in regulated Education environments such as Schools, or if your research will bring you into contact with children and young people (under the age of 18), you will need to have a Disclosure and Barring Service (DBS) CHECK, before you start. The DBS was previously known as the Criminal Records Bureau (CRB). If you do not already hold a current DBS check, and have not registered with the DBS update service, you will need to obtain one through UCL.
Ensure that you apply for the DBS check in plenty of time as will take around 4 weeks, though can take longer depending on the circumstances.

Further references

The www.ethicsguidebook.ac.uk website is very useful for assisting you to think through the ethical issues arising from your project.

- Robson, Colin (2011). *Real world research: a resource for social scientists and practitioner researchers* (3rd edition). Oxford: Blackwell. This text has a helpful section on ethical considerations.
- Alderson, P. and Morrow, V. (2011) *The Ethics of Research with Children and Young People: A Practical Handbook.* London: Sage. This text has useful suggestions if you are conducting research with children and young people.
- Wiles, R. (2013) What are Qualitative Research Ethics? Bloomsbury. A useful and short text covering areas including informed consent, approaches to research ethics including examples of ethical dilemmas.

Departmental use

If a project raises particularly challenging ethics issues, or a more detailed review would be appropriate, you may refer the application to the Research Ethics and Governance Administrator (via IOE.researchethics@ucl.ac.uk) so that it can be submitted to the Research Ethics Committee for consideration. A Research Ethics Committee Chair, ethics representatives in your department and the research ethics coordinator can advise you, either to support your review process, or help decide whether an application should be referred to the Research Ethics Committee.

Reviewer 1	
Supervisor name	Dr Alison O'Mara-Eves
Supervisor comments	This project poses low to no risk to human individuals, as the data have already been anonymised and published in other formats that are publicly available. Constanza has considered the ethical implications of doing justice to the original primary study participants and the existing research by conducting rigorous, transparent methods that attempt to minimise bias, as outlined in her protocol.
Supervisor signature	[*signature omitted deliberately*]
Reviewer 2	
Advisory committee/course team member name	Dr Dylan Kneale

Advisory committee/course team member comments	This study does not concern a sensitive topic, does not involve the use individual participant data, and is reliant solely on syntheses of existing published aggregate data. As such there are no additional ethical challenges to consider.	
Advisory committee/course team member signature	[*signature omitted deliberately*]	
Decision		
Date decision was made	September 8th 2017	
Decision	Approved	☒
	Referred back to applicant and supervisor	☐
	Referred to REC for review	☐
Recording	Recorded in the student information system	☐

7.2. Approved ethics form - Chapter 3

Section 1 Project details					
a.	Project title	School effectiveness or school handpicking? The effect of student selection in the Chilean school system			
b.	Student name	Constanza Gonzalez Parrao			
	UCL Data Protection Registration Number	Z6364106/2018/06/138 social research			
c.	Supervisor/Personal Tutor	Lorraine Dearden, Gill Wyness			
d.	Department	Social Science - QSS			
e.	Course category (Tick one)	PhD/MPhil	☒	EdD	☐
		MRes	☐	DEdPsy	☐
		MTeach	☐	MA/MSc	☐
		ITE	☐		
		Diploma (state which)	☐		
		Other (state which)	☐		
f.	Course/module title	Thesis dissertation chapter			
g.	**If applicable,** state who the funder is and if funding has been confirmed.	Bloomsbury DTC and CONICYT-Becas Chile			
h.	Intended research start date	01-10-2014			
i.	Intended research end date	30-09-2018			
j.	Country fieldwork will be conducted in *If research to be conducted abroad please ensure travel insurance is obtained through UCL* *http://www.ucl.ac.uk/finance/insurance/travel*	No fieldwork is contemplated for this research			
k.	Has this project been considered by another (external) Research Ethics Committee?				
	Yes ☐	External Committee Name:			

	No ☒ *go to Section 2*	Date of Approval:

If yes:

- **Submit a copy of the approval letter with this application.**
- Proceed to Section 10 Attachments.

Note: Ensure that you check the guidelines carefully as research with some participants will require ethical approval from a different ethics committee such as the National Research Ethics Service (NRES) or Social Care Research Ethics Committee (SCREC). In addition, if your research is based in another institution then you may be required to apply to their research ethics committee.

Section 2 Project summary

Research methods (tick all that apply)
Please attach questionnaires, visual methods and schedules for interviews (even in draft form).

☐ Interviews	☐ Controlled trial/other intervention study
☐ Focus groups	☐ Use of personal records
☐ Questionnaires	☐ Systematic review ***if only method used go to Section 5.***
☐ Action research	☒ Secondary data analysis ***if secondary analysis used go to Section 6.***
☐ Observation	☐ Advisory/consultation/collaborative groups
☒ Literature review	☐ Other, give details:

Please provide an overview of your research. *This should include some or all of the following: purpose of the research, aims, main research questions, research design, participants, sampling, your method of data collection (e.g., observations, interviews, questionnaires, etc.) and kind of questions that will be asked, reporting and dissemination (typically 300-500 words).*

Under a major reform in the 1980's, a voucher scheme was introduced in the school system to promote the development of the private sector in education. This reduced the restrictions on admission processes in schools; within a moderately broad law framework, each school has the autonomy to define its own admission process. One of the consequences we can see today is a wide prevalence of student selection practices among schools.

In the international context, there seems to be a consensus that student selection practices can be detrimental to school systems as they are unable to serve the whole student population. Therefore, selective practices are usually restricted to certain school ages (generally when students are older) and to particular capacities (for example, outstanding academic ability). Moreover, selective school systems also seem to be associated with academic and social segregation.

Indeed, the Chilean school system seems to be both academically and socially segregated. Then, it could be hypothesised that selective school admissions are associated with the levels of segregation in the education system: the more the schools can shape their student body, the more segregated the students will be. Yet in Chile, beyond theoretical discussions or descriptive data, there is little empirical evidence that directly associates student selection practices to academic and social segregation.

Exploring this relationship is especially relevant in the context of education policy in Chile and the school reform recently passed (2015), which contemplates changes to the schools' admission process. This research aims to develop the evidence on this topic by studying the prevalence over time of student selection practices in primary schools in Chile and examining the effect of student selection practices on academic performance and the school socioeconomic composition. The research will focus on the use of entry tests and parent interviews as archetypes of student selection by individual ability and family background.

The research will be based on a panel of all primary schools in Chile for which there is available data from years 2004 and 2013. Secondary data will be collected by linking administrative data from the Ministry of Education (MoE) and the national education quality assessment system (SIMCE), which is anonymised and available for research purposes.

Results are expected to be presented at academic conferences, published in an academic journal, and disseminated with a non-specialised audience with the purpose of education policy discussion.

Section 3 Participants

Please answer the following questions giving full details where necessary. Text boxes will expand for your responses.

a.	Will your research involve human participants?	Yes ☐	No ☐ ⇨ *go to Section 4*

b. Who are the participants (i.e. what sorts of people will be involved)? Tick all that apply.

☐ Early years/pre-school	☐ Unknown – specify below
☐ Ages 5-11	☐ Adults *please specify below*
☐ Ages 12-16	☐ Other – specify below
☐ Young people aged 17-18	

NB: Ensure that you check the **guidelines** (Section 1) carefully as research with some participants will require ethical approval from a different ethics committee such as the National Research Ethics Service (NRES).

c. If participants are under the responsibility of others (such as parents, teachers or medical staff) how do you intend to obtain permission to approach the participants to take part in the study?
(Please attach approach letters or details of permission procedures – see Section 9 Attachments.)

d. How will participants be recruited (identified and approached)?

e. Describe the process you will use to inform participants about what you are doing.

f. How will you obtain the consent of participants? Will this be written? How will it be made clear to participants that they may withdraw consent to participate at any time?
See the guidelines for information on opt-in and opt-out procedures. Please note that the method of consent should be appropriate to the research and fully explained.

g. **Studies involving questionnaires:** Will participants be given the option of omitting questions they do not wish to answer?
Yes ☐ No ☐

If **NO** please explain why below and ensure that you cover any ethical issues arising from this in section 8.

h. **Studies involving observation:** Confirm whether participants will be asked for their informed consent to be observed.
Yes ☐ No ☐

If **NO** read the guidelines (Ethical Issues section) and explain why below and ensure that you cover any ethical issues arising from this in section 8.

i. Might participants experience anxiety, discomfort or embarrassment as a result of your study?
Yes ☐ No ☐

If **yes** what steps will you take to explain and minimise this?
If **not**, explain how you can be sure that no discomfort or embarrassment will arise?

j. Will your project involve deliberately misleading participants (deception) in any way?
Yes ☐ No ☐

	If **YES** please provide further details below and ensure that you cover any ethical issues arising from this in section 8.
k.	Will you debrief participants at the end of their participation (i.e. give them a brief explanation of the study)? Yes ☐ No ☐
	If **NO** please explain why below and ensure that you cover any ethical issues arising from this in section 8.
l.	Will participants be given information about the findings of your study? (This could be a brief summary of your findings in general; it is not the same as an individual debriefing.) Yes ☐ No ☐
	If **no**, why not?

Section 4 Security-sensitive material Only complete if applicable

Security sensitive research includes: commissioned by the military; commissioned under an EU security call; involves the acquisition of security clearances; concerns terrorist or extreme groups.

a.	Will your project consider or encounter security-sensitive material?	Yes ☐ *	No ☐
b.	Will you be visiting websites associated with extreme or terrorist organisations?	Yes ☐ *	No ☐
c.	Will you be storing or transmitting any materials that could be interpreted as promoting or endorsing terrorist acts?	Yes ☐ *	No ☐

** Give further details in **Section 8 Ethical Issues***

Section 5 Systematic review of research Only complete if applicable

c.	Will you be collecting any new data from participants?	Yes ☐ *	No ☐
d.	Will you be analysing any secondary data?	Yes ☐ *	No ☐

** Give further details in **Section 8 Ethical Issues***
*If your methods do not involve engagement with participants (e.g. systematic review, literature review) **and** if you have answered **No** to both questions, please go to **Section 10 Attachments.***

Section 6 Secondary data analysis Complete for all secondary analysis

a.	Name of dataset/s	Administrative school data from the Chilean Ministry of Education. This research will use parts of these existing records.	
b.	Owner of dataset/s	The data is collected and made available for research by the Chilean Ministry of Education.	
c.	Are the data in the public domain?	Yes ☐	No ☒ ***If no,** do you have the owner's permission/license?* Yes ☒ No* ☐
d.	Are the data anonymised?	Yes ☒	No ☐
		Do you plan to anonymise the data? Yes ☐ No* ☐	
		Do you plan to use individual level data? Yes* ☒ No ☐	
		Will you be linking data to individuals? Yes* ☒ No ☐	

e.	Are the data sensitive (DPA 1998 definition)?	Yes* ☐	No ☒
f.	Will you be conducting analysis within the remit it was originally collected for?	Yes ☒	No* ☐
g.	**If no,** was consent gained from participants for subsequent/future analysis?	Yes ☐	No* ☐
h.	**If no,** was data collected prior to ethics approval process?	Yes ☐	No* ☐

** Give further details in **Section 8 Ethical Issues***
*If secondary analysis is only method used **and** no answers with asterisks are ticked, go to **Section 9 Attachments.***

Section 7 Data Storage and Security *Please ensure that you include all hard and electronic data when completing this section.*

a.	Confirm that all personal data will be stored and processed in compliance with the Data Protection Act 1998 (DPA 1998). *(See the Guidelines and the Institute's Data Protection & Records Management Policy for more detail.)*		Yes ☒
b.	Will personal data be processed or be sent outside the European Economic Area?	Yes ☒ *	No ☐

*** If yes,** please confirm that there are adequate levels of protections in compliance with the DPA 1998 and state what these arrangements are below.

All the data for this research is administrative data and will be provided by the Ministry of Education (MoE) Studies Centre. The student researcher will complete the MoE forms to detail the information needed at the student level. The researcher will also need to sign a confidentially agreement required by the MoE, based on the Chilean Protection of Personal Data Act, No 19.628 (link in Spanish only), declaring that the data will be utilised only for the purpose stated, that it will not be shared with third parties and that it will be deleted once it completes its specific use. The data delivered by the MoE will be anonymised.

Once obtained, the data will be processed and analysed in the UK.

c. Who will have access to the data and personal information, including advisory/consultation groups and during transcription?
Only the student researcher will have access to the data.

During the research

d. Where will the data be stored?
The anonymised dataset provided by the MoE will be stored in a personal drive owned by the researcher.

e. Will mobile devices such as USB storage and laptops be used? Yes ☒ * No ☐
*** If yes,** state what mobile devices:
The student researcher will use a hard drive to save the final anonymised data.
*** If yes,** will they be encrypted?
No

After the research

f. Where will the data be stored?
The data will be stored in a personal drive owned by the researcher.

g. How long will the data and records be kept for and in what format?
The data will be kept by the student researcher as a Stata file until its use is completed.

h. Will data be archived for use by other researchers? Yes ☐ * No ☒
*** If yes,** please provide details.

Section 8 Ethical issues

Are there particular features of the proposed work which may raise ethical concerns or add to the complexity of ethical decision making? If so, please outline how you will deal with these.
It is important that you demonstrate your awareness of potential risks or harm that may arise as a result of your research. You should then demonstrate that you have considered ways to minimise the likelihood and impact of each potential harm that you have identified. Please be as specific as possible in describing the ethical issues you will have to address. Please consider / address ALL issues that may apply.
Ethical concerns may include, but not be limited to, the following areas:

- Methods
- Sampling
- Recruitment
- Gatekeepers
- Informed consent
- International research
- Risks to participants and/or researchers
- Confidentiality/Anonymity
- Disclosures/limits to confidentiality
- Data storage and security both during and after the

− Potentially vulnerable participants − Safeguarding/child protection − Sensitive topics	research (including transfer, sharing, encryption, protection) − Reporting − Dissemination and use of findings

Data storage and security: A new dataset will be created for this research, combining administrative data at the school- and student-level from secondary sources. Although the dataset will be anonymised, the research includes a rich panel of 10 years of data. Due to the detailed nature of the data, it is important to take measures to ensure it is kept safe and confidential. To avoid disclosure of the data, these will be stored using encrypted drives and will only be managed by the student researcher.

Dissemination and use of findings: The research results are expected to be disseminated and published, at least in the academic sphere, and possibly to a wider audience related to the educational policy context. The data include primary schools in Chile for a period of 10 years, hence it will be analysed as an aggregate. However, cautionary measures will be taken not to provide information that could help identify individual schools.

Section 9 Further information

Outline any other information you feel relevant to this submission, using a separate sheet or attachments if necessary.

Users of the Ministry of Education data are required to register and sign a confidentiality agreement. Attached to this application are the data request form (in Spanish) and the English translation of the agreed responsibilities, which are general indications on how to manipulate, store, and cite the data.

The data is already anonymised by the MoE (to allow data linkage between datasets), and the information cannot be traced to individual students at any stage.

Section 10 Attachments Please attach the following items to this form, or explain if not attached

f.	Information sheets and other materials to be used to inform potential participants about the research, including approach letters	Yes ☐	No ☐
g.	Consent form	Yes ☐	No ☐
	If applicable:		
h.	The proposal for the project	Yes ☐	No ☐
i.	Approval letter from external Research Ethics Committee	Yes ☐	No ☐
j.	Full risk assessment	Yes ☐	No ☐

Section 11 Declaration

	Yes	No
I have read, understood and will abide by the following set of guidelines.	☒	☐

BPS ☐	BERA ☒	BSA ☐	Other (please state) ☒ Studies Centre - Ministry of Education, based on the Chilean Protection of Personal Data Act, No 19.628

I have discussed the ethical issues relating to my research with my supervisor.	☒	☐
I have attended the appropriate ethics training provided by my course.	☒	☐

I confirm that to the best of my knowledge:
The above information is correct and that this is a full description of the ethics issues that may arise in the course of this project.

Name	Constanza Gonzalez Parrao
Date	31 May 2017

Notes and references

Professional code of ethics

You should read and understand relevant ethics guidelines, for example:
British Psychological Society (2009) *Code of Ethics and Conduct*, and (2014) *Code of Human Research Ethics,* or British Educational Research Association (2011) *Ethical Guidelines, or* British Sociological Association (2002) *Statement of Ethical Practice*

Disclosure and Barring Service checks

If you are planning to carry out research in regulated Education environments such as Schools, or if your research will bring you into contact with children and young people (under the age of 18), you will need to have a Disclosure and Barring Service (DBS) CHECK, before you start. The DBS was previously known as the Criminal Records Bureau (CRB). If you do not already hold a current DBS check, and have not registered with the DBS update service, you will need to obtain one through UCL.
Ensure that you apply for the DBS check in plenty of time as will take around 4 weeks, though can take longer depending on the circumstances.

Further references

The www.ethicsguidebook.ac.uk website is very useful for assisting you to think through the ethical issues arising from your project.

- Robson, Colin (2011). *Real world research: a resource for social scientists and practitioner researchers* (3rd edition). Oxford: Blackwell. This text has a helpful section on ethical considerations.
- Alderson, P. and Morrow, V. (2011) *The Ethics of Research with Children and Young People: A Practical Handbook.* London: Sage. This text has useful suggestions if you are conducting research with children and young people.
- Wiles, R. (2013) What are Qualitative Research Ethics? Bloomsbury. A useful and short text covering areas including informed consent, approaches to research ethics including examples of ethical dilemmas.

Departmental use

If a project raises particularly challenging ethics issues, or a more detailed review would be appropriate, you may refer the application to the Research Ethics and Governance Administrator (via IOE.researchethics@ucl.ac.uk) so that it can be submitted to the Research Ethics Committee for consideration. A Research Ethics Committee Chair, ethics representatives in your department and the research ethics coordinator can advise you, either to support your review process, or help decide whether an application should be referred to the Research Ethics Committee.

Reviewer 1	
Supervisor name	Gill Wyness
Supervisor comments	I am happy that the researcher has taken the necessary steps to ensure there are no ethical issues or data protection issues with this project.
Supervisor signature	[*signature omitted deliberately*]
Reviewer 2	
Advisory committee/course team member name	Dr Lindsey Macmillan
Advisory committee/course team member comments	The researcher has considered the relevant issues to the secondary analysis of anonymised data.

Advisory committee/course team member signature	[*signature omitted deliberately*]	
Decision		
Date decision was made	28 June 2018	
Decision	Approved	☒
	Referred back to applicant and supervisor	☐
	Referred to REC for review	☐
Recording	Recorded in the student information system	☐

7.3. Approved ethics form - Chapter 4

Section 1 Project details					
a.	Project title	School lotteries and equal opportunities: Evaluating the impact of randomised admissions in Chile			
b.	Student name	Constanza Gonzalez Parrao			
c.	Supervisor/Personal Tutor	Lorraine Dearden, Gill Wyness			
d.	Department	Social Science - QSS			
e.	Course category (Tick one)	PhD/MPhil	☒	EdD	☐
		MRes	☐	DEdPsy	☐
		MTeach	☐	MA/MSc	☐
		ITE	☐		
		Diploma (state which)	☐		
		Other (state which)	☐		
f.	Course/module title	Thesis dissertation chapter			
g.	**If applicable,** state who the funder is and if funding has been confirmed.	Bloomsbury DTC and CONICYT-Becas Chile			
h.	Intended research start date	01-05-2017			
i.	Intended research end date	30-09-2018			
j.	Country fieldwork will be conducted in *If research to be conducted abroad please ensure travel insurance is obtained through UCL* *http://www.ucl.ac.uk/finance/insurance/travel*	Chile			

k.	Has this project been considered by another (external) Research Ethics Committee?	
	Yes ☐	External Committee Name:
	No ☒ *go to Section 2*	Date of Approval:

If yes:
- **Submit a copy of the approval letter with this application.**
- Proceed to Section 10 Attachments.

Note: Ensure that you check the guidelines carefully as research with some participants will require ethical approval from a different ethics committee such as the National Research Ethics Service (NRES) or Social Care Research Ethics Committee (SCREC). In addition, if your research is based in another institution then you may be required to apply to their research ethics committee.

Section 2 Project summary

Research methods (tick all that apply)
Please attach questionnaires, visual methods and schedules for interviews (even in draft form).

- ☐ Interviews
- ☐ Focus groups
- ☐ Questionnaires
- ☐ Action research
- ☐ Observation
- ☒ Literature review
- ☐ Controlled trial/other intervention study
- ☒ Use of personal records
- ☐ Systematic review ***if only method used go to Section 5.***
- ☒ Secondary data analysis ***if secondary analysis used go to Section 6.***
- ☐ Advisory/consultation/collaborative groups
- ☐ Other, give details:

Please provide an overview of your research. *This should include some or all of the following: purpose of the research, aims, main research questions, research design, participants, sampling, your method of data collection (e.g., observations, interviews, questionnaires, etc.) and kind of questions that will be asked, reporting and dissemination (typically 300-500 words).*

The Chilean school system is highly segregated, both at the academic and socioeconomic level. A key component associated with these levels of segregation is related to the schools' admission policies, which is one of the core issues that the new educational reform addresses. Based on the principles of "transparency, inclusive education, universal accessibility, equity, no arbitrary discrimination, and school choice", the School Inclusion Act (2015) introduces a centralised admission system for all schools that receive state funding, shifting the regulation of the admission processes from each school to the Ministry of Education.

This new admissions regulation specifies that schools will need to admit all applicants if they have places available, and in case of oversubscription, applicants with siblings in the school and children of school staff will have priority admission. Subsequently, a randomised process will assign the remaining places by taking into account the school preferences of each family. Randomised school admissions would give the same chance to all applicants to study in the school of their choice, and would make schools improve their efforts in producing quality outcomes.

School admission practices via lottery have not been commonly studied in the Chilean context, and to the best of my knowledge, there are no evaluations of school lotteries in Chile. There are currently some schools that claim to use lotteries in their admission process. Yet, this use is mainly for allocating some few places available after a priority admission criteria, which is determined by each school. The difficulty in identifying all of these schools and the small number of places allocated by lottery in each school hinders the evaluation of these admission policies. Likewise, the admission component of the school reform is being phased in since 2016 and will take some years to be implemented and evaluated at the national level.

However, there is a new school that opened recently and used a lottery system to allocate its whole student body. The school has been highly attractive for families, receiving much more applications than places available, which makes it a unique case to evaluate randomised admission at the student level. This research proposes to fill the evidence gap around school lotteries by evaluating with an experimental approach the effect of using school randomised admissions in the Chilean context.

The general objective is to try to anticipate the scenario of the school reform by evaluating the experience of a school that already uses random admission. The aim is to

understand whether there is evidence in Chile to translate a non-selective admission practice in an effect on academic attainment and development outcomes of the students, and on the socioeconomic composition of the school.

In this sense, the research aims to generate evidence that could provide relevant information for the implementation and evaluation of the new school admission system, which is currently being introduced. Methodologically, there is a value in evaluating a causal effect in this subject. Under an experimental approach, new and relevant evidence could be provided with a high explanatory power about the effect of randomisation on student performance, development, and socioeconomic measures. Substantially, the research could also indicate some direction on whether these admission practices are a feasible way to address school segregation.

A unique dataset would be created for this research. Data collection would imply digitalising some key information from the original application forms for each student who applied to this school and linking this information with administrative data from the Ministry of Education (MoE), which is already openly available for researchers. The Studies Centre from the MoE has already confirmed they can arrange for this information, provided the dataset is returned in an anonymised format. This unique anonymised dataset would be used for the analysis.

Results are expected to be presented at academic conferences, published in an academic journal, and disseminated with a non-specialised audience with the purpose of education policy discussion.

Section 3 Participants

Please answer the following questions giving full details where necessary. Text boxes will expand for your responses.

a.	Will your research involve human participants?	Yes ☐	No ☐ ⇨ *go to Section 4*

b. Who are the participants (i.e. what sorts of people will be involved)? Tick all that apply.

☐ Early years/pre-school	☐ Unknown – specify below
☐ Ages 5-11	☐ Adults *please specify below*
☐ Ages 12-16	☐ Other – specify below
☐ Young people aged 17-18	

NB: Ensure that you check the **guidelines** (Section 1) carefully as research with some participants will require ethical approval from a different ethics committee such as the National Research Ethics Service (NRES).

c. If participants are under the responsibility of others (such as parents, teachers or medical staff) how do you intend to obtain permission to approach the participants to take part in the study?
(Please attach approach letters or details of permission procedures – see Section 9 Attachments.)

d. How will participants be recruited (identified and approached)?

e. Describe the process you will use to inform participants about what you are doing.

f. How will you obtain the consent of participants? Will this be written? How will it be made clear to participants that they may withdraw consent to participate at any time? *See the guidelines for information on opt-in and opt-out procedures. Please note that the method of consent should be appropriate to the research and fully explained.*

g. **Studies involving questionnaires:** Will participants be given the option of omitting questions they do not wish to answer?
Yes ☐ No ☐

	If **NO** please explain why below and ensure that you cover any ethical issues arising from this in section 8.
h.	**Studies involving observation:** Confirm whether participants will be asked for their informed consent to be observed. Yes ☐ No ☐
	If **NO** read the guidelines (Ethical Issues section) and explain why below and ensure that you cover any ethical issues arising from this in section 8.
i.	Might participants experience anxiety, discomfort or embarrassment as a result of your study? Yes ☐ No ☐
	If **yes** what steps will you take to explain and minimise this? If **not**, explain how you can be sure that no discomfort or embarrassment will arise?
j.	Will your project involve deliberately misleading participants (deception) in any way? Yes ☐ No ☐
	If **YES** please provide further details below and ensure that you cover any ethical issues arising from this in section 8.
k.	Will you debrief participants at the end of their participation (i.e. give them a brief explanation of the study)? Yes ☐ No ☐
	If **NO** please explain why below and ensure that you cover any ethical issues arising from this in section 8.
l.	Will participants be given information about the findings of your study? (This could be a brief summary of your findings in general; it is not the same as an individual debriefing.) Yes ☐ No ☐
	If **no**, why not?

Section 4 Security-sensitive material Only complete if applicable

Security sensitive research includes: commissioned by the military; commissioned under an EU security call; involves the acquisition of security clearances; concerns terrorist or extreme groups.

a.	Will your project consider or encounter security-sensitive material?	Yes ☐ *	No ☐
b.	Will you be visiting websites associated with extreme or terrorist organisations?	Yes ☐ *	No ☐
c.	Will you be storing or transmitting any materials that could be interpreted as promoting or endorsing terrorist acts?	Yes ☐ *	No ☐

** Give further details in **Section 8 Ethical Issues***

Section 5 Systematic review of research Only complete if applicable

e.	Will you be collecting any new data from participants?	Yes ☐ *	No ☐
f.	Will you be analysing any secondary data?	Yes ☐ *	No ☐

** Give further details in **Section 8 Ethical Issues***
*If your methods do not involve engagement with participants (e.g. systematic review, literature review) **and** if you have answered **No** to both questions, please go to **Section 10 Attachments.***

Section 6 Secondary data analysis Complete for all secondary analysis

a.	Name of dataset/s	The data set does not exist yet, it will be created specifically for this research.	
b.	Owner of dataset/s	The student researcher would own the dataset	
c.	Are the data in the public domain?	Yes ☐	No ☒ ***If no,** do you have the owner's permission/license?* Yes ☒ No* ☐

d.	Are the data anonymised?	Yes ☐	No ☒		
		Do you plan to anonymise the data?	Yes ☒	No* ☐	
		Do you plan to use individual level data?	Yes* ☒	No ☐	
		Will you be linking data to individuals?	Yes* ☒	No ☐	
e.	Are the data sensitive (DPA 1998 definition)?		Yes* ☐	No ☒	
f.	Will you be conducting analysis within the remit it was originally collected for?		Yes ☒	No* ☐	
g.	**If no,** was consent gained from participants for subsequent/future analysis?		Yes ☐	No* ☐	
h.	**If no,** was data collected prior to ethics approval process?		Yes ☐	No* ☐	

** Give further details in **Section 8 Ethical Issues***
*If secondary analysis is only method used **and** no answers with asterisks are ticked, go to **Section 9 Attachments.***

Section 7 Data Storage and Security *Please ensure that you include all hard and electronic data when completing this section.*

a.	Confirm that all personal data will be stored and processed in compliance with the Data Protection Act 1998 (DPA 1998). *(See the Guidelines and the Institute's Data Protection & Records Management Policy for more detail.)*		Yes ☒
b.	Will personal data be processed or be sent outside the European Economic Area?	Yes ☒ *	No ☐

*** If yes,** please confirm that there are adequate levels of protections in compliance with the DPA 1998 and state what these arrangements are below.

The school data will be processed (i.e. digitised and managed) by the student researcher for the sole purpose of this research. The administrative data will be linked by the MoE Studies Centre, which uses a high standard for data confidentiality and security based on the Chilean Protection of Personal Data Act, No 19.628 (link in Spanish only). The student researcher will need to sign a confidentiality agreement for the use of anonymised individual-level data, declaring that the data will be utilised only for the purpose stated, that it will not be shared with third parties and that it will be deleted once it completes its specific use. The Studies Centre will be asked to provide a similar confidentiality agreement to the school, declaring that their student-level data will not be shared with third parties and will be eliminated after linkage with administrative data.

All data will be processed in Chile and analysed in the UK. To ensure data security, the final data set will be stored in the student researcher's personal space from the UCL network (N drive, not the online cloud). Then, the data would not need to be "transported" and it could only be accessed with the corresponding UCL passwords (only known to the researcher).

c. Who will have access to the data and personal information, including advisory/consultation groups and during transcription?

The student researcher will have access to the data from the school, which includes individual level information. The original paper copies of the forms will remain at the school. The dataset will be encrypted with a password to prevent accidental disclosure, and the password will be known to the researcher, the MoE staff liaising with the researcher, and if necessary, the school staff liaising with the researcher.

The data from the school will be shared with the MoE Studies Centre with the purpose of linking these with administrative data at the student level. The MoE will return an anonymised dataset to the researcher.

Only the student researcher will have access to this final dataset, and will be encrypted with a password to prevent accidental disclosure.

During the research

d.	Where will the data be stored? After linking the school and administrative data, the school data set will be returned to the school in an encrypted format and the student researcher will not hold any copies of it. The final linked and anonymised dataset will be stored in the UCL N drive of the researcher, and will hold two copies, one as a security copy and another one for everyday use. The data will be accessible through the UCL desktop machines and through the "Desktop@UCL Anywhere" tool.
e.	Will mobile devices such as USB storage and laptops be used? Yes ☒ * No ☐ *** If yes,** state what mobile devices: When not at UCL facilities, the student researcher will use a personal laptop to access the data only through the "Desktop@UCL Anywhere" tool. No USB storage will be used. *** If yes,** will they be encrypted? Yes, the copies of the data will be encrypted when possible.
After the research	
f.	Where will the data be stored? The data will remain in the UCL N drive.
g.	How long will the data and records be kept for and in what format? The data will be kept by the student researcher as a Stata file until its use is completed.
h.	Will data be archived for use by other researchers? Yes ☐ * No ☒ *** If yes,** please provide details.

Section 8 Ethical issues

Are there particular features of the proposed work which may raise ethical concerns or add to the complexity of ethical decision making? If so, please outline how you will deal with these.

It is important that you demonstrate your awareness of potential risks or harm that may arise as a result of your research. You should then demonstrate that you have considered ways to minimise the likelihood and impact of each potential harm that you have identified. Please be as specific as possible in describing the ethical issues you will have to address. Please consider / address ALL issues that may apply.

Ethical concerns may include, but not be limited to, the following areas:

- Methods
- Sampling
- Recruitment
- Gatekeepers
- Informed consent
- Potentially vulnerable participants
- Safeguarding/child protection
- Sensitive topics
- International research
- Risks to participants and/or researchers
- Confidentiality/Anonymity
- Disclosures/limits to confidentiality
- Data storage and security both during and after the research (including transfer, sharing, encryption, protection)
- Reporting
- Dissemination and use of findings

Data storage and security: A new dataset will be created for this research, combining administrative data and unique data from the school. The latter will be obtained in a non-anonymised format, so it is essential to guarantee to the school that the data will be kept safe and confidential. Measures will be taken to avoid disclosure of the data, such as the use of encrypted drives for storage, and asking the MoE (who will proceed with the data linkage) to delete the original data once linked. The final dataset provided by the MoE will be anonymised and will follow the same cautions to ensure data security.

Confidentiality/Anonymity: The school has authorised the use of the data, which is available in the original paper format. Consequently, the researcher does not have any influence on the content of data to be obtained from the school (i.e. the data will be gathered and digitised, not created). The research does not contemplate to gain consent from the individual students since the data is taken as a secondary source.

If the school wishes to, measures will be taken to maintain the school's anonymity in the research. The student researcher will try to prevent disclosure, for example, by not mentioning explicitly, in the report or to other researchers, the name of the school or the city where is located. However, this may end up being difficult to accomplish completely because of the uniqueness of the school. The student researcher will discuss this issue with the school, explaining ways to will try to avoid the exposure of the school. If necessary, additional measures will be taken until the school is comfortable collaborating in the research.

Dissemination and use of findings: The research results are expected to be disseminated and published, at least in the academic sphere, and possibly to a wider audience as this is a contingent topic for educational policy in Chile. Although measures will be taken to maintain the school's anonymity, the dissemination of this research may, unwantedly, put the school in the public eye. Hence, the findings and dissemination plan will be shared with the school to detect any especial or sensible topic for them.

Section 9 Further information

Outline any other information you feel relevant to this submission, using a separate sheet or attachments if necessary.

Below are the student's responses to the comments and questions from Reviewer 2.

I have some queries about this process. Please respond to each query in detail:

1. Is the information you will take from the admission application only to identify the sample or will you take additional information e.g. about primary school attainment/ family status/parental education/neighbourhood for the purpose of analysis?

I am not aware of the complete contents of those application forms. The main purpose of using these forms is to identify the sample and then be able to link it to administrative data from the MoE. The linkage done by the MoE will provide the additional information about the students (e.g., attainment scores or attendance rates) and their families (e.g., parental education or number of books at home). This additional information comes from MoE records and the national assessment system, which is one of the main sources of data for research in education in Chile. This means that the MoE already provides (in an anonymised format) these data for research purposes.

2. What information will be taken from the admission applications for the purpose of identifying students in the administrative data – how disclosive is this information and how will this be linked? For example are you linking on Date of Birth/ surname? Please give more specifics.

In order to identify the sample, I ultimately need only two pieces of information from the school: who applied, and who got in. The basic data that is required from the application forms is the national ID number of each student. Additionally, the school can complement the list of applicants by identifying their current students. No other information that could be used to potentially identify students will be taken from the admission applications. To avoid data mismatch, an extra validation exercise can be executed using only non-sensitive information from the application forms, such as the date of application.

The national ID number will suffice for the MoE to link the data. The final dataset will include a "masked ID number", which is the anonymised format the MoE provides for all data. After the MoE delivers the final dataset, the data from the application forms -including the ID number- will be returned to the school and I will not hold any copy.

3. Are you planning on sending an encrypted file and a separate key for the purpose of linking administrative data?

The encrypted file that will be given to the MoE for linking administrative data will have just a few columns: the national ID number, a dummy (0-1) of whether the student was

admitted to the school, and one or two other variables with non-sensitive information. Depending on the content of the admission forms, these two variables will be selected with the sole criterion of providing trivial information that could not be used to identify individual students. The columns can be named ID, var1, var2, etc., this way, the file by its own will not have substantial meaning.

Additionally, I will provide a list to the MoE including all data to be linked.

4. Once you have the linked data, how will you transfer it to the UK for analysis? Will it be on an encrypted USB key? Please clarify.

Once I get the final anonymised data set from the MoE, I will save it in my UCL N drive, which I will access remotely through the "Desktop@UCL Anywhere" tool. This method will allow me to access and work with the data safely and efficiently and will minimise risks of accidental security breaches or loss.

5. You have said that the school will give consent for the use of this data, however can they really do this for their current students - whose applications were accepted - and the students who were not accepted? It seems strange that they are allowed to keep the details of unsuccessful applicants on file in perpetuity? Please give more information

I proposed to the school to conduct this research more than a year ago, in which time I also mentioned the fact that accessing the admission forms was key. Hence, as far as I understand, the school is saving these data for the purpose of this study. Additionally, the school is also aware that the data would be linked and anonymised by the MoE.

6. In section 6f I think you have answered this question incorrectly you have answered 'yes' to the question 'Will you be conducting analysis within the remit it was originally collected for?' and given that are you using the application forms to identify the sample I don't think you are using the data for the same purpose. I think you need to provide more information about the issue of consent.

The final data set will be created (linked) for the sole purpose of this study and this information would not exist otherwise. Moreover, I do not have any effect on the content of these data as it was collected by the school and the MoE several months ago. The data from the applications forms is understood to be owned by the school, and they have agreed to the terms of the research (provided the confidentiality measures mentioned above apply). On the other side, the MoE freely provides these type of data for research purposes (provided the researcher accepts and complies with their confidentially standards). Hence, the anonymised data set, with which the analysis will be conducted, is assumed to contain secondary data for which there is no need to request for consent beyond its owners.

Section 10 Attachments Please attach the following items to this form, or explain if not attached

k.	Information sheets and other materials to be used to inform potential participants about the research, including approach letters	Yes ☐	No ☐
l.	Consent form	Yes ☐	No ☐
	If applicable:		
m.	The proposal for the project	Yes ☐	No ☐
n.	Approval letter from external Research Ethics Committee	Yes ☐	No ☐
o.	Full risk assessment	Yes ☐	No ☐

Section 11 Declaration

	Yes	No
I have read, understood and will abide by the following set of guidelines.	☒	☐

BPS ☐	BERA ☒	BSA ☐	Other (please state) ☒

			Studies Centre - Ministry of Education, based on the Chilean Protection of Personal Data Act, No 19.628

I have discussed the ethical issues relating to my research with my supervisor. ☒ ☐
I have attended the appropriate ethics training provided by my course. ☒ ☐

I confirm that to the best of my knowledge:
The above information is correct and that this is a full description of the ethics issues that may arise in the course of this project.

Name	Constanza Gonzalez Parrao
Date	31 Jul 2017

Notes and references

Professional code of ethics
You should read and understand relevant ethics guidelines, for example:
British Psychological Society (2009) *Code of Ethics and Conduct*, and (2014) *Code of Human Research Ethics,* or British Educational Research Association (2011) *Ethical Guidelines, or* British Sociological Association (2002) *Statement of Ethical Practice*

Disclosure and Barring Service checks
If you are planning to carry out research in regulated Education environments such as Schools, or if your research will bring you into contact with children and young people (under the age of 18), you will need to have a Disclosure and Barring Service (DBS) CHECK, before you start. The DBS was previously known as the Criminal Records Bureau (CRB). If you do not already hold a current DBS check, and have not registered with the DBS update service, you will need to obtain one through UCL.
Ensure that you apply for the DBS check in plenty of time as will take around 4 weeks, though can take longer depending on the circumstances.

Further references
The www.ethicsguidebook.ac.uk website is very useful for assisting you to think through the ethical issues arising from your project.

- Robson, Colin (2011). *Real world research: a resource for social scientists and practitioner researchers* (3rd edition). Oxford: Blackwell. This text has a helpful section on ethical considerations.
- Alderson, P. and Morrow, V. (2011) *The Ethics of Research with Children and Young People: A Practical Handbook.* London: Sage. This text has useful suggestions if you are conducting research with children and young people.
- Wiles, R. (2013) What are Qualitative Research Ethics? Bloomsbury. A useful and short text covering areas including informed consent, approaches to research ethics including examples of ethical dilemmas.

Departmental use

If a project raises particularly challenging ethics issues, or a more detailed review would be appropriate, you may refer the application to the Research Ethics and Governance Administrator (via IOE.researchethics@ucl.ac.uk) so that it can be submitted to the Research Ethics Committee for consideration. A Research Ethics Committee Chair, ethics representatives in your department and the research ethics coordinator can advise you, either to support your review process, or help decide whether an application should be referred to the Research Ethics Committee.

Reviewer 1

Supervisor name	Gill Wyness

Supervisor comments	This project poses low to no risk to human individuals, as the data are secondary data, and have been processed by the Chilean ministry of education. All data will be presented at aggregate level and there is no means for an individual to be identified in any published format. In addition, information on the individuals contained comes from widely used secondary data which is publicly available. Constanza has considered the ethical implications of the study by conducting rigorous, transparent methods in her research, and is in contact with the school to respond to any queries they may have.	
Supervisor signature	[*signature omitted deliberately*]	
Reviewer 2		
Advisory committee/course team member name	Morag Henderson	
Advisory committee/course team member comments	Having raised some initial questions regarding the original application I am satisfied with the subsequent response provided by Constanza Gonzalez Parrao. The detailed response that Constanza drafted about this issues raised satisfied my concerns about the storage, providence and use of the data.	
Advisory committee/course team member signature	[*signature omitted deliberately*]	
Decision		
Date decision was made	01 June 2018	
Decision	Approved	☒
	Referred back to applicant and supervisor	☐
	Referred to REC for review	☐
Recording	Recorded in the student information system	☐

Once completed and approved, please send this form and associated documents to the relevant programme administrator to record on the student information system and to securely store.
Further guidance on ethical issues can be found on the IOE website at http://www.ucl.ac.uk/srs/research-ethics-committee/ioe and www.ethicsguidebook.ac.uk

8. Appendix Chapter 2

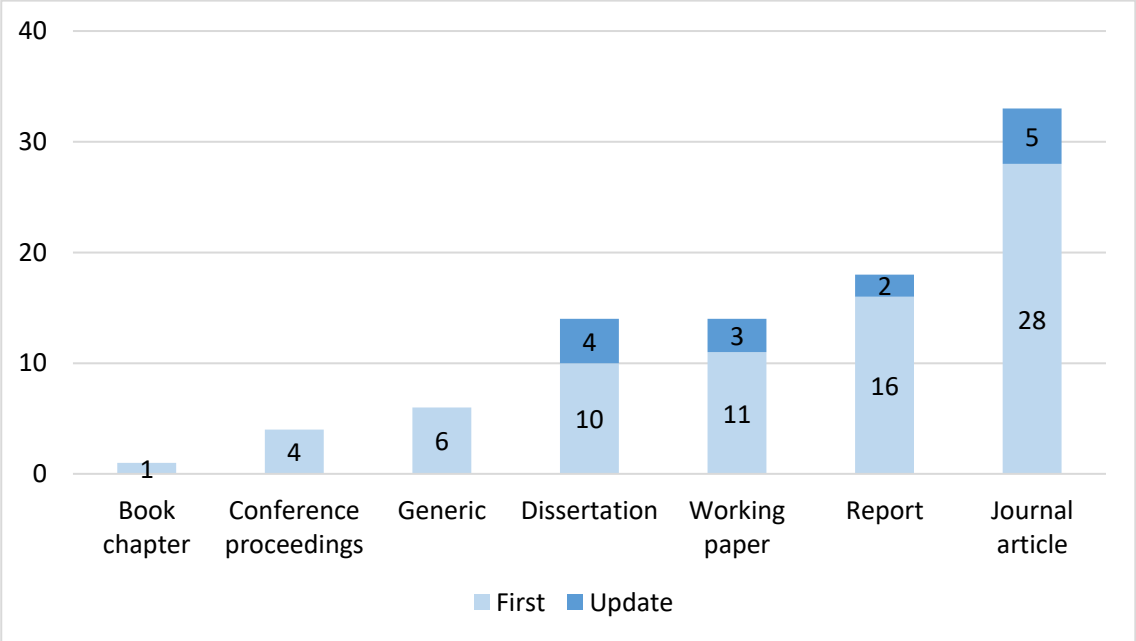
8.1. Updating the literature search

A second search was conducted in October 2018, replicating all the steps from the original search. This search process yielded a similar percentage of potentially included studies relative to the first search. As shown in *Table 8.1*, 58% of the references included based on title and abstract were finally included on full text, whereas in the original search 55% of these records were finally included in the review. Overall, 16% of the total references included after the screening by full text were identified in the update search, of which most are published records, particularly journal articles (see *Figure 8.1* below). Hence, the update search retrieved a non-trivial portion of relevant literature for the review.

Table 8.1 Percentage of included references by search stage

	Screened by T&A	Included on T&A	Included on full text
First search	6,493	138 (2%)	76 (55%)
Update search	615	24 (4%)	14 (58%)
Total	7,108	162 (2%)	90 (56%)

Figure 8.1 References included on full text, by publication type (total = 90)



8.2. Search strategy results

The following tables describe the results yielded in each search phase, including the strategy used, the search date and the total number of references retrieved per information source. They also include the cases and rationale for adapting the original search strategy and the detailed search procedure for two databases with retrieving restrictions (Google Scholar and JSTOR). *Table 8.2* details the first search and *Table 8.3* reports the update search results.

Table 8.2 Strategies and results per information source – First search

Strategy		Database	Search date (dd.mm.yy)	Total results
#1	(random* NEAR/3 admission* OR lotter*) AND (school* OR student*) AND (evaluation OR effect OR impact OR gain)	ERIC (ProQuest)	01.12.16	217
		Australian Education Index (ProQuest)	01.12.16	7
		PRISMA Database (ProQuest)	01.12.16	90
		India Database (ProQuest)	01.12.16	31
		Australia and New Zealand Database (ProQuest)	01.12.16	175
		East and South Asia Database (ProQuest)	01.12.16	95
		East Europe, Central Europe Database (ProQuest)	01.12.16	185
		Middle East and Africa Database (ProQuest)	01.12.16	221
		UK and Ireland Database (ProQuest)	01.12.16	298
		Web of Science	05.12.16	146
#2	(random* NEAR/3 admission* OR lotter* NEAR/3 admission*) AND (school* OR student*) AND (evaluation OR effect OR impact OR gain) [1]	Social Science Database (ProQuest)	01.12.16	123
		Education Database (ProQuest)	01.12.16	218
		Dissertations and Theses (ProQuest)	01.12.16	817
	(random N3 admission OR lottery N3 admission) AND (school OR student) AND (evaluation OR effect OR impact OR gain) [2]	British Education Index (EBSCO)	01.12.16	15
		American Doctoral Dissertations (EBSCO)	01.12.16	11
		Education Abstracts (EBSCO)	01.12.16	10
#3	(random* OR lotter*) AND admission* AND (school* OR student*) AND (evaluation OR effect OR impact OR gain) [3]	German Education Portal	01.12.16	4
		SciELO (English search)	02.12.16	7
#4	admission AND (school OR student) AND (evaluation OR effect OR impact OR gain) [4]	African Journals Online	01.12.16	1
		OpenGrey	02.12.16	9
	(admission AND school and effect)	EThOS	02.12.16	36
		Institute of Education Sciences	02.12.16	2
	(school AND effect)	Campbell Collaboration	05.12.16	34
#5	(aleatori* OR loteria) AND (admision* OR ingres*) AND (escuela* OR estudiante*) AND (evalua* OR effecto* OR impacto) [5]	SciELO (Spanish search)	02.12.16	7

Strategy		Database	Search date (dd.mm.yy)	Total results
#6	*[with all the words:]* (random admission school) AND *[with at least one of the words:]* (evaluation effect impact gain)	Google Scholar [6] (English search)	07.12.16	135
		135 potentially relevant references were retrieved from results 1-999 (13.5%) 80 potentially relevant references were identified from results 1-300 (27%) 37 potentially relevant references were identified from results 301-600 (12%) 18 potentially relevant references were identified from results 601-999 (4.5%) Total search results = 56,000		
	[with all the words:] (aleatorio admision escuela) AND [with at least one of the words:] (evaluacion efecto impacto)	Google Scholar (Spanish search)	11.01.17	134
		134 potentially relevant references were retrieved from results 1-999 (13.4%) 101 potentially relevant references were identified from results 1-300 (34%) 33 potentially relevant references were identified from results 301-600 (11%) 6 potentially relevant references were identified from results 601-999 (1.5%) 6 duplicated references were eliminated Total search results = 13,300		
#7	("random* admission"~3 OR lotter*) AND (school* OR student*) AND (evaluation OR effect OR impact OR gain) [+ years bounds + discipline + English]	JSTOR [7]	06.12.16	4,080
		731 references were retrieved in search 1970-1999 + Education + English 705 references were retrieved in search 2000-2016 + Education + English 179 references were retrieved in search 1970-2016 + Statistics + English 666 references were retrieved in search 1970-1995 + Economics + English 617 references were retrieved in search 1996-2003 + Economics + English 663 references were retrieved in search 2004-2009 + Economics + English 519 references were retrieved in search 2010-2016 + Economics + English		

Total references obtained through database searching	7,108
Total references scanned in Google Scholar	1,723
Total references obtained from other sources	36
Total references considered	8,867

Notes: [1] Variant of *Strategy 1* with a more narrowed concept of "lottery" to restrict results.
[2] Variant of *Strategy 2* for EBSCO databases, which gave more results than *Strategy 1*.
[3] Variant of *Strategy 1* for databases without a proximity function available.
[4] Variants of *Strategy 1* in its simplest versions to pick up results in these databases.
[5] Spanish variant of *Strategy 3* for searching on Spanish SciELO database.
[6] Google Scholar (GS) only shows the first 999 results and allows only 500 references to be saved in the GS library. More importantly, GS interprets saving all the results per page as an automated request and restricts the access to the following pages. Hence, once sorted by relevance, all 999 results were screened and only those potentially relevant for the review (based on title and description shown) were saved in the GS library.
[7] JSTOR does not show more than 1,000 results for any search. To ensure less than 1,000 results, the search was divided into disciplines and time bounds, and the language was restricted to English.

Table 8.3 Strategies and results per information source – Update search

Strategy [1]		Database	Search date (dd.mm.yy)	Total results
#1	(random* NEAR/3 admission* OR lotter*) AND (school* OR student*) AND (evaluation OR effect OR impact OR gain)	ERIC (ProQuest)	24.10.18	28
		Australian Education Index (ProQuest)	24.10.18	0
		PRISMA Database (ProQuest)	24.10.18	26
		India Database (ProQuest)	24.10.18	30
		Australia and New Zealand Database (ProQuest)	24.10.18	17
		East and South Asia Database (ProQuest)	24.10.18	30
		East Europe, Central Europe Database (ProQuest)	24.10.18	42
		Middle East and Africa Database (ProQuest)	24.10.18	46
		UK and Ireland Database (ProQuest)	24.10.18	44
		Web of Science	24.10.18	38
#2	(random* NEAR/3 admission* OR lotter* NEAR/3 admission*) AND (school* OR student*) AND (evaluation OR effect OR impact OR gain)	Social Science Database (ProQuest)	24.10.18	19
		Education Database (ProQuest)	24.10.18	20
		Dissertations and Theses (ProQuest)	24.10.18	112
	(random N3 admission OR lottery N3 admission) AND (school OR student) AND (evaluation OR effect OR impact OR gain)	British Education Index (EBSCO)	24.10.18	1
		American Doctoral Dissertations (EBSCO)	24.10.18	3
		Education Abstracts (EBSCO)	24.10.18	3
#3	(random* OR lotter*) AND admission* AND (school* OR student*) AND (evaluation OR effect OR impact OR gain)	German Education Portal	24.10.18	0
		SciELO (English search)	24.10.18	1
#4	admission AND (school OR student) AND (evaluation OR effect OR impact OR gain)	African Journals Online	24.10.18	2
		OpenGrey	24.10.18	0
	(admission AND school and effect)	EThOS	24.10.18	7
		Institute of Education Sciences	24.10.18	0
	(school AND effect)	Campbell Collaboration	24.10.18	14
#5	(aleatori* OR loteria) AND (admision* OR ingres*) AND (escuela* OR estudiante*) AND (evalua* OR effecto* OR impacto)	SciELO (Spanish search)	24.10.18	4

Strategy	Database	Search date (dd.mm.yy)	Total results
#6 *[with all the words:]* (random admission school) AND *[with at least one of the words:]* (evaluation effect impact gain)	Google Scholar [2] (English search)	24.10.18	99
	99 potentially relevant references were retrieved from results 1-740 results (13%) 79 potentially relevant references were identified from results 1-300 (26%) 20 potentially relevant references were identified from results 301-600 (7%) 2 potentially relevant references were identified from results 601-740 (1%) 2 duplicated references were eliminated Total search results = 17,300		
[with all the words:] (aleatorio admision escuela) AND [with at least one of the words:] (evaluacion efecto impacto)	Google Scholar (Spanish search)	26.10.18	5
	5 potentially relevant references were retrieved from results 1-170 (3%) Total search results = 2,860		

Strategy	Database	Search date (dd.mm.yy)	Total results
#7 ("random* admission"~3 OR lotter*) AND (school* OR student*) AND (evaluation OR effect OR impact OR gain) [+ years bounds + discipline + English]	JSTOR [3]	26.10.18	65
	22 references were retrieved in search 2017-2018 + Education + English 1 reference was retrieved in search 2017-2018 + Statistics + English 42 references were retrieved in search 2017-2018 + Economics + English		
	Total references obtained through database searching		656
	Total references scanned in Google Scholar		804
	Total references obtained from other sources		0
	Total references considered		1,460

Notes: [1] In this update stage, all search strategies were replicated from the original search except for a time limit: searches were conducted for the years 2017 and 2018.
[2] The screening in GS stopped if more than 100 consecutive results yielded no relevant references.
[3] No year restrictions were necessary for the search.

8.3. Data extraction tool

The following template was used to code each primary study. After completing the studies coding for a third of the references, the extraction tool was revised and fine-tuned with minor edits:

- In the Waiting list section, we added a new option “There is a waiting list” without specifying how the list was drawn.
- In the Intervention section, we added the options “School policy” and “Educational system policy” to specify the policy level in cases where the randomisation level was unknown.
- In the Methods section, we replaced the option “Experimental” with other, more specific options to reflect better the methods used to calculate the (quasi)experimental estimates.
- In the Methods section, we specified that for studies applying several analysis strategies, the coding should follow the strategy used to estimate the effect sizes extracted for the review.
- In the Outcomes section, we made explicit the distinction between outcomes and mechanisms to explain the results. These “potential mechanisms”, explored commonly at the school level, are not outcomes of the Intervention.
- In the Outcomes section, we added a new subsection as a checkbox to include any additional information that is potentially relevant but that does not fit elsewhere in the coding tool.

<u>Data extraction tool</u>

1. General information

This section covers administrative details of the study.

1.1. Authors' affiliation

If there is more than one author, select all options that apply and specify the name of each institution.

- University
- Research centre
- Government agency
- Other (specify)

1.2. Year of publication

- 1970
- 1971
- …
- 2017
- 2018

1.3. Publication type
- Journal article
- Book chapter
- Report
- Working paper
- Dissertation
- Conference proceedings
- Generic

1.4. Retrieval source
- Retrieved from the original search
- Retrieved from the update search
- Retrieved from other sources

2. Aim and sample

This section covers the objective of the study and provides details of the sample analysed.

2.1. Does the programme or school have a specific name?
- Yes: Specify name of programme/school.
- No: Specify details or description of programme/school.

2.2. Is the research question/aim of the study explicit and clear?
- Yes: Add the textual copy of RQ/aim and page number for reference.
- No: Add the closest reference to a RQ/aim and the page number for reference.

2.3. In which country is the study conducted?
- China
- Netherlands
- South Korea
- UK
- Uruguay
- US

2.4. In which city/state is the study conducted? US studies only.

If a study is conducted in multiple cities/states, select all that apply AND the option Multi-state. If the report does not specify the location of the sample, select the option Unknown. Add a new option (add child code, code type: selectable) if a city is not yet listed.
- Boston: city in the state of Massachusetts
- Chicago: city in the state of Illinois
- Connecticut: state
- Denver: city in the state of Colorado
- Illinois: state
- Los Angeles: city in the state of California
- Massachusetts: state
- Michigan: state
- New York City: city in the state of New York
- North Carolina: state
- Pennsylvania: state
- Portland: city in the state of Oregon
- San Diego: city in the state of California
- Virginia: state
- Washington, DC: state
- Multi-state: Unknown

- Houston: city in the state of Texas
- Newark: city in the state of New Jersey
- Philadelphia: city in the state of Pennsylvania

2.5. School characteristics

2.5.1. School type

Select one of the options marked with an * and all other options that apply. Add a brief description of the school type and page number for reference. Add a new option (add child code, code type: selectable) if a distinctive school type is not yet listed.

- * Public school: These are schools publicly funded (by the government) and run by a public local authority.
- * Private school: These are schools privately funded (families pay a fee) and privately run by a foundation, NGO, non-for-profit school network, for-profit school network, etc.
- * Charter/Academy school: These are schools publicly funded (government) and privately run by a foundation, NGO, non-for-profit school network, for-profit school network, etc.
- Magnet school: These are public schools with specialised curricula.
- Career magnet/academy: These are US vocational schools.
- Early college high school: These are US schools allowing students to earn college credits.
- Small schools of choice (SSC): These are smaller US public schools.
- Montessori school: These are schools based on the Montessori method: self-directed activity, hands-on learning and collaborative play".
- KIPP public charter school: These are schools run by the non-profit school network "Knowledge is Power Program".
- SEED public charter school: These are US boarding schools.
- No Excuses charter school: These are schools with very high academic and behavioural expectations from students (their background is "no excuse" for a poor performance).
- Success Academy charter school: These are US schools run by the Success Academy schools network.
- Promise Academy charter school: These are US "No Excuses" schools.
- MATCH charter school: This is a US school.
- National Heritage Academy: These are schools run by the NHA for-profit charter schools network.
- Expeditionary Learning Schools: These are schools based on the Expeditionary Learning approach: Mastery of Knowledge and Skills, Character, and High-Quality Student Work.
- Unclear / Not reported (specify)
- Building 21 in-district charter school: collaboration between school district and other private/NGO entities. It's within the school district but follows other rules, has greater autonomy.

2.5.2. School grades

The ages described for each level are only for reference; priority should be given to the study description. Select all that apply and specify details and page number for reference.

- Elementary/Primary school: Students aged 6-7 to 12 years old.
- Secondary/High school: Students aged 13 to 18 years old.
- Unclear / Not reported (specify)

2.5.3. School context

Specify details and add page number for reference.

- Only urban
- Only rural
- Both urban and rural
- Unclear / Not reported (specify)

2.5.4. Admission process

(A) Purpose of using lotteries

Is the study explicit about why the admission process of the school (or the school network, or the school system) is based on a random assignment of students? Select all that apply. For each of the options, specify details of why they use lottery admissions and add page number for reference.

- Yes, they are required to: Using lotteries in the admission process is required by the local/national education act.
- Yes, it relates to its evaluation: Using lotteries in the admission process facilitates the impact evaluation of the school program or school system.
- Yes, it relates to fairness: Using lotteries in the admission process gives an equal chance of studying in the school to all applicants.
- Yes, it relates to something else
- No
- Unclear / Not reported (specify)

(B) Priority admission criteria

Before the lottery draw, are school places filled due to any of these criteria? Specify details and add page number for reference.

- Siblings: Applicants have priority admission if a younger/older sibling already studies in the school.
- Residential: Applicants have priority admission if they live in the neighbourhood surrounding the school. This “proximity” rule may change for each study.
- School staff: Applicants have priority admission if a parent is part of the school staff.
- Special education needs: Applicants have priority admission if they have a special education need.
- Other (specify)
- No, there is no priority admission
- Unclear / Not reported (specify)

(C) Oversubscription

Are there more applicants than places available at the school? Specify details and add page number for reference.

- Oversubscribed at every level: All school grades evaluated were oversubscribed and this is explicitly mentioned in the study.
- Oversubscribed not at every level: Not every school grade evaluated was oversubscribed and this is explicitly mentioned in the study. Specify which levels were oversubscribed.
- Not oversubscribed: None of the school grades evaluated was oversubscribed and this is explicitly mentioned in the study. If available, add any reasons/observations mentioned in the study for this.
- Unclear / Not reported (specify)

(D) Waiting list

Is there a waiting list and how is it created? In case that a lottery "winner" does not take up the school place, applicants in the waiting list may be offered that school place. Specify details on how the list is created/managed and add page number for reference.

- Waiting list is randomly drawn
- Waiting list is not randomly drawn
- There is no waiting list
- Unclear / Not reported (specify)
- There is a waiting list: how it is drawn is UNCLEAR

2.5.5. Other distinctive characteristics

Is there anything else about the school that is relevant for understanding the study? If so, add details and page numbers. If all main features have been included, select NO.

- Yes (specify)
- No

2.6. Students' characteristics

Does the sample evaluated include students with any of these characteristics?

2.6.1. Does the sample include female and/or male students?

Specify details and add page number for reference.

- Only female
- Only male
- Both female and male
- Unclear / Not reported (specify)

2.6.2. Does the sample include socioeconomically disadvantaged groups?

Add a comment with the definition/description of this group according to the study and page number for reference.

- Yes
- No
- Unclear / Not reported (specify)

2.6.3. Does the sample include minority ethnic groups?

Generally, these would be any non-white ethnic group, but priority should be given to the study description. Add a comment with the group description and page number for reference.

- Yes
- No
- Unclear / Not reported (specify)

2.6.4. Does the sample include students with special education needs?

Add a comment with the definition/description of this group according to the study and page number for reference.

- Yes
- No
- Unclear / Not reported (specify)

2.6.5. Other distinctive characteristics

Is there anything else about the students in the sample that is relevant for understanding the study? If so, add details and page numbers. If all main features have been included in the points above, select NO.

- Yes (specify)
- No

3. Evaluation

This section provides details of the data and methods used in the analyses and its results.

3.1. Data source

3.1.1. Data collection

How was the data gathered for this study? If the study involves both primary and secondary data, select both options. Specify details and add page number for reference.

- Primary source: The data used in the analyses was created/collected for the purpose of this study.
- Secondary source: The data used in the analyses are of public access or are accessible to the researchers. The authors did not collect the data.
- Unclear / Not reported (specify)

3.1.2. Sample scope

Given the research question/aim (see Section 2.2), did the study include in the original sample all possible schools/students available to evaluate? If they excluded one or more schools/ students, why was this the case? The assumption is that the authors will not exclude from the sample any school/student a priori (before the data collection). For example, the authors are interested in a school network but may decide to exclude one of the schools that should have been part of the research sample because it is a school too far away from the rest. Specify details and add page number for reference.

- Yes: The authors included all possible schools/students aimed to be studied.
- No: The authors excluded some schools/students aimed to be studied. Specify how many schools/students were excluded from the original sample and any reasons for this decision.
- Unclear / Not reported (specify)

3.1.3. Measurement instruments

Did the authors create new instruments for the purpose of this study? Only referred to outcome measures. If the researchers also conducted surveys/interviews to contextualise their study/results, do not account this as outcome instruments.

- Yes: The authors created a new instrument for the analyses (or more than one), which had not been used before. Specify the instruments they created. Specify if the instrument was piloted before the study.
- No: The authors used already existing instruments in the analyses (e.g. a state or national standardised test). Specify the instruments they used.
- Unclear / Not reported (specify)

3.2. Intervention

The list of options below aims to capture simultaneously two aspects of the intervention: the randomisation level (Did the admission process randomise all school places?) and policy level (Is the lottery an admission process only for this school or is it part of a broader school system policy?).

It is virtually impossible that a study does not specify or imply the policy level. In the unlikely event that a study does not specify the randomisation level, then add a new option (add child code, code type: selectable) to specify only the policy level. Specify details and add page number for reference.

- School policy - All places
  - The admission process of the school (or a small group of schools of the same type) is

based on the random allocation of students, and it is not part of the traditional school application process in the city/ state.
- The admission process randomly allocated all places available in the school(s).

- School policy - All places after priority
  - The admission process of the school (or a small group of schools of the same type) is based on the random allocation of students, and it is not part of the traditional school application process in the city/ state.
  - After assigning priority admission to certain students (see Section 2.5.4.B), the admission process randomly allocated all other places available in the school(s).
  Specify the percentage of randomly allocated school places.
- School policy - A percentage of places
  - The admission process of the school (or a small group of schools of the same type) is based on the random allocation of students, and it is not part of the traditional school application process in the city/ state.
  - The admission process randomly allocated a percentage of the school places available (besides priority admission) and the rest is decided by the school based on their own criteria.
  Specify the percentage of randomly allocated school places.
  Specify the (non-random) selection criteria used (admission tests, prior grades, etc.).
- Educational system policy - All places
  - The admission process of schools in the city/state is based on the random allocation of students, commonly after families have applied to several schools and ranked their preferred choices. Normally, a local authority, rather than a school, will administer this admission process.
  - The admission process randomly allocated all places available in the school(s).
- Educational system policy - All places after priority
  - The admission process of schools in the city/state is based on the random allocation of students, commonly after families have applied to several schools and ranked their preferred choices. Normally, a local authority, rather than a school, will administer this admission process.
  - After assigning priority admission to certain students (see Section 2.5.4.B), the admission process randomly allocated all other places available in the school(s).
  Specify the percentage of randomly allocated school places.
- Educational system policy - A percentage of places
  - The admission process of schools in the city/state is based on the random allocation of students, commonly after families have applied to several schools and ranked their preferred choices. Normally, a local authority, rather than a school, will administer this admission process.
  - The admission process randomly allocated a percentage of the school places available (besides priority admission) and the rest is decided by the school based on their own criteria.
  Specify the percentage of randomly allocated school places.
  Specify the (non-random) selection criteria used (admission tests, prior grades, etc.).
- Unclear / Not reported (specify)
- Educational system policy: the randomisation level is UNCLEAR
- School policy: the randomisation level is UNCLEAR

3.3. Comparison groups

Which are the groups that the study is comparing? Select all that apply, specify detail and add page number for reference.

- Areas: Other similar larger areas using alternative types of school admission processes. These could be school districts, states, councils or similar. This would apply when an entire area (instead of one school) uses randomised admissions.

- Schools: Other schools in similar areas using an alternative type of admission process. This would apply when a study is focused on a particular area and compares schools within such an area.
- Students: Students that applied to a school with random admission but were not assigned a place. This would apply when a study focuses on schools and compares students who applied to those schools.
- Unclear / Not reported (specify)

3.4. Period of evaluation

3.4.1. Ages evaluated

What age are the students evaluate in the study? Different countries have different school grades, so for comparability purposes it is better to use the age of the students in the sample. Select all that apply and specify details and page number for reference.

- 5 years old
- 6 years old
- …
- 17 years old
- 18 years old
- Unclear / Not reported (specify)

3.4.2. Follow-up evaluations

Does the study have follow-up evaluations? These are when the same sample of schools/ students continues to be observed, and outcomes (the original or other) are measured over a longer period of time. An example of a follow-up evaluation could be measuring high school outcomes for students that were originally measured in primary school. If the study has any linked records, select all that apply, specify details (such as how long after the first measure is this follow-up evaluation) and add a page number for reference. Add a new option (add child code, code type: selectable) if necessary.

- First measure: This is the original analysis of the sample.
- First follow-up: The first follow-up would be the second measure in time for the same sample.
- Second follow-up: The second follow-up would be the third measure in time for the same sample.
- Unclear / Not reported (specify)

3.5. Compliance

3.5.1. School level

Did all schools stay in their assigned group? This applies when the comparison is done between schools, and some manage to change groups.

- Yes
- No: Specify the number of non-compliant schools, reasons for non-compliance and other details, along with the page number for reference.
- Unclear / Not reported (specify)

3.5.2. Student level

Did all students stay in their assigned group? This applies when the comparison is done between students, and some manage to change groups.

- Yes
- No: Specify the number of non-compliant students, reasons for non-compliance and other details, along with the page number for reference.
- Unclear / Not reported (specify)

3.5.3. Compliance concerns

Does the study report concerns about compliance issues? If YES, did the authors take appropriate measures to control for this? Specify details and add page number for reference.

- Yes
- No
- Unclear / Not reported (specify)

3.6. Attrition

3.6.1. School level

Did schools drop out of the study? This applies when a school considered to be part of the original sample is not part of the analyses. This could be due to, for example, such school deciding to stop being part of the study, or there is missing data for such school. Particularly when selecting “Yes”, specify details and add page number for reference.

- Yes: Specify percentage of attrition, reasons for dropping out, and other details, along with the page number for reference.
- No
- Unclear / Not reported (specify)

3.6.2. Student level

Did students drop out of the study? This applies when a student considered to be part of the original sample is not part of the analyses. This is generally due to missing data for such student (because she left the school/district, there is no baseline/outcome data for her, etc.). Particularly when selecting “Yes”, specify details and add page number for reference.

- Yes: Specify percentage of attrition, reasons for dropping out, and other details, along with the page number for reference.
- No
- Unclear / Not reported (specify)

3.6.3. Attrition concerns

Does the study report concerns about attrition issues? For example, are those who dropped out of the study (at any level) different from those who stayed? Did the authors take appropriate measures to control for this? Specify details and add page number for reference.

- Yes
- No
- Unclear / Not reported (specify)

3.7. Sample size

3.7.1. Total number of participant schools

Select, specify the total figure (original sample) and add page number for reference. If the total number of participants is unclear or unreported, register "-99" in the Info box.

3.7.2. Total number of participant students

Select, specify the total figure (original sample) and add page number for reference. If the total number of participants is unclear or unreported, register “-99” in the Info box.

3.7.3. Original vs analysis sample

Is the analysis sample different from the original sample? Besides potential issues of compliance and attrition, are there other reasons why the sample of schools/students

analysed is different from the original (planned) sample. For example, the authors may decide to exclude part of the sample (say, one school) because the admission lotteries were run differently, creating comparability issues.

- Yes
- No
- Unclear / Not reported (specify)

3.8. Data analysis

3.8.1. Methods

Which methods does the study apply to analyse the data? If the study applies different analysis strategies, focus ONLY on the strategy used for estimating the extracted outcomes/effect sizes. Add a new option (add child code, code type: selectable) if an analysis method is not yet listed.

- Experimental: Treatment vs control comparison
- Before/after: Difference in Difference evaluation
- Matching
- Instrumental variable
- Unclear / Not reported (specify)
- Non-experimental: Comparison of different treatment/control groups, where there are also other not-randomised differences between them (example, Knechtel 2017).
- Chi-Sq: Chi-square is used to determine whether there is a relationship between two categorical variables (Field, 2009). [NIKOLOV p.48]
- MANOVA: Multivariate Analysis of Variance (MANOVA) is used to analyze "two groups of subjects on several dependent variables simultaneously; focusing on cases where the variables considered together make sense as a group" [NIKOLOV, p.49]
- Value-added
- Fixed-effects
- First Differences
- Regression Discontinuity
- Weighting
- Linear regression models
- Treatment effect bounds: GMM estimator
- T-test: testing if there is a significant difference between the means of two groups

3.8.2. Analysis strategy

What is the focus of the analysis in the study? Specify details and add page number for reference.

- Intention-To-Treat: Compare those assigned to treatment/control groups, disregarding any potential issues of compliance.
- Treatment-On-Treated: Compare those who were actually in the treatment/control groups, disregarding any potential issues of attrition.
- None: The data is analysed in another way
- Unclear / Not reported (specify)

3.8.3. Baseline measures

Does the study report if the treatment and control groups were balanced before the admission randomisation? Specify details and add page number for reference.

- Yes, groups were balanced: There are no statistical differences between treatment and control groups before the school places were randomised.

- Yes, groups were not balanced: There are statistical differences in at least one characteristic between treatment and control groups before the school places were randomised.
- Not reported: The study does not report baseline measures. We do not know if these groups were balanced.

3.8.4. Sub-group analyses
Does the study report sub-group analyses? Specify details and add page number for reference. Add a new option (add child code, code type: selectable) if a sub-group is not yet listed.
- Yes, by gender
- Yes, by age
- Yes, by ses: any measure of socioeconomic (dis)advantage
- Yes, by race: race, ethnic minority
- Yes, by urban
- No
- Unclear / Not reported (specify)
- Yes, by grades
- Yes, by school type
- Yes, 1st gen college: Student who are the first in their families to attend postsecondary education
- Yes, by academic: Any academic criteria, such as previous academic performance, gifted students, non-prepared students, etc.

3.8.5. Validity

(A) Validity analyses
Does the study provide validity analyses, such as sensitivity analyses, robustness checks, etc.? Specify details and add page number for reference.
- Yes
- No
- Unclear / Not reported (specify)

(B) Validity concerns
Does the study report concerns about the validity of the analyses? Specify details and add page number for reference.
- Yes
- No
- Unclear / Not reported (specify)

3.8.6. Covariates
Select all covariates/controls included in the models. Add a new option (add child code, code type: selectable) if a variable is not yet listed.
- Gender
- Age
- Race/Ethnicity
- Primary language: any indicator of the language (other than English) that is the primary language of the student. It could be measured as: Language spoken at home, limited English proficiency, etc.
- Adults in HH: continuous (number of adults in household) or binary (is there at least one adult in household)

- HH income: household or family income. It could be measured in categories (between £100-£500) or continuously
- Mother's education
- Father's education
- Risk sets: Group of schools each student/family applied to. The unique combination of lotteries to which each student applied. Could also be mentioned as lottery fixed effects
- Access to computer at home
- Access to internet at home
- Number of books at home: Could be either books in general or children's books
- None
- School fixed effects
- Special education
- Free school meal: or any equivalent indicator
- Prior academic achievement
- Poverty (neighbourhood): Poverty measure at a greater level than the household, usually at the neighbourhood level.
- Education (neighbourhood): Education measure at a greater level than the household, usually at the neighbourhood level.
- missing indicators: not to lose observations I presume
- Unclear
- Priority status: indicator denoting if the student is part of the priority admission group (for whichever grounds)
- switched schools
- Year/Grade fixed effects: FEs/dummies
- Retained: when students need to repeat the grade due to poor performance, attendance, etc.
- At-risk status: refers to group that would leave the district/study if loses the lottery. Is specific to Engberg's bounds estimated which account for differential attrition.
- GT: controls related to Gifted and Talented students. It could be identification of GT student, if the student was enrolled in a GT program, etc.
- Area: dummies for the area, borough or similar

3.9. Outcomes

3.9.1. Outcome measures

Academic performance and school socioeconomic composition are the primary outcomes the review is looking at. Add a new option (add child code, code type: outcome) for other secondary outcomes not yet listed. When entering outcome data in ER4:

(1) Add a straightforward title and a brief description of the outcome.

(2) Specify immediately the outcome classification group from the list below.

(3) Group 1: treatment and Group 2: control.

For more details on how to create a new outcome, see ER4 Manual (v 8.0), pages 98-102.

Some studies explore potential mechanisms to explain their main estimates, most commonly using outcomes at the school level. DO NOT count these as outcome measures.

- Academic performance
- School socioeconomic composition
- Graduation rate
- Socio-emotional

- Attendance/Absenteeism: Measures of days attended or absent in school.
- Special education
- Executive function
- Disciplinary measures
- Non-cognitive skills: "Cognitive (...) is shorthand for cognitive ability and knowledge, constructs that can be reliably measured by standardized intelligence and achievement tests (Messick, 1979). Non-cognitive, therefore, has become a catchall term for traits or skills not captured by assessments of cognitive ability and knowledge. [West (2016), p.149]
- School-level engagement: School actions that facilitate student engagement (e.g. Edmunds (2013))

3.9.2. Outcome classifications

This section does not retrieve study information, but it serves to identify potential sub-groups for the analyses. If the sub-group is the same for all of the outcomes of a study (e.g. country), there is no need to add a classification code. However, if the sub-group differs by outcomes within the same study (e.g. test subject, follow-up), then use classification codes when entering the outcome data in ER4. Add a new option (add child code, code type: outcome classification code) if a relevant sub-group is not yet listed.

- Follow-up 1
- Follow-up 2
- Subject: math
- Subject: reading
- Subject: science
- Measure: ITT: Intention To Treat, that is, the impact of **receiving an offer** at the school the student applied to. This would not account for compliance.
- Measure: TOT: Treatment On Treated, that is, the impact of **attending** the school the student applied to. This would account for compliance.

3.9.3. Effect size calculation

Does the study report the necessary information to calculate the effect size of outcomes? Specify details and add page number for reference.

- The study reports all necessary information: The study reports all necessary information (means, proportions, etc. and variance measures) to directly calculate the effect size of outcomes.
- The study does not report all information: The study does not report essential information to directly calculate the effect size of outcomes. Assumptions and post-calculations were made to estimate an effect size. Specify all assumptions and post-calculations done for the effect size calculations.
- Unclear / Not reported (specify)
- Not applicable: the study does not measure primary outcomes so, in first instance, these effect sizes will not be extracted.

3.9.4. Extra info

OPTIONAL - Include here any information that doesn't fit elsewhere in the coding tool but that may eventually be helpful/interesting to discuss.

3.10. Conclusions and limitations

3.10.1. Main results/conclusions

Description of the main results and conclusions of the study as stated by the authors.

- Reported: Specify textual copy of description and page number for reference.
- Not reported: Add comment explaining this option.

3.10.2. Limitations

Description/discussion of the limitations of the results and conclusions of the study as stated by the authors. Examples of limitations: (1) low external validity, as those who apply to these schools may be different from those who do not apply; (2) low internal validity, as there are issues of attrition and/or compliance.

- Reported: At least one limitation is discussed. Specify textual copy and page number for reference.
- Not reported: There is no mention in the study document about the results limitations. Is there anything close to this?

8.4. Calculation of effect sizes

For reliability purposes and to avoid potential biases in the effect sizes, the research team defined under an iterative process the following rules and assumptions for the calculation of effect sizes:

- If linked studies report follow-up measures, or if a study reports more than one measure for the same group of students, we extracted the first and last evaluation along with how long after is this last measure reported.

- In line with the assumption of independence of outcomes (Thomas, O'Mara-Eves, Kneale, et al., 2017), in the following cases we combined outcome measures to provide one effect per study for the overall meta-analysis:
  - If a study reports more than one academic outcome measure for the same subject (e.g. two tests measuring different skills within the same subject).
  - If a study reports outcome measures for different groups of students separately (e.g. two different cohorts).

- We used the approach that provided more raw information (e.g. if a study provides enough information to calculate the effect size using either the unstandardized regression coefficient or through a t-test with unequal samples, then we prioritised using the t-test to include the sample size data).

- If the study does not report separately the sample size for the treatment and control groups, and we cannot be sure of these numbers from the sample description, we assumed equal sample size between these groups and noted this assumption in the corresponding section of the extraction tool.

- Ideally, the study provides sufficient data to easily extract the effect size into ER4. In case missing data hinders the calculation of the effect size of a study, we imputed these data using alternative methods and under sensible assumptions following these priorities:
  - Using the incomplete data into the Campbell Collaboration web-based effect-size calculator (Wilson, n.d.).
  - Using p-values to reflect or approximate the statistical significance of the effect described in the report. Additionally, to correctly indicate the direction of the effect

size, t-values were calculated from the (estimated) p-values using the qt() function in R.

- Entering as much information as possible and coding the study with a high risk of outcome reporting bias.

- Given the review's scope and the limited resources of the research team, we extracted the effect sizes for the primary outcomes, and specifically for the academic performance measures, we focused on standardised scores rather than on binary outcomes such as proficient/not proficient. Since the range for secondary outcomes was open, we began by listing all outcomes measured (for the narrative synthesis) and extracted their effect sizes only if at least five records provided comparable and relevant measures.

- We listed all outcomes from linked studies that had not already been entered from the master records.

- We listed every outcome the primary studies intended to measure, even if it was not finally reported in the study results.

8.5. Risk of Bias assessment tool

The following outline was used to assess the risk of biased estimates in each primary study. The assessment tool focused on eight categories: selection, baseline imbalance, identification, compliance, attrition, contamination, sample reporting, outcome reporting. After completing the Risk of Bias assessment for a third of the references, the tool was adjusted with these edits:

- In the Attrition section, we added a new option "Already adjusted" to reflect those studies that explicitly address differential attrition in the analyses.
- In the Attrition section, we incorporated the standards from What Works Clearinghouse (2017, p. 11) to evaluate overall and differential attrition levels.

Risk of Bias assessment

1. Selection

Are there concerns about how participants were included (or eventually excluded) from the research sample in a way that may affect the comparability between the sample and the population aimed to be studied? Consider Section 3.1. (Data source) and Section 3.2. (Intervention - policy level).

- Low risk of bias: The study included all possible schools/students in the research sample, or there is a reasonable justification for excluding potential participants. This exclusion does not affect the comparability between the sample and the population of interest.
- High risk of bias: The study excluded some potential participants from the research sample with poor justification for it. This exclusion affects the comparability between the sample and the population of interest.
- Unclear (specify)

2. Baseline imbalance

If the treatment and control groups were not balanced at baseline, are there concerns about the measures taken by the authors to account/control for this? Consider Section 3.8.3. (Baseline measures).

- Low risk of bias: The groups were balanced at baseline or were not balanced in characteristics that do not strongly affect the outcome measures. The authors control for this issue anyway, or report this without concerns about the study estimates.
- High risk of bias: The study does not report the (im)balance of the groups or, if the groups were not balanced in characteristics directly related to the outcome measures, the authors do not control for this in the analyses.
- Unclear (specify)

3. Identification bias

If the school(s) studied applied some form of priority admission criteria, are there concerns about the inclusion of these students in the analyses? If so, then these priority admission students would be treated as "randomised" students, which would bias the intervention effect. For example, if students got in the school because of the siblings criterion, then the outcome of socioeconomic composition would be biased. Consider Section 2.5.4.B (Priority admission criteria) and Section 3.2. (Intervention - randomisation level).

- Low risk of bias: The study sample does not include priority admission students or, if it does, these students were excluded from the analyses. There are no concerns that the estimations are biased.
- High risk of bias: The study sample includes a percentage of priority admission students and these students were included in the analyses, or the report is unclear about this. There are concerns that the estimations are biased.
- Unclear (specify)

4. Compliance

Are there concerns that schools or students did not comply with their randomly assigned group and how the study managed this issue? Consider Section 3.5. (Compliance) and Section 3.10.2 (Limitations).

- Low risk of bias: The study reports no cases or a moderate level of non-compliance (as stated by the authors) and this is not presented as a concern.
- High risk of bias: The study reports a high level of non-compliance (as stated by the authors) and this is presented with some concern.
- Unclear (specify)

5. Attrition

Are there concerns about schools or students that have dropped out of the study and how the researchers managed this issue? Consider Section 3.6. (Attrition) and Section 3.10.2 (Limitations). Additionally, to evaluate overall and differential attrition, use What Works Clearinghouse thresholds (WWC, 2017, p. 11)

- Low risk of bias: The study reports no cases or a moderate level of attrition (as stated by the authors) and this is not presented as a concern. Following WWC standards, the attrition levels are tolerable.
- High risk of bias: The study reports a high level of attrition (as stated by the authors) and this is presented with some concern. Following WWC standards, the attrition levels are unacceptable.
- Unclear (specify)
- Already adjusted: Check this option if the study estimates already account for differential attrition / selective attrition. This option was added because of Engberg (2014), and may end up being relevant only for this study.

6. Contamination

Given the cases of compliance and attrition in the study, are there concerns about schools or students ending up receiving the treatment they were not randomly assigned for and how the researchers managed this issue? Case of contamination related to compliance: a participant from the control group is exposed to the intervention treatment by attending the same school where she lost the lottery or a different school with lottery. Case of contamination related to attrition: a participant from the intervention group does not receive the intervention treatment by not attending the school where she was offered a place. Consider Section 4. (Compliance) and Section 5. (Attrition).

- Low risk of bias: There are no cases of contamination in the study or, if there are, the authors adequately account/control for this issue (preferably by excluding these cases from the analysis).
- High risk of bias: There are cases of contamination in the study and the authors do not adequately account/control for this issue.
- Unclear (specify)

7. Sample reporting

Is there an adequate description of the sample and are there concerns about any differences between the original sample and the analysis sample? Consider Section 3.7. (Analysis sample).

- Low risk of bias: The study describes the sample explicitly and clearly, and if there are differences between the original and analysis sample, these are negligible.
- High risk of bias: The study does not sufficiently describe the sample, and there are concerning differences between the original and analysis sample.
- Unclear (specify)

8. Outcome reporting

Are there concerns that the study does not report on all the outcomes intended to measure, as declared on the research questions/aims? Are there concerns that the study does not report the necessary or minimum data to calculate an effect size? Consider Section 2.2. (Research question/aim) and Section 3.9. (Outcomes).

- Low risk of bias: The study reports on all outcomes considered in the research question/aim, or if an outcome is not reported, the authors address this issue giving reasonable explanations. The study provides the necessary data to calculate an effect size, or reasonable assumptions have been made for this purpose.
- High risk of bias: The study does not report on all outcomes considered in the research question/aim, and the authors do not address or explain this issue. The study does not provide the minimum data to calculate an effect size and strong assumptions had to be made for this purpose.
- Unclear (specify)

8.6. References obtained through other sources

The literature search included 36 new records that were retrieved from sources other than through database searching, all of which were identified in the first search stage. These new references derive from two types of sources:

a) Linked versions of already included studies:

- One record is the full report of a brief report picked up by the search
- One record is the book chapter of a working paper picked up by the search
- Two records are different reports of a conference proceedings picked up by the search

b) References used in systematic reviews and "quasi-reviews" (a coined term to indicate studies that analyse the estimates of individual primary studies, but that are not based on systematic, rigorous and potentially replicable literature search procedures):

Following the protocol, all references from relevant system reviews were revised and new records were added to the review:

- Betts et.al (2016): 14% of studies (n=7) already included in the review. Six records not considered yet were manually added to the review.
- Betts et.al (2011): 29% of studies (n=24) already included in the review. 17 records not considered yet (plus two linked records) were manually added to the review.
- Shakeel et.al (2016): 59% of studies (n=17) already included in the review. Seven records not considered yet were manually added to the review.
- Krowka et.al (2017): 100% of studies (n=5) already included in the review.

Additionally, the literature search identified the following relevant "quasi-reviews", however, our review had already identified and retrieved the vast majority of their references:

- Chabrier et.al (2016): 88% of studies (n=8) in review; one institutional report not found by search.
- Cheng et.al (2017): 100% of studies (n=10) already included in review.
- Wang et.al (2018): 100% of studies (n=7) already included in review.

8.7. Double-screening of references

A summary of the double-screening processes is shown in *Table 8.4*. In the first search stage, the double-screening of 30% of studies based on title and abstract yielded an excellent 99% of agreement considering the binary options of the inclusion criteria. Given this result and the restricted resources of the reviewers, for the update search, the selection of references based on title and abstract was single-coded by R1.

For the double-screening of records based on full text, 30% of studies were considered in the first search and resulted in a slightly higher but still acceptable disagreement rate (3 cases based on include/exclude criteria). After discussing these cases in detail, the research team decided that the disagreements derived more from having different publication types of the same record than on misinterpretations of the eligibility criteria.

In the update search, a larger proportion of records (36%) were double-coded based on full text due to not-retrievable records. The single case in which R1 and R2 disagreed at this stage came from an exclusion decision defined in the first search; namely, that studies based on the random allocation of educational vouchers or scholarships would not be considered in the review. After discussing the case, the researchers deemed that this disagreement was due to the time passed between the two search stages rather than to conflicting views of the inclusion criteria. Accordingly, in both search stages, the research team proceeded confidently with the review.

Table 8.4 Double-coding results for each screening and search phase

	Screened on Title and abstract		Screened on Full Text	
	Single coded	Double coded	Single coded	Double coded
First search				
Number of references	4,554 (70%)	1,939 (30%)	96 (70%)	41 (30%)
Disag. Include/Exclude		19 (1%)		3 (7%)
Disag. Full Criteria		39 (2%)		4 (10%)
Update search				
Number of references	615 (100%)	-	14 (64%)	8 (36%)
Disag. Include/Exclude		-		1 (13%)
Disag. Full Criteria		-		1 (13%)

8.8. References excluded based on full text

Table 8.5 Excluded references in screening by full report

Study	Title	Reason for exclusion	Publication type
Hull (2017)	Estimating Institutional Quality with Instruments: Three Essays and Applications in Education and Healthcare	Not retrieved	Dissertation
Setren (2017)	Essays on the Economics of Education	Not retrieved	Dissertation
Weinstein (2008)	Neighborhood racial dynamics, parental school choice behavior, and student achievement: Evidence from natural experiments	Not retrieved	Dissertation
Abdulkadiroglu (2015)	Free to Choose: Can School Choice Reduce Student Achievement?	Exclude on focus	Working paper
Abdulkadiroglu (2016)	Charters without Lotteries: Testing Takeovers in New Orleans and Boston	Exclude on focus	Journal article
Abdulkadiroglu (2017b)	Regression Discontinuity in Serial Dictatorship: Achievement Effects at Chicago's Exam Schools	Exclude on focus	Conf. proceedings
Abdulkadiroglu (2018)	Free to Choose: Can School Choice Reduce Student Achievement?	Exclude on focus	Journal article
Ackerman (2017)	A critical look at methodologies used to evaluate charter school effectiveness	Exclude on focus	Journal article
Angrist (2001)	Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment. NBER Working Paper Series.	Exclude on focus	Report
Angrist (2002)	Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment	Exclude on focus	Journal article
Angrist (2006)	Long-Term Educational Consequences of Secondary School Vouchers: Evidence from Administrative Records in Colombia	Exclude on focus	Journal article
Barnard (2003)	Principal Stratification Approach to Broken Randomized Experiments: A Case Study of School Choice Vouchers in New York City	Exclude on focus	Journal article
Bell (2013)	Methods for Analyzing Data from a Randomized Control Trial with a Nationally Representative Sample	Exclude on focus	Conf. proceedings
Bettinger (2006)	Using experimental economics to measure the effects of a natural educational experiment on altruism	Exclude on focus	Journal article
Bettinger (2010)	Are educational vouchers only redistributive?	Exclude on focus	Journal article
Bitler (2015)	Distributional Analysis in Educational Evaluation: A Case Study from the New York City Voucher Program	Exclude on focus	Journal article
Booker (2010)	The Unknown World of Charter High Schools	Exclude on focus	Journal article

Study	Title	Reason for exclusion	Publication type
Brown (2005)	The impact of preschool on middle -class children in a public inclusion program	Exclude on focus	Dissertation
Bu (2016)	Neighborhood influence towards students' performance in charter school	Exclude on focus	Dissertation
Greene (1999)	Effectiveness of School Choice: The Milwaukee Experiment	Exclude on focus	Journal article
Greene (2000)	The Effect of School Choice: An Evaluation of the Charlotte Children's Scholarship Fund Program. Civic Report No. 12.	Exclude on focus	Report
Hastings (2006)	Preferences and heterogeneous treatment effects in a public school choice lottery. NBER Working Paper No. 12145	Exclude on focus	Report
Hastings (2007)	No Child Left Behind: Estimating the Impact on Choices and Student Outcomes. NBER Working Paper No. 13009	Exclude on focus	Working paper
Hooyer (2010)	Education services for 10th-grade students: Comparison of the California High School Exit Exam between virtual charter schools and traditional charter schools	Exclude on focus	Dissertation
Howell (2000)	Test-Score Effects of School Vouchers in Dayton, Ohio, New York City, and Washington, DC: Evidence from Randomized Field Trials.	Exclude on focus	Conf. proceedings
Howell (2000)	School Choice in Dayton, Ohio: An Evaluation After One Year.	Exclude on focus	Conf. proceedings
Howell (2002)	School vouchers and academic performance: results from three randomized field trials	Exclude on focus	Journal article
Hoxby (2003)	School choice and school competition: Evidence from the United States	Exclude on focus	Journal article
Jin (2009)	Public Schools versus Private Schools: Causal Inference with Partial Compliance	Exclude on focus	Journal article
Jin (2010)	A Modified General Location Model for Noncompliance With Missing Data: Revisiting the New York City School Choice Scholarship Program Using Principal Stratification	Exclude on focus	Journal article
Kisida (2015)	Customer Satisfaction and Educational Outcomes: Experimental Impacts of the Market-Based Delivery of Public Education	Exclude on focus	Journal article
Krueger (2003)	Principal Stratification Approach to Broken Randomized Experiments: A Case Study of School Choice Vouchers in New York City [with Comment]	Exclude on focus	Journal article
Krueger (2004)	Another look at the New York city school voucher experiment	Exclude on focus	Journal article
Lamarche (2011)	Measuring the incentives to learn in Colombia using new quantile regression approaches	Exclude on focus	Journal article
Mathis (2002)	Academic, Socioeconomic and Transportation Correlates in a Rural Public School Voucher System.	Exclude on focus	Report
Mayer (2002)	School Choice in New York City after Three Years: An Evaluation of the School Choice Scholarships Program. Final Report.	Exclude on focus	Report

Study	Title	Reason for exclusion	Publication type
Muehlenbein (2017)	Three Essays on the Economics of Education Choice	Exclude on focus	Dissertation
Muralidharan (2015)	The Aggregate Effect of School Choice: Evidence from a Two-Stage Experiment in India	Exclude on focus	Journal article
Muthén (2003)	Principal Stratification Approach to Broken Randomized Experiments: A Case Study of School Choice Vouchers in New York City [with Comment]	Exclude on focus	Journal article
Myers (2000)	School Choice in New York City after Two Years: An Evaluation of the School Choice Scholarships Program. Interim Report.	Exclude on focus	Report
Peterson (1998)	Initial Findings from an Evaluation of School Choice Programs in Washington, D.C.	Exclude on focus	Report
Peterson (1998)	An Evaluation of the New York City School Choice Scholarships Program: The First Year.	Exclude on focus	Generic
Peterson (2001)	Exploring Explanations for Ethnic Differences in Voucher Impacts on Student Test Scores.	Exclude on focus	Report
Peterson (2001)	An Evaluation of the Children's Scholarship Fund.	Exclude on focus	Report
Peterson (2003)	Latest Results from the New York City Voucher Experiment.	Exclude on focus	Report
Puma (2010)	Head Start Impact Study. Final Report.	Exclude on focus	Report
Puma (2012)	Third grade follow-up to the Head Start Impact Study: Final report (OPRE Report 2012-45)	Exclude on focus	Report
Saavedra (2009)	The role of resources and incentives in education production	Exclude on focus	Dissertation
Sanbonmatsu (2006)	Neighborhoods and Academic Achievement: Results from the Moving to Opportunity Experiment	Exclude on focus	Journal article
Wang (2017)	Transitional student admission mechanism from tracking to mixing: an agent-based policy analysis	Exclude on focus	Journal article
Wang (2017b)	Agent-Based Overlapping Generations Modeling for Educational Policy Analysis	Exclude on focus	Dissertation
Wolf (2000)	School Choice in Washington, D.C.: An Evaluation after One Year.	Exclude on focus	Report
Wolf (2001)	Results of a School Voucher Experiment: The Case of Washington, D.C. after Two Years.	Exclude on focus	Generic
Wolf (2005)	Evaluation of the DC Opportunity Scholarship Program: First Year Report on Participation	Exclude on focus	Report
Wolf (2008)	Evaluation of the DC Opportunity Scholarship Program: Impacts after Two Years. NCEE 2008-4023	Exclude on focus	Report
Wolf (2008)	Evaluation of the DC Opportunity Scholarship Program: Impacts after Two Years. Executive Summary. NCEE 2008-4024	Exclude on focus	Report
Wolf (2009)	Evaluation of the DC Opportunity Scholarship Program: Impacts after Three Years. Executive Summary. NCEE 2009-4051	Exclude on focus	Report
Wolf (2009)	Evaluation of the DC Opportunity Scholarship Program: Impacts after Three Years. NCEE 2009-4050	Exclude on focus	Report
Wolf (2013)	School Vouchers and Student Outcomes: Experimental Evidence from Washington, DC	Exclude on focus	Journal article
Wolf (2015)	Private school choice in developing countries: experimental results from Delhi, India	Exclude on focus	Book chapter

Study	Title	Reason for exclusion	Publication type
WWC (2010)	WWC Quick Review of the Report "Evaluation of the DC Opportunity Scholarship Program: Impacts After Three Years"	Exclude on focus	Generic
Hoxby (2005)	Do Charter Schools Help Their Students? Civic Bulletin No. 38	Exclude on type	Generic
Levenstein (2008)	Methodological Issues in Intervention Research:: Lessons from The Parent-Child Home Program Experience	Exclude on type	Book chapter
Maxwell (2010)	No Clear Edge for Charter Schools Found in 15-State Study: More Successes Seen in Charter Schools Serving Disadvantaged Students	Exclude on type	Journal article
Chabrier (2016)	What Can We Learn from Charter School Lotteries?	Exclude on design	Journal article
Fenzel (2009)	Effective Alternative Urban Middle Schools: Findings from Research on Nativity Miguel Schools	Exclude on design	Journal article
Hahn (2018)	Does greater school autonomy make a difference? Evidence from a randomized natural experiment in South Korea	Exclude on design	Journal article
Simon (1973)	The Development and Evaluation of an Alternative High School: A Report on S.E.E. (School of Experiential Education). Phase 2.	Exclude on design	Report
Song (2015)	Essays in the economics of education	Exclude on design	Dissertation
Song (2017)	Sorting, school performance and quality: Evidence from China	Exclude on design	Working paper
Wofford (1973)	Philadelphia's Parkway Program: An Evaluation.	Exclude on design	Generic
Avery (2013)	Evaluation of the College Possible Program: Results from a Randomized Controlled Trial. NBER Working Paper No. 19562	Exclude on outcomes	Report

8.9. Bibliography for included studies

1. Abdulkadiroglu, A., Angrist, J. D., Dynarski, S. M., Kane, T. J., & Pathak, P. A. (2011). Accountability and Flexibility in Public Schools: Evidence from Boston's Charters And Pilots. *The Quarterly Journal of Economics*, *126*(2), 699–748. https://doi.org/10.1093/qje/qjr017

2. Abdulkadiroglu, A., Angrist, J. D., Narita, Y., & Pathak, P. A. (2017). Research Design Meets Market Design: Using Centralized Assignment for Impact Evaluation. *Econometrica*, *85*(5), 1373–1432. https://doi.org/10.3982/ECTA13925

3. Abdulkadiroglu, A., Hu, W., & Pathak, P. A. (2013). *Small High Schools and Student Achievement: Lottery-Based Evidence from New York City* (Working paper No. w19576). Cambridge, MA: National Bureau of Economic Research. Retrieved from http://www.nber.org/papers/w19576

4. Allen, R., Burgess, S., & McKenna, L. (2013). The short-run impact of using lotteries for school admissions: early results from Brighton and Hove's reforms: *Lotteries for school admissions*. *Transactions of the Institute of British Geographers*, *38*(1), 149–166. https://doi.org/10.1111/j.1475-5661.2012.00511.x

5. Angrist, J. D., Pathak, P. A., & Walters, C. R. (2013). Explaining Charter School Effectiveness. *American Economic Journal: Applied Economics*, *5*(4), 1–27. https://doi.org/10.1257/app.5.4.1

6. Ballou, D. (2007). *Magnet Schools and Peers: Effects on Mathematics Achievement*. Vanderbilt University. Retrieved from http://search.proquest.com/docview/61811679

7. Balsa, A. I., & Cid, A. (2016). A Randomized Impact Evaluation of a Tuition-Free Private School Targeting Low Income Students in Uruguay. *Journal of Applied Economics*, *19*(1), 65–94. https://doi.org/10.1016/S1514-0326(16)30003-4

8. Bifulco, R., Cobb, C. D., & Bell, C. (2009). Can Interdistrict Choice Boost Student Achievement? The Case of Connecticut's Interdistrict Magnet School Program. *Educational Evaluation and Policy Analysis*, *31*(4), 323–345. https://doi.org/10.3102/0162373709340917

9. Bui, S. A., Craig, S. G., & Imberman, S. A. (2014). Is Gifted Education a Bright Idea? Assessing the Impact of Gifted and Talented Programs on Students. *American Economic Journal: Economic Policy*, *6*(3), 30–62. https://doi.org/10.1257/pol.6.3.30

10. Cook, J. B. (2017). Essays in the Economics of Education. *Cornell University Library*. https://doi.org/10.7298/x4gt5k9v

11. Crain, R. L., Heebner, A. L., & Si, Y.-P. (1992). *The Effectiveness of New York City's Career Magnet Schools: An Evaluation of Ninth Grade Performance Using an Experimental Design* (p. 91). National Center for Research in Vocational Education. Retrieved from https://eric.ed.gov/?id=ED344064

12. Cullen, J. B., & Jacob, B. A. (2009). Is Gaining Access to Selective Elementary Schools Gaining Ground? Evidence from Randomized Lotteries. In *The problems of disadvantaged youth: an economic perspective* (pp. 43–84). University of Chicago Press.

13. Cullen, J. B., Jacob, B. A., & Levitt, S. (2006). The Effect of School Choice on Participants: Evidence from Randomized Lotteries. *Econometrica*, *74*(5), 1191–1230. https://doi.org/10.1111/j.1468-0262.2006.00702.x

14. Culverhouse, R. (2018). *How School Choice Can Affect Academic Success: A Study of Two Virginia Inner-City Magnet Middle School Sixth-Grade Students' Reading and Math Scores*. Regent University. Retrieved from https://search.proquest.com/docview/2019195757?accountid=14511

15. Curto, V. E., & Fryer, R. G. (2014). The Potential of Urban Boarding Schools for the Poor: Evidence from SEED. *Journal of Labor Economics*, *32*(1), 65–93. https://doi.org/10.1086/671798

16. Dynarski, S., Hubbard, D., Jacob, B., & Robles, S. (2018). *Estimating the Effects of a Large For-Profit Charter School Operator* (Working paper No. w24428). National Bureau of Economic Research. Retrieved from https://www.nber.org/papers/w24428

17. Edmunds, J. A., Unlu, F., Glennie, E., Bernstein, L., Fesler, L., Furey, J., & Arshavsky, N. (2017). Smoothing the Transition to Postsecondary Education: The Impact of the Early College Model. *Journal of Research on Educational Effectiveness*, *10*(2), 297–325. https://doi.org/10.1080/19345747.2016.1191574

18. Engberg, J., Epple, D., Imbrogno, J., Sieg, H., & Zimmer, R. (2014). Evaluating Education Programs That Have Lotteried Admission and Selective Attrition. *Journal of Labor Economics*, *32*(1), 27–63. https://doi.org/10.1086/671797

19. Foreman, L. M., Anderson, K. P., Ritter, G. W., & Wolf, P. J. (2017). Using "Broken" Lotteries to Check the Validity of Charter School Evaluations Using Matching Designs. *Educational Policy*, 089590481774154. https://doi.org/10.1177/0895904817741543

20. Hastings, J., Kane, T. J., & Staiger, D. O. (2006). Gender and Performance: Evidence from School Assignment by Randomized Lottery. *The American Economic Review*, *96*(2), 232–236.

21. Hastings, J., Neilson, C. A., & Zimmerman, S. D. (2012). *The Effect of School Choice on Intrinsic Motivation and Academic Outcomes* (Working paper No. w18324). National Bureau of Economic Research. Retrieved from http://www.nber.org/papers/w18324

22. Hemelt, S. W., Lenard, M. A., & Paeplow, C. G. (2017). *Building Better Bridges to Life after High School: Experimental Evidence on Contemporary Career Academies* (No. 176) (p. 46). National Center for Analysis of Longitudinal Data in Education Research. Retrieved from https://search.proquest.com/docview/1895981561?accountid=14511

23. Hoxby, C. M., & Murarka, S. (2009). *Charter Schools in New York City: Who Enrolls and How They Affect Their Students' Achievement* (Working paper No. w14852). National Bureau of Economic Research. Retrieved from https://www.nber.org/papers/w14852

24. Hoxby, C. M., & Rockoff, J. E. (2005). *The impact of charter schools on student achievement*. RAND. Retrieved from http://www.rand.org/content/dam/rand/www/external/labor/seminars/adp/pdfs/2005hoxby.pdf

25. McClure, L., Strick, B., Jacob-Almeida, R., & Reicher, C. (2005). *The Preuss School at UCSD: School Characteristics and Students' Achievement*. The Center for Research on Educational Equity, Assessment and Teaching Excellence, University of California, San Diego. Retrieved from http://create.ucsd.edu/_files/publications/PreussReportDecember2005.pdf

26. Nikolov, L. (2017). *In-District Charter Programming in an Urban School District: The Promises and Challenges of Semi-Autonomous Schooling*. Lehigh University. Retrieved from https://search.proquest.com/docview/1916576952

27. Ruijs, N. (2017). The effects of Montessori education: Evidence from admission lotteries. *Economics of Education Review*, *61*, 19–34. https://doi.org/10.1016/j.econedurev.2017.09.001

28. Steele, J. L., Slater, R. O., Zamarro, G., Miller, T., Li, J., Burkhauser, S., & Bacon, M. (2017). Effects of Dual-Language Immersion Programs on Student Achievement: Evidence From Lottery Data. *American Educational Research Journal*, *54*(1_suppl), 282S-306S. https://doi.org/10.3102/0002831216634463

29. Unterman, R. (2017). *An Early Look at the Effects of Success Academy Charter Schools*. MDRC. Retrieved from https://eric.ed.gov/?id=ED575961

30. Wong, M. D., Coller, K. M., Dudovitz, R. N., Kennedy, D. P., Buddin, R., Shapiro, M. F., … Chung, P. J. (2014). Successful Schools and Risky Behaviors Among Low-Income Adolescents. *PEDIATRICS*, *134*(2), e389–e396. https://doi.org/10.1542/peds.2013-3573

31. Zhang, H. (2009). *Essays in the economics of education and program evaluation*. Massachusetts Institute of Technology. Retrieved from http://search.proquest.com/docview/305096514

8.10. References excluded from master records

Table 8.6 Included references that are not unique records

Study	Title	Reason for exclusion	Publication type
Abdulkadiroglu (2009)	Informing the Debate: Comparing Boston's Charter, Pilot and Traditional Schools	Linked study	Report
Abdulkadiroglu (2009b)	Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots	Linked study	Working paper
Angrist (2011)	Explaining Charter School Effectiveness	Linked study	Report
Angrist (2012)	Explaining charter school effectiveness	Linked study	Report
Angrist (2012b)	Explaining Charter School Effectiveness	Linked study	Conf. proceedings
Bergman (2012)	Essays on the Economics of Education	Linked study	Dissertation
Bui (2011)	Is Gifted Education a Bright Idea? Assessing the Impact of Gifted and Talented Programs on Achievement	Linked study	Report
Bui (2012)	Essays on applied microeconomics	Linked study	Dissertation
Crain (1999)	The Effects of Academic Career Magnet Education on High Schools and Their Graduates	Linked study	Report
Crain (1999b)	The Effects of Career Magnet Schools	Linked study	Report
Cullen (2007)	Is Gaining Access to Selective Elementary Schools Gaining Ground? Evidence From Randomized Lotteries	Linked study	Working paper
Curto (2011)	Estimating the Returns to Urban Boarding Schools: Evidence from SEED	Linked study	Working paper
Deming (2010)	Long-term impacts of educational interventions	Linked study	Dissertation
Deming (2011)	Better Schools, Less Crime?	Linked study	Journal article
Deming (2011b)	School Choice, School Quality and Postsecondary Attainment	Linked study	Working paper
Deming (2012)	Does School Choice Reduce Crime?: Evidence from North Carolina	Linked study	Journal article
Deming (2014)	School Choice, School Quality, and Postsecondary Attainment	Linked study	Journal article
Edmunds (2013)	Mandated Engagement: The Impact of Early College High Schools	Linked study	Journal article
Foreman (2017b)	You Can't Always Get What You Want: Using 'Broken Lotteries' to Check the Validity of Charter School Evaluations Using Matching Designs	Linked study	Working paper
Glennie (2016)	The Small, Stand-Alone Early College: Impact on High School Outcomes	Linked study	Conf. proceedings

Study	Title	Reason for exclusion	Publication type
Heebner (1995)	The Impact of Career Magnet High Schools: Experimental and Qualitative Evidence	Linked study	Journal article
Hoxby (2008)	New York City Charter Schools	Linked study	Journal article
Hoxby (2009b)	How New York City's charter schools affect achievement	Linked study	Report
Hu (2015)	Essays on NYC High Schools	Linked study	Dissertation
Liu (2010)	Peer group effects on student outcomes: Evidence from randomized lotteries	Linked study	Dissertation
Rockoff (2004)	Essays on the finance and production of public education	Linked study	Dissertation
Walters (2013)	School Choice, School Quality, and Human Capital: Three Essays	Linked study	Dissertation
WWC (2013)	WWC Review of the Report "Better Schools, Less Crime?"	Linked study	Generic
Angrist (2010)	Inputs and Impacts in Charter Schools: KIPP Lynn	Overlapping sample	Journal article
Angrist (2010b)	Who Benefits from KIPP?	Overlapping sample	Working paper
Angrist (2012c)	Who Benefits from KIPP?	Overlapping sample	Journal article
Angrist (2014)	Stand and Deliver: Effects of Boston's Charter High Schools on College Preparation, Entry, and Choice	Overlapping sample	Conf. proceedings
Angrist (2016)	Stand and Deliver: Effects of Boston's Charter High Schools on College Preparation, Entry, and Choice	Overlapping sample	Journal article
Bloom (2010)	Transforming the High School Experience: How New York City's New Small Schools Are Boosting Student Achievement and Graduation Rates	Overlapping sample	Report
Bloom (2013)	Sustained Progress: New Findings about the Effectiveness and Operation of Small Public High Schools of Choice in New York City	Overlapping sample	Report
Bloom (2014)	Can Small High Schools of Choice Improve Educational Prospects for Disadvantaged Students?	Overlapping sample	Journal article
Clark (2011)	Do Charter Schools Improve Student Achievement? Evidence from a National Randomized Study	Overlapping sample	Working paper
Clark (2015)	Do Charter Schools Improve Student Achievement?	Overlapping sample	Journal article
Dobbie (2009)	Are High Quality Schools Enough to Close the Achievement Gap? Evidence from a Social Experiment in Harlem	Overlapping sample	Working paper
Dobbie (2011)	Are High-Quality Schools Enough to Increase Achievement Among the Poor? Evidence from the Harlem Children's Zone	Overlapping sample	Journal article
Dobbie (2013)	Essays in Labor Economics	Overlapping sample	Dissertation
Dobbie (2015)	The Medium-Term Impacts of High-Achieving Charter Schools	Overlapping sample	Journal article
Gleason (2010)	The Evaluation of Charter School Impacts: Final Report	Overlapping sample	Report
Grigg (2014)	Impacts and Alternatives: Evidence from an Elementary Charter School Evaluation	Overlapping sample	Journal article
Haxton (2016)	Longitudinal Findings from the Early College High School Initiative Impact Study	Overlapping sample	Journal article

Study	Title	Reason for exclusion	Publication type
Hughes (2012)	Linking Research and Practice in New York: A New York City Small Schools of Choice Case Study	Overlapping sample	Conf. proceedings
Kemple (2000)	Career Academies: Impacts on Students' Engagement and Performance in High School	Overlapping sample	Report
Knechtel (2017)	Pre-Kindergarten Impacts Over Time: An Analysis of KIPP Charter Schools	Overlapping sample	Report
Kraft (2015)	How to Make Additional Time Matter: Integrating Individualized Tutorials into an Extended Day	Overlapping sample	Journal article
NCEE (2010)	The Evaluation of Charter School Impacts: NCEE Study Snapshot	Overlapping sample	Generic
Shollenberger (2015)	Essays on Schools, Crime, and Punishment	Overlapping sample	Dissertation
Spencer (2017)	An Examination of Student Mobility in U.S. Public Schools	Overlapping sample	Dissertation
Tuttle (2013)	KIPP Middle Schools: Impacts on Achievement and Other Outcomes. Final Report	Overlapping sample	Report
Tuttle (2015)	Understanding the Effect of KIPP as It Scales: Volume I, Impacts on Achievement and Other Outcomes	Overlapping sample	Report
West (2016)	Promise and Paradox: Measuring Students' Non-Cognitive Skills and the Impact of Schooling	Overlapping sample	Journal article
WWC (2010)	WWC Quick Review of the Report "The Evaluation of Charter School Impacts: Final Report"	Overlapping sample	Generic
WWC (2010b)	WWC Quick Review of the Article "Are High-Quality Schools Enough to Close the Achievement Gap? Evidence from a Social Experiment in Harlem"	Overlapping sample	Generic
WWC (2014)	WWC Review of the Report "Sustained Progress: New Findings about the Effectiveness and Operation of Small Public High Schools of Choice in New York City"	Overlapping sample	Generic
WWC (2015)	WWC Review of the Report "Stand and Deliver: Effects of Boston's Charter High Schools on College Preparation, Entry, and Choice"	Overlapping sample	Generic

8.11. Funnel plots

The following funnel plots show the relationship between the effect sizes and standard errors for each subject/analysis strategy. Using sensitivity analyses, we checked every study outside the funnel; that is, with a smaller standard error than expected for such an effect size.

Figure 8.2 Funnel plot for math, ITT

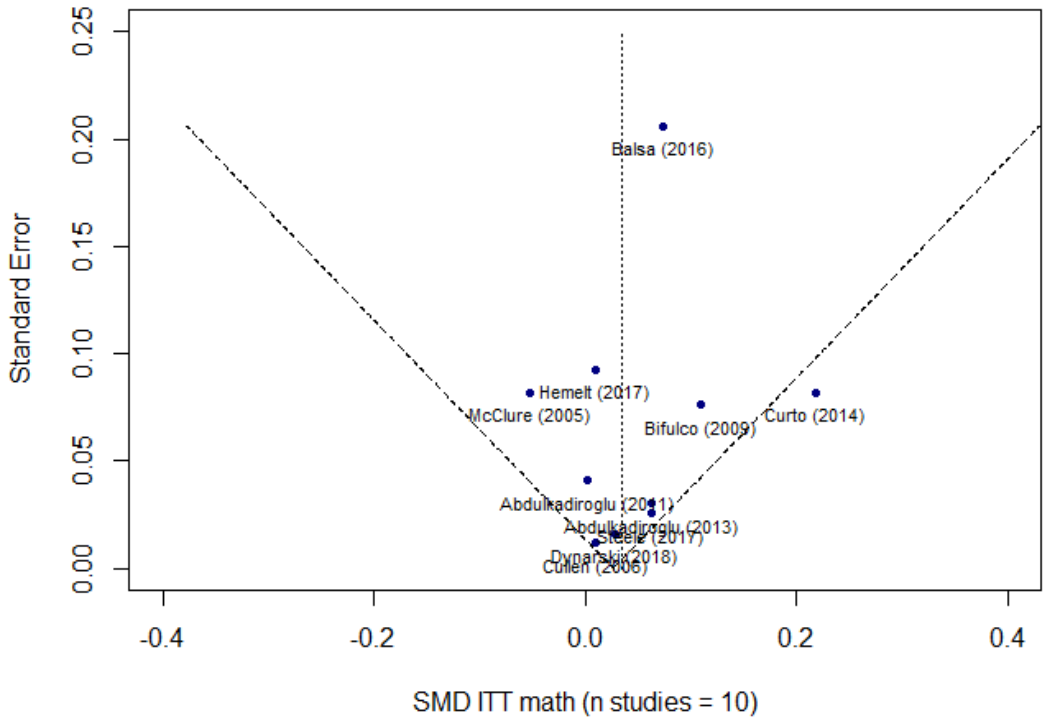


Figure 8.3 Funnel plot for math, TOT

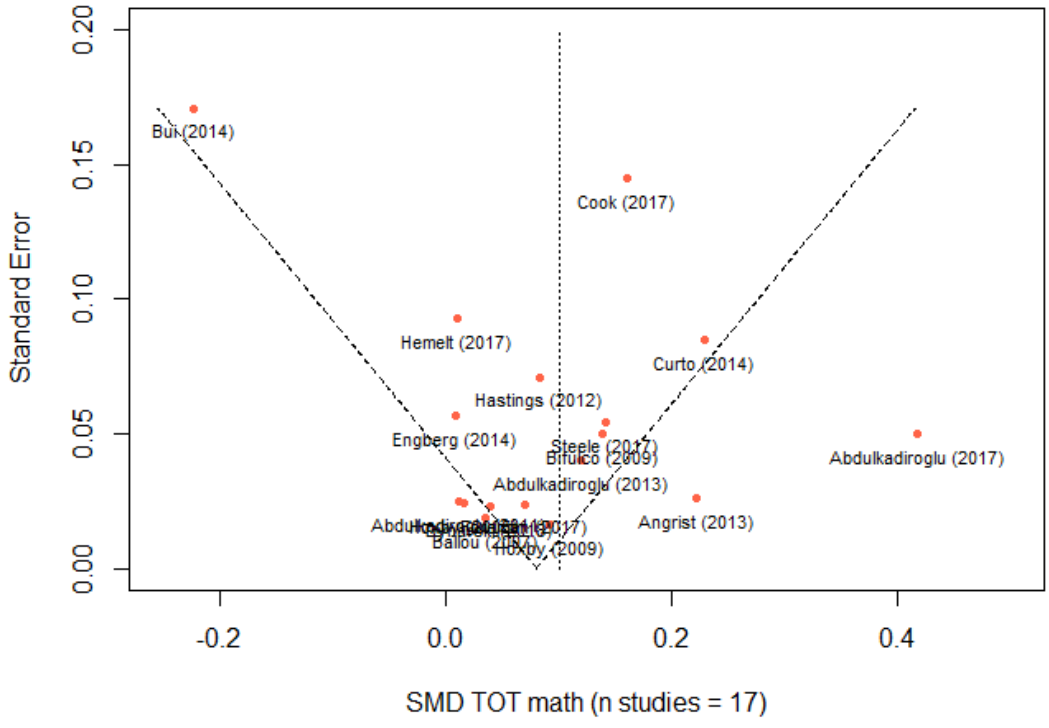


Figure 8.4 Funnel plot for reading, ITT

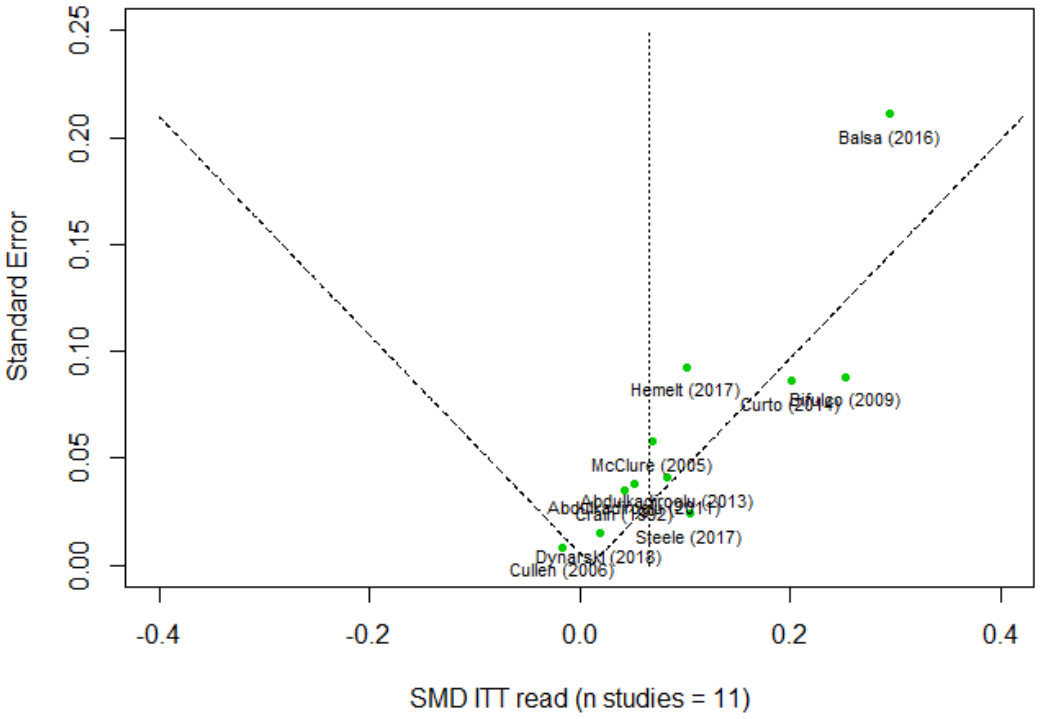
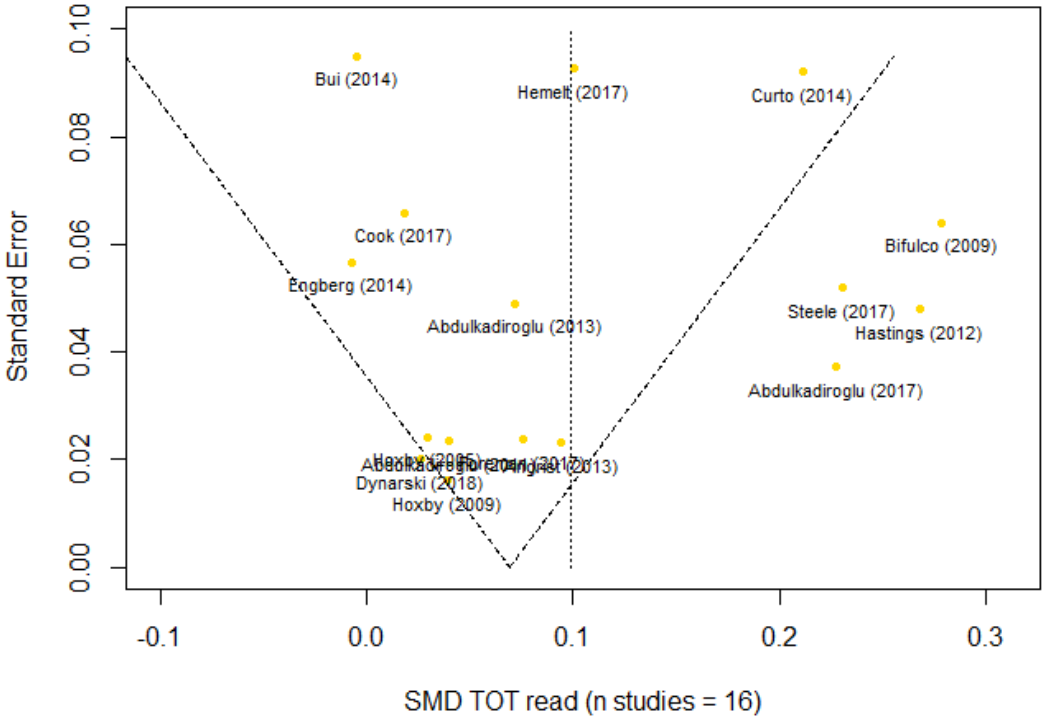


Figure 8.5 Funnel plot for reading, TOT



8.12. Use of covariates

Table 8.7 shows the breakdown of the use of control variables for studies included in the quantitative synthesis. The little variation in this use did not permit including this variable as part of the subgroup analyses.

Table 8.7 Use of covariates by categories and subject/strategy

Analysis strategy	Total # of studies	# of studies including covariates in analysis			
		Demographic	Educational	Methodological	None
ITT math	10	8	8	8	2
ITT reading	11	8	9	8	2
TOT math	17	16	16	15	0
TOT reading	16	16	15	15	0

8.13. Estimates for additional analyses

The following tables (*Table 8.8* to *Table 8.19*) show the subgroup, meta-regression, and sensitivity analysis estimates for each subject/analysis strategy outcome. The shaded sections indicate statistically significant results, which are discussed in the main body of the results.

Table 8.8 Subgroup analysis estimates for math, ITT

	SMD	CI lower	CI upper	Test for subgroup differences	
				Q	p-value
School type					
Public	0.03	0.00	0.07	0.16	0.68
Other	0.06	-0.12	0.23		
Intervention: policy level					
Educational system	0.05	-0.01	0.11	0.85	0.36
School system	0.03	0.00	0.05		
School sample size					
5 or less	0.09	-0.10	0.28	1.06	0.30
More than 5	0.03	0.00	0.05		
Methods: 2 groups					
IV	0.05	0.01	0.09	5.02	0.03
Other	0.01	-0.02	0.04		
ES calculations					
Assumptions were made	0.01	-0.10	0.12	5.34	0.02
Study reports all info	0.05	0.01	0.08		
Study location					
No studies with unknown location					

Table 8.9 Meta-regression estimates for math, ITT

	estimate	std error	p-value	I^2	R^2
Methods (ref cat: IV)					
Other	-0.04	0.02	0.11	10.1	62.3
ES calculations (ref cat: Assump. were made)					
Study reports all info	0.04	0.02	0.10	9.3	65.5
Year of publication	0.00	0.00	0.27	21.7	4.6

Table 8.10 Sensitivity analysis estimates for math, ITT

	# of studies	SMD	CI lower	CI upper	I^2	I^2 CI lower	I^2 CI upper
All studies	10	0.04	0.00	0.07	0.33	0.00	0.68
Exc Curto (2014)	9	0.02	0.01	0.04	0.00	0.00	0.65
Exc McClure (2005)	9	0.04	0.01	0.07	0.36	0.00	0.71
Exc both studies	8	0.03	0.01	0.05	0.01	0.00	0.68
Exc methods: other	7	0.05	0.01	0.09	0.27	0.00	0.68
Exc ES calc: Assumptions made	8	0.05	0.01	0.08	0.15	0.00	0.58
Inc studies with ITT & TOT	7	0.05	0.01	0.09	0.27	0.00	0.68

Table 8.11 Subgroup analysis estimates for math, TOT

	SMD	CI lower	CI upper	Test for subgroup differences	
				Q	p-value
School type [a]					
Public	0.06	0.01	0.11	2.04	0.15
Other	0.14	0.01	0.27		
Intervention: policy level					
Educational system	0.11	0.04	0.18	5.14	0.02
School system	0.04	-0.05	0.13		
School sample size					
5 or less	0.06	-0.04	0.15		
More than 5	0.13	0.03	0.24	2.49	0.29
Not reported	0.05	-0.10	0.20		
ES calculations					
Assumptions were made	0.04	0.00	0.08	5.65	0.02
Study reports all info	0.13	0.05	0.20		
Study location					
Known	0.13	0.05	0.21	4.08	0.04
Unknown	0.05	0.01	0.08		

Note a: this analysis excludes one study evaluating both public and charter schools (Hastings, 2012).

Table 8.12 Meta-regression estimates for math, TOT

	estimate	std error	p-value	I^2	R^2
Intervention: policy level (ref cat: Ed system)					
School system	-0.08	0.09	0.38	86.0	0.0
ES calculations (ref cat: Assump. were made)					
Study reports all info	0.09	0.06	0.13	83.8	5.3
Study location (ref cat: Known)					
Unknown	-0.08	0.06	0.18	85.4	0.0
Year of publication	0.01	0.01	0.25	85.3	0.0

Table 8.13 Sensitivity analysis estimates for math, TOT

	# of studies	SMD	CI lower	CI upper	I^2	I^2 CI lower	I^2 CI upper
All studies	17	0.10	0.04	0.16	0.86	0.78	0.90
Exc Abdulkadiroglu (2017)	16	0.08	0.04	0.12	0.77	0.63	0.86
Exc Bui (2014)	16	0.11	0.05	0.16	0.86	0.79	0.91
Exc Abdulkadiroglu (2011)	16	0.11	0.05	0.17	0.85	0.78	0.90
Exc Angrist (2013)	16	0.09	0.03	0.15	0.81	0.70	0.88
Exc Ballou (2007)	16	0.11	0.04	0.17	0.86	0.78	0.91
Exc Hoxby (2005)	16	0.11	0.05	0.17	0.86	0.78	0.90
Exc Foreman (2017)	16	0.10	0.04	0.17	0.86	0.80	0.91
Exc all studies above	10	0.09	0.05	0.13	0.28	0.00	0.66
Exc Policy level: school	15	0.11	0.04	0.18	0.87	0.80	0.91
Exc ES calc: assumptions made	13	0.13	0.05	0.20	0.87	0.79	0.92
Exc Study location: unknown	11	0.13	0.05	0.21	0.90	0.84	0.94
Inc studies with ITT & TOT	7	0.08	0.02	0.15	0.62	0.12	0.83

Table 8.14 Subgroup analysis estimates for reading, ITT

	SMD	CI lower	CI upper	Test for subgroup differences	
				Q	p-value
School type					
Public	0.07	0.00	0.14	0.03	0.87
Other	0.08	-0.07	0.23		
Intervention: policy level					
Educational system	0.08	0.00	0.16	2.30	0.13
School system	0.03	-0.02	0.08		
School sample size [a]					
5 or less	0.16	0.01	0.32	4.88	0.03
More than 5	0.05	-0.01	0.10		
Methods: 3 groups					
Anova, Manova, Chi-Sq, T tests	0.05	-0.10	0.20		
OLS	0.07	-1.69	1.83	1.73	0.42
IV	0.09	0.02	0.15		
Methods: 2 groups					
IV	0.09	0.02	0.15	2.61	0.11
Other	0.02	-0.07	0.12		
ES calculations					
Assumptions were made	0.01	-0.47	0.49	2.78	0.10
Study reports all info	0.08	0.03	0.13		
Study location					
No studies with unknown location					

Note a: this analysis excludes one study with no information about the school sample size (Crain, 1992).

Table 8.15 Meta-regression estimates for reading, ITT

	estimate	std error	p-value	I^2	R^2
School sample size (ref cat: 5 or less)					
More than 5	-0.11	0.05	0.05	77.2	25.2
Year of publication	0.00	0.00	0.50	76.6	0.0

Table 8.16 Sensitivity analysis estimates for reading, ITT

	# of studies	SMD	CI lower	CI upper	I^2	I^2 CI lower	I^2 CI upper
All studies	11	0.07	0.02	0.11	0.79	0.63	0.88
Exc Curto (2014)	10	0.06	0.01	0.11	0.79	0.62	0.88
Exc Bifulco (2009)	10	0.06	0.01	0.10	0.77	0.59	0.88
Exc Balsa (2016)	10	0.06	0.01	0.11	0.80	0.65	0.89
Exc Cullen (2006)	10	0.08	0.03	0.12	0.55	0.09	0.78
Exc Steele (2014)	10	0.05	0.00	0.11	0.71	0.45	0.85
Exc all studies above	6	0.03	0.01	0.06	0.00	0.00	0.65
Exc school size: 5 or less	6	0.05	-0.01	0.10	0.84	0.66	0.92
Inc studies with ITT & TOT	7	0.09	0.02	0.15	0.68	0.28	0.85

Table 8.17 Subgroup analysis estimates for reading, TOT

	SMD	CI lower	CI upper	Test for subgroup differences	
				Q	p-value
School type [a]					
Public	0.09	0.00	0.18	0.05	0.82
Other	0.08	0.01	0.15		
Intervention: policy level					
Educational system	0.11	0.05	0.16	6.31	0.01
School system	0.03	-0.17	0.23		
School sample size					
5 or less	0.12	-0.03	0.27		
More than 5	0.09	0.03	0.15	0.25	0.88
Not reported	0.10	-0.29	0.48		
ES calculations					
Assumptions were made	0.05	-0.04	0.14	4.28	0.04
Study reports all info	0.12	0.06	0.18		
Sample location					
Known	0.10	0.04	0.17	0.18	0.67
Unknown	0.08	-0.07	0.22		

Note a: this analysis excludes one study evaluating both public and charter schools (Hastings, 2012).

Table 8.18 Meta-regression estimates for reading, TOT

	estimate	std error	p-value	I^2	R^2
Intervention: policy level (ref cat: Ed system)					
School system	-0.06	0.08	0.44	80.0	0.0
ES calculations (ref cat: Assump. were made)					
Study reports all info	0.08	0.06	0.18	80.8	0.0
Year of publication	0.00	0.01	0.69	80.5	0.0

Table 8.19 Sensitivity analysis estimates for reading, TOT

	# of studies	SMD	CI lower	CI upper	I^2	I^2 CI lower	I^2 CI upper
All studies	16	0.10	0.05	0.15	0.80	0.68	0.87
Exc Abdulkadiroglu (2017)	15	0.09	0.04	0.14	0.75	0.59	0.85
Exc Bifulco (2009)	15	0.09	0.04	0.14	0.78	0.65	0.87
Exc Dynarski (2018)	15	0.11	0.05	0.16	0.80	0.68	0.87
Exc Hastings (2012)	15	0.09	0.04	0.13	0.76	0.60	0.85
Exc Steele (2014)	15	0.09	0.04	0.14	0.78	0.65	0.87
Exc all studies above	11	0.05	0.03	0.08	0.12	0.00	0.52
Exc Policy level: school	14	0.11	0.05	0.16	0.81	0.70	0.88
Exc ES calc: assumptions made	13	0.12	0.06	0.18	0.83	0.72	0.89
Exc high risk of selection bias	13	0.08	0.03	0.13	0.78	0.62	0.87
Inc studies with ITT & TOT	7	0.12	0.03	0.22	0.79	0.57	0.90

8.14. Estimates for meta-biases analyses

Table 8.20 and *Table 8.21* show estimates for the evaluation of potential publication bias in the set of studies included in the quantitative synthesis using subgroup and correlation analyses, respectively. *Table 8.22* and *Table 8.23* show the results of meta-regression and subgroup analyses based on the individual and overall Risk of Bias assessment of master records included in the quantitative synthesis. Shaded sections indicate statistically significant results, which are discussed in the main body of the results.

Table 8.20 Publication bias assessment using subgroup analysis

Publication type	SMD	CI lower	CI upper	Test for subgroup differences	
				Q	p-value
Math ITT					
Published	0.05	-0.02	0.11	0.23	0.63
Grey literature	0.03	0.00	0.07		
Reading ITT					
Published	0.10	-0.01	0.21	2.24	0.13
Grey literature	0.03	0.00	0.06		
Math TOT					
Published	0.13	0.01	0.25	2.03	0.15
Grey literature	0.06	0.02	0.09		
Reading TOT					
Published	0.12	0.04	0.20	1.34	0.25
Grey literature	0.07	-0.01	0.15		

Table 8.21 Publication bias assessment using correlations

	average # of schools	average effect size	correlation estimate	t statistic	df	p-value	CI lower	CI upper
Pooled	21.2	0.09	-0.06	-0.39	45	0.70	-0.34	0.23
Math ITT	20.9	0.05	-0.09	-0.25	8	0.81	-0.68	0.57
Math TOT	20.8	0.09	0.22	0.77	12	0.46	-0.36	0.67
Reading ITT	20.9	0.11	-0.38	-1.17	8	0.27	-0.82	0.33
Reading TOT	22.0	0.11	-0.22	-0.74	11	0.48	-0.69	0.38

Note: All correlations estimates used complete observations.

Table 8.22 Meta-bias assessment using meta-regressions

	estimate	std error	p-value	I^2	R^2
Math ITT					
Selection	-0.04	0.03	0.28	24.8	29.2
Baseline	*				
Identification	*				
Compliance	0.09	0.09	0.34	36.3	0.0
Attrition	0.09	0.09	0.34	36.3	0.0
Contamination	0.09	0.09	0.34	36.3	0.0
Sample reporting	*				
Outcome reporting	0.01	0.04	0.80	40.6	0.0
Reading ITT					
Selection	-0.09	0.04	0.08	63.1	50.4
Baseline	*				
Identification	*				
Compliance	0.02	0.06	0.77	80.3	0.0
Attrition	0.00	0.09	0.98	80.6	0.0
Contamination	0.02	0.06	0.77	80.3	0.0
Sample reporting	0.03	0.07	0.70	80.7	0.0
Outcome reporting	0.04	0.05	0.46	80.6	0.0
Math TOT					
Selection	-0.01	0.08	0.89	86.3	0.0
Baseline	*				
Identification	0.07	0.08	0.40	85.8	0.0
Compliance	0.03	0.11	0.76	86.5	0.0
Attrition	0.03	0.11	0.76	86.5	0.0
Contamination	0.03	0.11	0.76	86.5	0.0
Sample reporting	-0.06	0.21	0.77	86.5	0.0
Outcome reporting	0.07	0.11	0.55	86.1	0.0
Reading TOT					
Selection	-0.14	0.06	0.04	75.1	27.7
Baseline	*				
Identification	0.06	0.07	0.41	81.0	0.0
Compliance	0.03	0.09	0.78	81.3	0.0
Attrition	0.03	0.09	0.78	81.3	0.0
Contamination	0.03	0.09	0.78	81.3	0.0
Sample reporting	0.09	0.12	0.47	81.2	0.0
Outcome reporting	0.08	0.09	0.37	79.9	0.0

Note: For each meta-regression, the category of reference is high risk of bias; hence, estimates with an * indicate an absence of studies with high risk of such bias.

Table 8.23 Meta-bias assessment using subgroup analysis

Risk of Bias	SMD	CI lower	CI upper	Test for subgroup differences	
				Q	p-value
Math ITT					
Overall RoB: 2 categories					
High/Moderate	0.04	0.01	0.07	0.01	0.91
Low	0.04	-0.07	0.15		
Reading ITT					
Overall RoB: 3 categories					
High	0.05	-0.10	0.20		
Moderate	0.10	-0.02	0.21	1.35	0.51
Low	0.06	-0.07	0.18		
Overall RoB: 2 categories					
High/Moderate	0.08	0.01	0.14	0.20	0.65
Low	0.06	-0.07	0.18		
Math TOT					
Overall RoB: 2 categories					
High/Moderate	0.06	0.02	0.11	1.34	0.25
Low	0.12	0.02	0.22		
Reading TOT					
Overall RoB: 2 categories					
High/Moderate	0.10	0.00	0.19	0.01	0.91
Low	0.10	0.03	0.18		

9. Appendix Chapter 3

9.1. Description of variables used in the analysis

Table 9.1 Description and values of variables

Student-level variables	
Reading	SIMCE reading test scores (standardised)
Math	SIMCE math test scores (standardised)
Male	Gender: 1=male, 0=female
SEN	Special Educational Needs: 1 if student has SEN, 0 otherwise
Attendance	Percentage of attendance
Passed	Academic situation: 1=passed the grade, 0 otherwise
Failed	Academic situation: 1=failed the grade, 0 otherwise
Left	Academic situation: 1=left school before end of the year, 0 otherwise
Retention	Grade retention: 1=student is repeating the grade, 0 otherwise
Computer at home	Computer at home: 1=household has a computer, 0 otherwise
Internet at home	Internet connection at home: 1=household has Internet, 0 otherwise
Books at home	Total number of books in student's household: 1=None, 2=Less than 10, 3=Between 10-50, 4=Between 51-100, 5=More than 100
Father's education Mother's education	Highest educational level of the student's father and mother: 1=Some primary education or less, 2=Primary education completed, 3=Some secondary or vocational education, 4=Secondary or vocational education completed, 5=Some higher education, 6=Higher education completed, 7=Postgraduate education
Household income	Average monthly income of the student's household (in pounds): 1=Less than £100, 2=Between £101-£200, 3=Between £201-£300, 4=Between £301-£400, 5=Between £401-£500, 6=Between £501-£600, 7=Between £601-£800, 8=Between £801-£1000, 9=Between £1001-£1200, 10=Between £1201-£1400, 11=Between £1,401-£1,600, 12=Between £1,601-£1,800, 13=Between £1,801-£2,000, 14=Between £2,001-£2,200, 15=More than £2,200
School-level variables	
Entry test Parent interview Transcript Preschool Play session Marriage Baptismal Salary	School admission mechanisms: 1=if the school required the mechanism in the admission process, 0 otherwise
Enrolment	Total number of students enrolled in the school
Public	School type: 1=public school, 0 otherwise
Private-subsidised	School type: 1= private-subsidised school, 0 otherwise
Private	School type: 1=private school, 0 otherwise
Grade4	School grade: 1=4th grade (10-year-olds)
Grade8	School grade: 1=8th grade (14-year-olds)

9.2. Robustness check using complete cases

As a robustness analysis, the primary models were also estimated using only complete cases regarding the student-level variables, for which missing data was originally imputed with the grade mean of each school. The variables include test scores, special educational needs, academic status, grade retention, highest educational level of mother and father, number of books at home, computer and internet access at home, and household average monthly income. Considering only complete cases on these variables leaves a sample of 8,209 schools and 1,966,353 students.

As shown in *Table 9.2*, the results of the models with only complete cases are not substantially different from the main models beyond presenting slightly smaller coefficients. Consequently, the imputation strategy discussed in section *3.3* was successful in fulfilling its original purpose of avoiding a reduced research sample and achieving an accurate number of students per grade, school, and year.

Table 9.2 Effect of selective admissions (only complete cases)

	Reading (1)	Math (2)	SES Index (3)
Use of Entry Tests			
Always users	0.04 **	0.04 **	0.02 **
Started / Resumed	0.01	0.05 **	0.00
N° schools	8,109	8,107	8,209
N° students	1,960,820	1,960,895	1,964,983
Use of Parent Interviews			
Always users	0.03 *	0.04 *	0.03 **
Started / Resumed	0.02 *	0.02 *	-0.01 *
N° schools	8,109	8,107	8,209
N° students	1,960,820	1,960,895	1,964,983

Notes: Dependent variable: SIMCE standardised test scores (columns 1 & 2) and school socioeconomic index (column 3). These specifications include the same student- and school-level control variables, as well school and year fixed effects. ** p<0.01, * p<0.05.

9.3. Socioeconomic index validation

After creating the socioeconomic (SES) index using principal components analysis, I conducted a validation exercise to assess this measure. *Table 9.3* shows the mean statistics of a range of relevant variables according to different categories of the SES index, which were constructed based on standard deviations from the index mean. Values lower than -0.5 standard deviations from the mean were considered low SES (35% of cases); values between -0.5 and 0.5 standard deviations from the mean were classified as medium SES (36% of cases); and values above 0.5 standard deviations from the mean were considered high SES (28% of cases).

For all these variables, the differences between the levels of SES index (low, medium and high) are evident. Students with a high SES index show higher test scores and better academic and family characteristics, compared to students with low SES index. In the same line, students with a high SES index are more likely to attend private and private-subsidised schools, which use in greater proportion entry tests and parent interviews in their admission processes. Hence, the socioeconomic index would be capturing different social contexts of students in the sample.

Table 9.3 Mean statistics for the overall sample and by SES index groups

	Overall sample	SES index		
		Low	Med	High
Reading score	257.0	240.3	255.2	280.7
Math score	252.5	233.0	250.1	280.7
SEN	0.03	0.04	0.03	0.01
Attendance	0.89	0.89	0.89	0.91
Approved	0.93	0.92	0.93	0.95
Failed	0.03	0.03	0.03	0.02
Left	0.04	0.05	0.04	0.03
Retention	0.01	0.02	0.01	0.00
Computer at home	0.60	0.30	0.66	0.91
Internet at home	0.39	0.10	0.39	0.77
No. of books at home	2.84	2.39	2.83	3.43
Father's highest education	3.62	2.61	3.58	4.99
Mother's highest education	3.59	2.57	3.57	4.94
Household monthly income	3.93	2.15	3.24	7.16
Use of entry tests	0.27	0.04	0.23	0.62
Use of parent interviews	0.19	0.02	0.10	0.54
Public school	0.47	0.77	0.45	0.11
Private-subsidised school	0.46	0.23	0.55	0.66
Private school	0.07	0.00	0.00	0.24

Notes: The SES index was constructed using PCA with six variables: computer, Internet and number of books at home, highest educational level of mother and father, and average household income. The three categories of the SES index were constructed based on standard deviations from its mean.

9.4. Summary of main models

The following tables show a series of regression models, from cross-sectional to the final model used in the main analyses, for reading test scores (*Table 9.4*), math test scores (*Table 9.5*), and the school/grade socioeconomic index (*Table 9.6*). In each table, Panel A shows the results for the use of Entry Tests, Panel B presents the results for the use of Parent Interviews, and the final panel indicates the sample size and specification details for each model.

Table 9.4 Summary of models for reading

	Outcome: reading standardised test scores				
	(1)	(2)	(3)	(4)	(5)
Panel A: Use of Entry Tests					
Cross-sectional use	0.06**				
	(0.01)				
Always users		0.11**	0.09**	0.05**	0.05**
		(0.01)	(0.01)	(0.01)	(0.01)
Started / Resumed		0.01	0.00	-0.02*	0.01
		(0.01)	(0.01)	(0.01)	(0.01)
Stopped		-0.01	-0.01	-0.03**	-0.01
		(0.01)	(0.01)	(0.01)	(0.01)
Constant	-0.12**	-0.12**	-0.67**	-0.64**	-0.64**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
R^2 within	0.00	0.00	0.04	0.04	0.05
R^2 between	0.31	0.27	0.43	0.43	0.43
R^2 overall	0.07	0.07	0.17	0.16	0.17
Panel B: Use of Parent Interviews					
Cross-sectional use	0.04**				
	(0.01)				
Always users		0.12**	0.10**	0.06**	0.06**
		(0.01)	(0.01)	(0.01)	(0.01)
Started / Resumed		0.01	0.00	-0.01	0.02**
		(0.01)	(0.01)	(0.01)	(0.01)
Stopped		0.02*	0.01	0.00	0.03**
		(0.01)	(0.01)	(0.01)	(0.01)
Constant	-0.11**	-0.12**	-0.67**	-0.65**	-0.64**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
R^2 within	0.00	0.00	0.04	0.04	0.05
R^2 between	0.21	0.20	0.43	0.43	0.43
R^2 overall	0.07	0.07	0.17	0.17	0.17
N students	3,941,194	3,941,194	3,300,108	3,300,108	3,300,108
N schools	8,155	8,155	8,114	8,114	8,114
Categories of use	NO	YES	YES	YES	YES
Controls	NO	NO	YES	YES	YES
School FE	NO	NO	NO	YES	YES
Year FE	NO	NO	NO	NO	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05.

Table 9.5 Summary of models for math

	Outcome: math standardised test scores				
	(1)	(2)	(3)	(4)	(5)
Panel A: Use of Entry Tests					
Cross-sectional use	0.08**				
	(0.01)				
Always users		0.14**	0.14**	0.09**	0.09**
		(0.01)	(0.01)	(0.01)	(0.01)
Started / Resumed		0.05**	0.04**	0.02	0.05**
		(0.01)	(0.01)	(0.01)	(0.01)
Stopped		0.02	0.03*	0.00	0.03*
		(0.01)	(0.01)	(0.01)	(0.01)
Constant	-0.19**	-0.20**	-0.91**	-0.82**	-0.82**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
R^2 within	0.00	0.00	0.05	0.05	0.05
R^2 between	0.35	0.33	0.53	0.52	0.53
R^2 overall	0.10	0.11	0.22	0.22	0.22
Panel B: Use of Parent Interviews					
Cross-sectional use	0.04**				
	(0.01)				
Always users		0.11**	0.10**	0.06**	0.06**
		(0.01)	(0.01)	(0.02)	(0.02)
Started / Resumed		0.02	0.01	-0.01	0.03**
		(0.01)	(0.01)	(0.01)	(0.01)
Stopped		0.01	0.01	0.00	0.03**
		(0.01)	(0.01)	(0.01)	(0.01)
Constant	-0.19**	-0.19**	-0.91**	-0.82**	-0.81**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
R^2 within	0.00	0.00	0.05	0.05	0.05
R^2 between	0.23	0.22	0.52	0.52	0.52
R^2 overall	0.09	0.09	0.22	0.22	0.22
N students	3,941,409	3,941,409	3,300,357	3,300,357	3,300,357
N schools	8,155	8,155	8,113	8,113	8,113
Categories of use	NO	YES	YES	YES	YES
Controls	NO	NO	YES	YES	YES
School FE	NO	NO	NO	YES	YES
Year FE	NO	NO	NO	NO	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05.

Table 9.6 Summary of models for socioeconomic index

	Outcome: school/grade SES index				
	(1)	(2)	(3)	(4)	(5)
Panel A: Use of Entry Tests					
Cross-sectional use	-0.02*				
	(0.01)				
Always users		0.00	0.02	0.01	0.02*
		(0.01)	(0.01)	(0.01)	(0.01)
Started / Resumed		0.27**	0.22**	0.21**	-0.00
		(0.01)	(0.01)	(0.01)	(0.01)
Stopped		0.28**	0.22**	0.22**	-0.02*
		(0.01)	(0.01)	(0.01)	(0.01)
Constant	-0.44**	-0.47**	-0.12**	0.52**	0.43**
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)
R^2 within	0.00	0.04	0.16	0.16	0.58
R^2 between	0.42	0.15	0.04	0.05	0.10
R^2 overall	0.29	0.06	0.01	0.02	0.07
Panel B: Use of Parent Interviews					
Cross-sectional use	0.08**				
	(0.01)				
Always users		0.01	0.06**	0.05**	0.03**
		(0.01)	(0.01)	(0.01)	(0.01)
Started / Resumed		0.29**	0.25**	0.25**	-0.01
		(0.01)	(0.01)	(0.01)	(0.01)
Stopped		0.25**	0.20**	0.19**	-0.02**
		0.01	0.06**	0.05**	0.03**
Constant	-0.46**	-0.49**	-0.12**	0.54**	0.43**
	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)
R^2 within	0.00	0.03	0.16	0.16	0.58
R^2 between	0.40	0.01	0.11	0.12	0.14
R^2 overall	0.36	0.06	0.03	0.03	0.08
N students	3,945,578	3,945,578	3,305,584	3,305,584	3,305,584
N schools	8,233	8,233	8,213	8,213	8,213
Categories of use	NO	YES	YES	YES	YES
Controls	NO	NO	YES	YES	YES
School FE	NO	NO	NO	YES	YES
Year FE	NO	NO	NO	NO	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05.

10. Appendix Chapter 4

10.1. School sample

Table 10.1 shows full details of the sample used in the analyses, from the original list of applicants provided by the school, to the final enrolment of students according to the Ministry of Education's official data.

From the school lists, some applications had an invalid or no ID (3% overall the two cohorts, n=48), so they could not be linked with MoE's data. From those applicants with valid IDs, a further 3% (n=54) was not found in the MoE's data, and 4% (n=78) of students in the final dataset was not in the original school lists. Moreover, 30 observations identified in the data for cohort 2016 were applicants from cohort 2015 that did not finally enrol in the school. Given the small number of these observations and that I aimed to identify cases to a unique cohort, these cases were excluded from the analysis sample.

Despite being a hand-crafted process managed entirely by the school, their applicants' lists identified 96% of the students who ultimately attended the school. This data management process yielded a total of 830 applicants attending the school and 1,032 attending other schools.

Table 10.1 Detailed school sample from applicants list to enrolment, per cohort

	Original school list			Linkage with MoE's data					Enrolment	
Grade	Applicants	Invalid ID	Valid applicants	Not matched	From prev. cohort	Matched	From MoE's data	Total linked	Enrolled (T)	Not enrolled (C)
Cohort 2015										
PK	117	5	112	4	-	108	3	111	66	45
K	100	6	94	3	-	91	14	105	84	21
1st	122	9	113	0	-	113	6	119	89	30
2nd	130	7	123	2	-	121	8	129	85	44
3rd	124	4	120	4	-	116	5	121	87	34
4th	131	5	126	1	-	125	9	134	91	43
5th	-	-	-	-	-	-	-	-	-	-
9th	163	8	155	2	-	153	17	170	116	54
10th	-	-	-	-	-	-	-	-	-	-
Total	887	44	843	16	-	827	62	889	618	271
Cohort 2016										
PK	235	4	231	14	1	216	5	221	70	151
K	131	0	131	5	11	115	3	118	18	100
1st	137	0	137	5	0	132	0	132	11	121
2nd	82	0	82	3	1	78	1	79	12	67
3rd	73	0	73	2	6	65	0	65	10	55
4th	68	0	68	1	4	63	2	65	11	54
5th	71	0	71	2	5	64	1	65	3	62
9th	232	0	232	6	2	224	4	228	77	151
10th	0	0	0	0	0	0	0	0	0	0
Total	1,029	4	1,025	38	30	957	16	973	212	761
Pooled total	1,916	48	1,868	54	30	1,784	78	1,862	830	1,032

10.2. Main analysis outputs

Table 10.2 shows the main results for the effects on attendance for each year evaluated. Odd columns show the output without controls, and even columns incorporate controls at the student and school level. *Table 10.3* to *Table 10.7* present the same information for the outcomes final mark, test scores, and passing grade, respectively. All analyses were conducted using Stata, version 15 (StataCorp, 2017; Jann, 2014).

Table 10.2 Effect on attendance, with and without control variables

VARIABLES	Year 0 (1)	Year 0 (2)	Year 1 (3)	Year 1 (4)	Year 2 (5)	Year 2 (6)	Year 3 (7)	Year 3 (8)
Treatment	0.29	0.27	-2.72**	-1.97**	-1.17**	-0.94**	-0.46	-0.27
	(0.48)	(0.51)	(0.32)	(0.33)	(0.30)	(0.34)	(0.38)	(0.39)
Male		1.09*		-0.33		0.15		-0.36
		(0.51)		(0.34)		(0.31)		(0.45)
Vulnerable		-0.19		-0.72*		0.50		0.35
		(0.38)		(0.30)		(0.78)		(0.47)
Grade t_0		0.51**		0.37*		-0.12		-0.22*
		(0.13)		(0.16)		(0.07)		(0.11)
Mark t_0		3.59**		1.74**		0.71*		1.21**
		(0.42)		(0.54)		(0.34)		(0.44)
Attendance t_0				0.35**		0.32**		0.31**
				(0.05)		(0.07)		(0.08)
Passed t_0		6.82*		-6.81**		-4.08**		-6.46**
		(3.32)		(1.49)		(1.41)		(1.32)
School type t_0		0.92		0.72*		0.28		0.85
		(0.75)		(0.32)		(0.34)		(0.43)
Rural school t_0		1.04		0.75		0.71		-0.33
		(0.85)		(0.71)		(0.50)		(1.28)
Constant	92.89**	59.75**	93.59**	54.37**	93.79**	63.31**	93.57**	63.45**
	(0.37)	(6.49)	(0.32)	(6.79)	(0.27)	(6.37)	(0.30)	(8.01)
Observations	1,048	1,048	1,306	1,041	1,493	1,022	1,808	1,020
R-squared	0.00	0.15	0.04	0.21	0.01	0.16	0.00	0.13
Controls	NO	YES	NO	YES	NO	YES	NO	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.3 Effect on final mark, with and without control variables

VARIABLES	Year 0 (1)	Year 0 (2)	Year 1 (3)	Year 1 (4)	Year 2 (5)	Year 2 (6)	Year 3 (7)	Year 3 (8)
Treatment	-0.05	-0.06	-0.12*	-0.05*	-0.08	-0.03	-0.03	0.01
	(0.04)	(0.03)	(0.05)	(0.03)	(0.05)	(0.02)	(0.04)	(0.03)
Male		-0.19**		-0.14**		-0.14**		-0.14**
		(0.03)		(0.02)		(0.02)		(0.03)
Vulnerable		-0.08*		-0.11**		-0.06*		-0.03
		(0.04)		(0.04)		(0.03)		(0.03)
Grade t_0		-0.10**		-0.03**		-0.01		0.00
		(0.01)		(0.01)		(0.01)		(0.01)
Mark t_0				0.80**		0.65**		0.63**
				(0.02)		(0.03)		(0.05)
Attendance t_0		0.02**		-0.00		-0.00		-0.00
		(0.00)		(0.00)		(0.00)		(0.00)
Passed t_0		1.10**		-0.97**		-0.60**		-0.60**
		(0.11)		(0.10)		(0.09)		(0.10)
School type t_0		0.05		0.13**		0.11**		0.10*
		(0.04)		(0.05)		(0.04)		(0.04)
Rural school t_0		0.12*		-0.14		-0.05		-0.10*
		(0.05)		(0.08)		(0.05)		(0.05)
Constant	5.89**	3.66**	5.87**	2.26**	5.92**	2.94**	5.94**	2.62**
	(0.03)	(0.26)	(0.05)	(0.35)	(0.04)	(0.34)	(0.04)	(0.30)
Observations	1,048	1,048	1,306	1,041	1,493	1,022	1,808	1,020
R-squared	0.00	0.41	0.01	0.63	0.00	0.52	0.00	0.44
Controls	NO	YES	NO	YES	NO	YES	NO	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.4 Effect on test scores, with and without control variables

VARIABLES	Year 1 (1)	Year 1 (2)	Year 2 (3)	Year 2 (4)
			Reading	
Treatment	-2.82	-0.76	-11.87*	-15.38**
	(5.56)	(4.51)	(5.74)	(4.20)
Male		5.02		-15.07**
		(5.35)		(4.65)
Vulnerable		-6.27		-8.52
		(6.16)		(15.42)
Mark t_0		49.13**		60.87**
		(5.59)		(9.61)
Attendance t_0		-0.01		-1.65**
		(0.54)		(0.26)
Passed t_0		9.11		-88.21**
		(15.35)		(14.90)
School type t_0		9.40		-0.06
		(5.72)		(10.50)
Rural school t_0		-4.18		-20.57
		(5.96)		(10.55)
Constant	266.84**	-53.22	261.82**	147.64**
	(5.56)	(36.23)	(5.92)	(24.91)
Observations	181	180	162	161
R-squared	0.00	0.33	0.01	0.34
Controls	NO	YES	NO	YES
			Math	
Treatment	-8.69	-7.18	-2.10	-6.78
	(5.63)	(4.95)	(7.74)	(5.64)
Male		14.28**		4.49
		(4.76)		(7.88)
Vulnerable		-4.00		-3.19
		(5.86)		(8.96)
Mark t_0		53.67**		72.46**
		(3.49)		(9.04)
Attendance t_0		0.27		-1.15**
		(0.29)		(0.32)
Passed t_0		-70.98*		-80.48**
		(30.96)		(27.62)
School type t_0		6.61		5.21
		(7.12)		(8.05)
Rural school t_0		-4.20		-18.08
		(4.84)		(13.96)
Constant	265.09**	-31.87	256.56**	-6.75
	(5.63)	(31.91)	(8.00)	(40.62)
Observations	180	179	162	161
R-squared	0.01	0.34	0.00	0.40
Controls	NO	YES	NO	YES

Notes: Since only test scores for 4th graders were examined, these models do not control for grade. Scores are set to have a mean of 250 and a standard deviation of 50 points. Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.5 Effect on passing, with and without control variables

VARIABLES	Year 0 (1)	Year 0 (2)	Year 1 (3)	Year 1 (4)	Year 2 (5)	Year 2 (6)	Year 3 (7)	Year 3 (8)
Treatment	0.00	0.02*	-0.09**	-0.08**	-0.05**	-0.04**	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Male		-0.00		-0.03**		-0.03**		-0.00
		(0.01)		(0.01)		(0.01)		(0.01)
Vulnerable		-0.01*		-0.02		-0.02*		0.02
		(0.01)		(0.01)		(0.01)		(0.01)
Grade t_0		0.01**		-0.01		-0.01**		-0.00
		(0.00)		(0.00)		(0.00)		(0.00)
Mark t_0		0.09**		0.19**		0.06**		0.05*
		(0.02)		(0.04)		(0.01)		(0.02)
Attendance t_0		0.00*		-0.00		-0.00		0.00
		(0.00)		(0.00)		(0.00)		(0.00)
Passed t_0				-0.29**		-0.06		-0.02
				(0.08)		(0.04)		(0.06)
School type t_0		-0.01		0.04		0.04**		-0.00
		(0.01)		(0.02)		(0.02)		(0.01)
Rural school t_0		-0.01		-0.08**		-0.01		-0.04
		(0.01)		(0.03)		(0.02)		(0.03)
Constant	0.97**	0.09	0.95**	0.11	0.97**	0.74**	0.98**	0.69**
	(0.01)	(0.20)	(0.01)	(0.25)	(0.01)	(0.12)	(0.01)	(0.11)
Observations	1,054	1,048	1,314	1,048	1,526	1,045	1,834	1,039
R-squared	0.00	0.17	0.02	0.20	0.01	0.08	0.00	0.03
Controls	NO	YES	NO	YES	NO	YES	NO	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.6 Effect on passing by grade in Year 1

VARIABLES	Y1 2nd grade (1)	Y1 3rd grade (2)	Y1 4th grade (3)	Y1 9th grade (4)
Treatment	-0.01	-0.03	-0.02**	-0.12**
	(0.02)	(0.02)	(0.01)	(0.03)
Male	-0.02	-0.03	0.01	-0.09**
	(0.02)	(0.02)	(0.01)	(0.02)
Vulnerable	0.04	-0.03*	-0.01	-0.07**
	(0.03)	(0.01)	(0.01)	(0.03)
Grade t_0	0.34**	0.35**	-0.19*	0.32
	(0.07)	(0.11)	(0.09)	(0.33)
Mark t_0	0.20**	0.11**	0.00	0.31**
	(0.05)	(0.04)	(0.01)	(0.07)
Attendance t_0	-0.00	0.00	0.01**	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
School type t_0	-0.01	0.04	-0.03	0.12**
	(0.03)	(0.03)	(0.02)	(0.03)
Rural school t_0	-0.04	-0.02	-0.11**	-0.17**
	(0.09)	(0.05)	(0.04)	(0.05)
Constant	-1.29*	-1.48*	1.51**	-3.87
	(0.60)	(0.69)	(0.45)	(3.63)
Observations	202	186	190	381
R-squared	0.16	0.10	0.24	0.27
Controls	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.7 Effect on passing for first cohort of 9th graders by year

VARIABLES	Year 0 (1)	Year 1 (2)	Year 2 (3)	Year 3 (4)
Treatment	0.02	-0.12**	-0.01	0.03
	(0.02)	(0.03)	(0.02)	(0.02)
Male	-0.00	-0.09**	-0.06*	-0.02
	(0.01)	(0.02)	(0.03)	(0.02)
Vulnerable	-0.01	-0.07*	-0.00	0.01
	(0.01)	(0.03)	(0.04)	(0.03)
Mark t_0	0.02	0.30**	0.11**	0.07*
	(0.02)	(0.06)	(0.03)	(0.03)
Attendance t_0	0.00	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
School type t_0	-0.01	0.13**	0.11	0.02
	(0.01)	(0.04)	(0.05)	(0.02)
Rural school t_0	-0.01	-0.17**	0.10*	-0.15
	(0.01)	(0.05)	(0.04)	(0.08)
Constant	0.80**	-0.65	0.09	0.16
	(0.16)	(0.45)	(0.24)	(0.33)
Observations	380	381	305	283
R-squared	0.04	0.26	0.09	0.06
Controls	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

10.3. Robustness check: observations source

The following tables (*Table 10.8* to *Table 10.10*) show the full results for the sensitivity analyses considering the source of the sample observations (the school list or the Ministry of Education's data) for each of the main outcomes. While 96% of students were identified through the application lists provided by the school, a small group of students (4% overall the two cohorts) was subsequently identified through the MoE's data. These results remain largely consistent with the main analyses in terms of their magnitude and significance.

Table 10.8 Effect on attendance controlling for the source of observations

VARIABLES	Year 0 (1)	Year 1 (2)	Year 2 (3)	Year 3 (4)
Treatment	0.40	-1.89**	-0.93**	-0.21
	(0.52)	(0.33)	(0.31)	(0.39)
Male	1.06*	-0.34	0.14	-0.37
	(0.51)	(0.35)	(0.31)	(0.46)
Vulnerable	-0.19	-0.72*	0.49	0.34
	(0.38)	(0.30)	(0.79)	(0.47)
Grade t_0	0.51**	0.37*	-0.12	-0.22*
	(0.13)	(0.16)	(0.07)	(0.11)
Mark t_0	3.58**	1.74**	0.71*	1.21**
	(0.43)	(0.54)	(0.34)	(0.44)
Attendance t_0		0.35**	0.32**	0.31**
		(0.05)	(0.07)	(0.08)
Passed t_0	6.82*	-6.80**	-4.08**	-6.46**
	(3.32)	(1.48)	(1.41)	(1.31)
School type t_0	0.91	0.72*	0.27	0.84
	(0.76)	(0.32)	(0.35)	(0.43)
Rural school t_0	1.06	0.77	0.72	-0.31
	(0.85)	(0.71)	(0.50)	(1.26)
Source	1.47	0.90**	0.12	0.76
	(1.16)	(0.13)	(0.73)	(0.40)
Constant	58.42**	53.63**	63.22**	62.81**
	(6.49)	(6.66)	(6.07)	(7.87)
Observations	1,048	1,041	1,022	1,020
R-squared	0.15	0.21	0.16	0.13
Controls	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.9 Effect on final mark controlling for the source of observations

VARIABLES	Year 0 (1)	Year 1 (2)	Year 2 (3)	Year 3 (4)
Treatment	-0.06	-0.05	-0.03	0.01
	(0.04)	(0.03)	(0.02)	(0.03)
Male	-0.19**	-0.14**	-0.14**	-0.14**
	(0.03)	(0.02)	(0.02)	(0.03)
Vulnerable	-0.08*	-0.11**	-0.06*	-0.03
	(0.04)	(0.04)	(0.03)	(0.03)
Grade t_0	-0.10**	-0.03**	-0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Mark t_0		0.80**	0.65**	0.63**
		(0.02)	(0.03)	(0.05)
Attendance t_0	0.02**	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Passed t_0	1.10**	-0.97**	-0.60**	-0.60**
	(0.11)	(0.10)	(0.09)	(0.10)
School type t_0	0.05	0.13**	0.11**	0.10**
	(0.04)	(0.05)	(0.04)	(0.04)
Rural school t_0	0.12*	-0.14	-0.05	-0.10
	(0.06)	(0.08)	(0.05)	(0.05)
Source	0.04	0.04**	-0.01	0.02
	(0.08)	(0.01)	(0.06)	(0.04)
Constant	3.63**	2.23**	2.95**	2.60**
	(0.28)	(0.34)	(0.37)	(0.30)
Observations	1,048	1,041	1,022	1,020
R-squared	0.41	0.63	0.52	0.44
Controls	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.10 Effect on passing controlling for the source of observations

VARIABLES	Year 0 (1)	Year 1 (2)	Year 2 (3)	Year 3 (4)
Treatment	0.02*	-0.07**	-0.04**	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Male	-0.00	-0.03**	-0.03**	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Vulnerable	-0.01*	-0.02	-0.02*	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Grade t_0	0.01**	-0.01	-0.01**	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Mark t_0	0.09**	0.19**	0.06**	0.05*
	(0.02)	(0.04)	(0.01)	(0.02)
Attendance t_0	0.00*	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Passed t_0		-0.29**	-0.06	-0.02
		(0.08)	(0.04)	(0.06)
School type t_0	-0.01	0.04	0.04**	-0.00
	(0.01)	(0.02)	(0.02)	(0.01)
Rural school t_0	-0.01	-0.08**	-0.01	-0.04
	(0.01)	(0.03)	(0.02)	(0.03)
Source	-0.00	0.05**	0.01	0.01
	(0.02)	(0.01)	(0.02)	(0.04)
Constant	0.09	0.06	0.73**	0.68**
	(0.20)	(0.24)	(0.13)	(0.14)
Observations	1,048	1,048	1,045	1,039
R-squared	0.17	0.20	0.08	0.03
Controls	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

10.4. Robustness check: siblings and twins

The following tables show the full results for the robustness analyses considering the cases of siblings and twins within the sample. Since this information is only available for the first cohort, columns 1-4 in each table show what would be the main effect considering only cohort 2015, while columns 5-8 present the results when controlling for either the group of siblings in the data (roughly 30%) or the group of twins in the sample (five pairs). These are shown for the effects on attendance (*Table 10.11* and *Table 10.12*), final mark (*Table 10.13* and *Table 10.14*), and passing grade (*Table 10.15* and *Table 10.16*), respectively. Results indicate that there are no systematic differences when incorporating the information on siblings and twins in the analyses. Then it could be assumed that if I had the data for the entire sample (using both cohorts), these analyses would remain consistent.

Table 10.11 Effect on attendance controlling for siblings

	Cohort 2015				Controlling for siblings			
	Year 0	Year 1	Year 2	Year 3	Year 0	Year 1	Year 2	Year 3
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.01	-0.98	-0.35	0.75	0.18	-0.76	-0.32	0.91
	(0.60)	(0.59)	(0.47)	(0.61)	(0.62)	(0.65)	(0.46)	(0.63)
Male	0.91	-0.53	0.05	-0.51	0.76	-0.87*	0.16	-0.53
	(0.55)	(0.34)	(0.35)	(0.40)	(0.55)	(0.33)	(0.35)	(0.43)
Vulnerable	0.75	-0.83*	0.46	0.16	0.77	-0.78*	0.08	-0.13
	(0.66)	(0.39)	(0.80)	(0.71)	(0.72)	(0.38)	(0.70)	(0.68)
Grade t_0	0.64**	0.47**	-0.02	-0.19	0.68**	0.45**	-0.04	-0.20
	(0.11)	(0.13)	(0.09)	(0.14)	(0.11)	(0.14)	(0.09)	(0.16)
Mark t_0	3.63**	2.52**	1.40**	1.69**	3.56**	2.09**	1.27**	1.65**
	(0.39)	(0.59)	(0.42)	(0.55)	(0.39)	(0.68)	(0.40)	(0.52)
Attendance t_0		0.46**	0.43**	0.43**		0.44**	0.44**	0.44**
		(0.05)	(0.04)	(0.07)		(0.05)	(0.05)	(0.07)
Passed t_0	6.80*	-10.35**	-6.51**	-8.58**	6.31*	-9.13**	-6.31**	-8.66**
	(2.91)	(1.91)	(1.42)	(1.71)	(3.08)	(2.42)	(1.49)	(1.80)
School type t_0	0.73	1.08*	0.31	0.27	0.46	1.33*	0.10	0.12
	(0.89)	(0.42)	(0.51)	(0.55)	(1.04)	(0.52)	(0.52)	(0.59)
Rural school t_0	-0.75	0.43	0.14	-0.18	-1.07	0.26	0.22	-0.33
	(1.18)	(1.05)	(0.76)	(1.09)	(1.34)	(1.19)	(0.92)	(1.22)
Siblings					-0.14	-0.96	0.37	0.01
					(0.55)	(0.83)	(0.50)	(0.70)
Constant	58.84**	40.93**	50.37**	50.95**	60.14**	44.05**	51.09**	51.53**
	(4.73)	(6.63)	(4.77)	(7.82)	(5.09)	(7.42)	(5.22)	(8.33)
Observations	546	539	524	528	511	504	492	495
R-squared	0.20	0.24	0.25	0.22	0.19	0.24	0.25	0.22
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.12 Effect on attendance controlling for twins

	Cohort 2015				Controlling for twins			
	Year 0	Year 1	Year 2	Year 3	Year 0	Year 1	Year 2	Year 3
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.01	-0.98	-0.35	0.75	0.15	-0.92	-0.28	0.89
	(0.60)	(0.59)	(0.47)	(0.61)	(0.63)	(0.58)	(0.43)	(0.60)
Male	0.91	-0.53	0.05	-0.51	0.77	-0.77*	0.10	-0.54
	(0.55)	(0.34)	(0.35)	(0.40)	(0.56)	(0.33)	(0.32)	(0.42)
Vulnerable	0.75	-0.83*	0.46	0.16	0.74	-0.85*	0.05	-0.16
	(0.66)	(0.39)	(0.80)	(0.71)	(0.71)	(0.42)	(0.72)	(0.68)
Grade t_0	0.64**	0.47**	-0.02	-0.19	0.68**	0.46**	-0.04	-0.20
	(0.11)	(0.13)	(0.09)	(0.14)	(0.11)	(0.13)	(0.09)	(0.15)
Mark t_0	3.63**	2.52**	1.40**	1.69**	3.54**	2.08**	1.25**	1.64**
	(0.39)	(0.59)	(0.42)	(0.55)	(0.39)	(0.69)	(0.38)	(0.51)
Attendance t_0		0.46**	0.43**	0.43**		0.44**	0.44**	0.43**
		(0.05)	(0.04)	(0.07)		(0.06)	(0.05)	(0.07)
Passed t_0	6.80*	-10.35**	-6.51**	-8.58**	6.37*	-9.04**	-6.24**	-8.59**
	(2.91)	(1.91)	(1.42)	(1.71)	(3.08)	(2.40)	(1.45)	(1.72)
School type t_0	0.73	1.08*	0.31	0.27	0.50	1.28**	0.21	0.20
	(0.89)	(0.42)	(0.51)	(0.55)	(1.01)	(0.47)	(0.52)	(0.59)
Rural school t_0	-0.75	0.43	0.14	-0.18	-1.05	0.31	0.21	-0.32
	(1.18)	(1.05)	(0.76)	(1.09)	(1.34)	(1.22)	(0.91)	(1.22)
Twins					-4.15	-4.39*	-5.37	-5.50
					(2.94)	(2.01)	(6.22)	(5.52)
Constant	58.84**	40.93**	50.37**	50.95**	60.16**	44.04**	51.44**	51.77**
	(4.73)	(6.63)	(4.77)	(7.82)	(5.10)	(7.65)	(5.25)	(8.40)
Observations	546	539	524	528	511	504	492	495
R-squared	0.20	0.24	0.25	0.22	0.19	0.24	0.26	0.22
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.13 Effect on final mark controlling for siblings

	Cohort 2015				Controlling for siblings			
	Year 0	Year 1	Year 2	Year 3	Year 0	Year 1	Year 2	Year 3
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.06	-0.06*	-0.04	-0.01	-0.06	-0.06*	-0.04	-0.02
	(0.05)	(0.03)	(0.03)	(0.05)	(0.06)	(0.03)	(0.04)	(0.05)
Male	-0.17**	-0.16**	-0.16**	-0.15**	-0.17**	-0.17**	-0.17**	-0.15**
	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
Vulnerable	-0.06	-0.10**	-0.11**	-0.06	-0.04	-0.09**	-0.11**	-0.05
	(0.05)	(0.03)	(0.03)	(0.05)	(0.06)	(0.03)	(0.03)	(0.05)
Grade t_0	-0.12**	-0.01	-0.01	0.01	-0.12**	-0.01*	-0.01	0.01
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Mark t_0		0.83**	0.65**	0.63**		0.81**	0.66**	0.64**
		(0.03)	(0.03)	(0.06)		(0.04)	(0.03)	(0.06)
Attendance t_0	0.03**	0.00	-0.00	0.00	0.03**	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Passed t_0	1.07**	-1.01**	-0.67**	-0.67**	1.14**	-0.96**	-0.68**	-0.66**
	(0.11)	(0.12)	(0.07)	(0.10)	(0.11)	(0.15)	(0.09)	(0.12)
School type t_0	0.12*	0.17**	0.18**	0.14*	0.12*	0.17**	0.19**	0.14*
	(0.05)	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)
Rural school t_0	0.08	-0.23**	0.00	-0.06	0.04	-0.23**	0.03	-0.03
	(0.09)	(0.07)	(0.04)	(0.08)	(0.10)	(0.08)	(0.05)	(0.09)
Siblings					-0.00	0.03	0.03	0.03
					(0.04)	(0.02)	(0.03)	(0.03)
Constant	3.10**	1.66**	2.71**	2.43**	3.10**	1.80**	2.61**	2.37**
	(0.40)	(0.21)	(0.42)	(0.28)	(0.42)	(0.25)	(0.43)	(0.31)
Observations	546	539	524	528	511	504	492	495
R-squared	0.48	0.67	0.57	0.46	0.48	0.67	0.58	0.47
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.14 Effect on final mark controlling for twins

	Cohort 2015				Controlling for twins			
	Year 0	Year 1	Year 2	Year 3	Year 0	Year 1	Year 2	Year 3
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.06	-0.06*	-0.04	-0.01	-0.06	-0.06*	-0.04	-0.01
	(0.05)	(0.03)	(0.03)	(0.05)	(0.06)	(0.03)	(0.03)	(0.05)
Male	-0.17**	-0.16**	-0.16**	-0.15**	-0.17**	-0.17**	-0.17**	-0.15**
	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
Vulnerable	-0.06	-0.10**	-0.11**	-0.06	-0.04	-0.09**	-0.11**	-0.05
	(0.05)	(0.03)	(0.03)	(0.05)	(0.06)	(0.03)	(0.03)	(0.05)
Grade t_0	-0.12**	-0.01	-0.01	0.01	-0.12**	-0.01*	-0.01	0.01
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mark t_0		0.83**	0.65**	0.63**		0.81**	0.66**	0.64**
		(0.03)	(0.03)	(0.06)		(0.04)	(0.03)	(0.06)
Attendance t_0	0.03**	0.00	-0.00	0.00	0.03**	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Passed t_0	1.07**	-1.01**	-0.67**	-0.67**	1.15**	-0.96**	-0.68**	-0.66**
	(0.11)	(0.12)	(0.07)	(0.10)	(0.11)	(0.14)	(0.09)	(0.12)
School type t_0	0.12*	0.17**	0.18**	0.14*	0.12*	0.18**	0.19**	0.15*
	(0.05)	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)
Rural school t_0	0.08	-0.23**	0.00	-0.06	0.04	-0.23**	0.03	-0.03
	(0.09)	(0.07)	(0.04)	(0.08)	(0.10)	(0.08)	(0.05)	(0.09)
Twins					-0.15	-0.51**	-0.20	-0.12
					(0.11)	(0.09)	(0.15)	(0.15)
Constant	3.10**	1.66**	2.71**	2.43**	3.10**	1.83**	2.63**	2.38**
	(0.40)	(0.21)	(0.42)	(0.28)	(0.42)	(0.24)	(0.44)	(0.31)
Observations	546	539	524	528	511	504	492	495
R-squared	0.48	0.67	0.57	0.46	0.48	0.67	0.58	0.46
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.15 Effect on passing controlling for siblings

	Cohort 2015				Controlling for siblings			
VARIABLES	Year 0 (1)	Year 1 (2)	Year 2 (3)	Year 3 (4)	Year 0 (5)	Year 1 (6)	Year 2 (7)	Year 3 (8)
Treatment	0.02	-0.08**	-0.06**	-0.00	0.02	-0.07**	-0.06**	-0.00
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Male	-0.00	-0.05**	-0.05**	0.01	0.00	-0.06**	-0.05**	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Vulnerable	-0.03*	-0.04**	-0.03**	-0.00	-0.03*	-0.03**	-0.04**	-0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Grade t_0	0.01**	-0.01**	-0.02**	-0.01*	0.01**	-0.02**	-0.02**	-0.01*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Mark t_0	0.10**	0.18**	0.05**	0.05	0.11**	0.15**	0.04**	0.04
	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)
Attendance t_0	0.00*	0.00**	-0.00	0.00	0.00*	0.00**	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Passed t_0		-0.30**	-0.07**	-0.06		-0.24*	-0.06**	-0.04
		(0.11)	(0.03)	(0.08)		(0.10)	(0.02)	(0.07)
School type t_0	-0.03	0.07**	0.05**	-0.02	-0.03	0.06*	0.06**	-0.02
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
Rural school t_0	-0.01	-0.08**	-0.05	-0.05	-0.01	-0.11**	-0.06	-0.06
	(0.02)	(0.03)	(0.03)	(0.04)	(0.02)	(0.04)	(0.03)	(0.04)
Siblings					-0.00	-0.01	-0.01	0.02
					(0.01)	(0.02)	(0.01)	(0.03)
Constant	-0.04	-0.24	0.91**	0.75**	-0.03	-0.04	0.90**	0.82**
	(0.25)	(0.14)	(0.14)	(0.19)	(0.25)	(0.13)	(0.14)	(0.15)
Observations	546	546	543	543	511	511	508	508
R-squared	0.21	0.25	0.10	0.04	0.21	0.22	0.12	0.04
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

Table 10.16 Effect on passing controlling for twins

	Cohort 2015				Controlling for twins			
	Year 0	Year 1	Year 2	Year 3	Year 0	Year 1	Year 2	Year 3
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.02	-0.08**	-0.06**	-0.00	0.02	-0.07**	-0.06**	-0.00
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
Male	-0.00	-0.05**	-0.05**	0.01	0.00	-0.06**	-0.05**	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Vulnerable	-0.03*	-0.04**	-0.03**	-0.00	-0.03*	-0.03**	-0.04**	0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Grade t_0	0.01**	-0.01**	-0.02**	-0.01*	0.01**	-0.02**	-0.02**	-0.01*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Mark t_0	0.10**	0.18**	0.05**	0.05	0.11**	0.15**	0.04**	0.04
	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)
Attendance t_0	0.00*	0.00**	-0.00	0.00	0.00*	0.00**	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Passed t_0		-0.30**	-0.07**	-0.06		-0.23*	-0.06**	-0.04
		(0.11)	(0.03)	(0.08)		(0.10)	(0.02)	(0.07)
School type t_0	-0.03	0.07**	0.05**	-0.02	-0.03	0.07**	0.06**	-0.02
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Rural school t_0	-0.01	-0.08**	-0.05	-0.05	-0.01	-0.11**	-0.06	-0.06
	(0.02)	(0.03)	(0.03)	(0.04)	(0.02)	(0.04)	(0.03)	(0.04)
Twins					0.07	-0.42*	0.01	0.07
					(0.06)	(0.19)	(0.09)	(0.04)
Constant	-0.04	-0.24	0.91**	0.75**	-0.04	-0.03	0.90**	0.82**
	(0.25)	(0.14)	(0.14)	(0.19)	(0.26)	(0.13)	(0.13)	(0.15)
Observations	546	546	543	543	511	511	508	508
R-squared	0.21	0.25	0.10	0.04	0.21	0.23	0.12	0.04
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses. ** p<0.01, * p<0.05

10.5. Attrition

The following tables present the assessment of attrition due to data availability in the main analyses based on two approaches: considering the total number of students in the sample (*Table 10.17*) and considering the maximum number of observations with available data for each analysis (*Table 10.18*).

In both tables, columns 1-3 indicate the sample used in each of the main analyses per outcome and year, columns 4-6 show the criteria and observations against which the analysis sample is compared, and columns 7-10 present the attrition levels for enrolled and not enrolled applicants along with their difference and mean attrition for each regression model including control variables. The last column follows the WWC standards for attrition (WWC, 2017), based on its overall and differential levels. A green zone suggests that the threat of attrition bias is tolerable; a yellow zone indicates a high risk of bias if there is evidence to assume that data missingness is related to the treatment; finally, a red zone denotes unacceptable levels of attrition. In the case of this research, there is no evidence to suggest that the attrition levels are associated with the treatment; hence, I can conclude that the main analyses are not significantly threated by attrition bias due to data availability.

Table 10.17 Attrition analysis considering total sample

Outcomes	Analysis sample			Total sample			% Attrition				Bias zone
	T (1)	C (2)	Total (3)	T (4)	C (5)	Total (6)	T (7)	C (8)	Diff (9)	Mean (10)	(11)
attendance Y0	486	562	1048	830	1032	1862	41%	46%	4%	43%	yellow
attendance Y1	483	558	1041	830	1032	1862	42%	46%	4%	44%	yellow
attendance Y2	464	558	1022	830	1032	1862	44%	46%	2%	45%	green
attendance Y3	466	554	1020	830	1032	1862	44%	46%	2%	45%	green
final mark Y0	486	562	1048	830	1032	1862	41%	46%	4%	43%	yellow
final mark Y1	483	558	1041	830	1032	1862	42%	46%	4%	44%	yellow
final mark Y2	464	558	1022	830	1032	1862	44%	46%	2%	45%	green
final mark Y3	466	554	1020	830	1032	1862	44%	46%	2%	45%	green
passing Y0	486	562	1048	830	1032	1862	41%	46%	4%	43%	yellow
passing Y1	486	562	1048	830	1032	1862	41%	46%	4%	43%	yellow
passing Y2	483	562	1045	830	1032	1862	42%	46%	4%	44%	yellow
passing Y3	482	557	1039	830	1032	1862	42%	46%	4%	44%	yellow

Table 10.18 Attrition analysis considering max data availability

Outcomes	Analysis sample			Max # obs with outcome data			% Attrition				Bias zone
	T (1)	C (2)	Total (3)	T (4)	C (5)	Total (6)	T (7)	C (8)	Diff (9)	Mean (10)	(11)
attendance Y0	486	562	1048	486	562	1048	0%	0%	0%	0%	green
attendance Y1	483	558	1041	587	719	1306	18%	22%	5%	20%	green
attendance Y2	464	558	1022	658	835	1493	29%	33%	4%	31%	green
attendance Y3	466	554	1020	791	1017	1808	41%	46%	4%	43%	yellow
final mark Y0	486	562	1048	486	562	1048	0%	0%	0%	0%	green
final mark Y1	483	558	1041	587	719	1306	18%	22%	5%	20%	green
final mark Y2	464	558	1022	658	835	1493	29%	33%	4%	31%	green
final mark Y3	466	554	1020	791	1017	1808	41%	46%	4%	43%	yellow
passing Y0	486	562	1048	491	563	1054	1%	0%	1%	1%	green
passing Y1	486	562	1048	591	723	1314	18%	22%	5%	20%	green
passing Y2	483	562	1045	686	840	1526	30%	33%	4%	31%	green
passing Y3	482	557	1039	812	1022	1834	41%	45%	5%	43%	yellow