

# Are lotteries the best chance for the success of students and schools? A protocol for a systematic review and meta-analysis on school randomised admissions

Several school systems or specific school programs around the world involve the use of lotteries to assign students into schools. This admission mechanism is usually favoured to foster equality of opportunities in education. However, there has not been an effort to systematise existing evaluations of this type of interventions. This review protocol proposes to contribute to the literature on this topic with a systematic search and a meta-analysis, from an international perspective, of the effects that randomised school admissions have on student academic performance and school socioeconomic composition measures. The results and policy implications will serve as a new and relevant contribution for researchers and policy makers related to school choice, and for education authorities involved with school lotteries.

Keywords: school lottery, random assignment, student allocation, education policy, impact evaluation

## 1. Introduction

### 1.1. Background

The use of lotteries for different social decisions is not recent. It dates back to medieval times, even with religious meanings in some cases, and there are still examples of the use of lotteries in modern life: 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 (Duxbury, 1999; Stone, 2008).

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. 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 the 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 on the 60-70's (Berends, Springer, Ballou, & Walberg, 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 (Cfat, 1992). Moreover, lotteries have been incorporated into admission processes in schools in different cases, such as in the United States, the United Kingdom, New Zealand, the Netherlands, or Sweden (Stone, 2008; Sutton Trust, 2007).

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

Lotteries could be critical to help pursue this goal of equity in education under a school choice system (Stone, 2008; Belfield & Levin, 2005; Hill, 2005; Social Market Foundation, 2004; Walford, 1996), 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.

Randomly-based decisions are proposed to generate a fair outcome because all applicants are assumed equal, yet they 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 random-based decision, by its own, would not provide an equal treatment of the school places. This is why lotteries are frequently used in combination with other admission mechanisms, generally in the form of priority criteria of admission (Sutton Trust, 2007). Once these priorities are allocated, the remaining students can be assumed to be in equal conditions, and then a random allocation would provide a just decision-making mechanism.

Although we are aware of some individual school programmes or policy evaluations including school random admissions (Deming, Hastings, Kane, & Staiger, 2014; Allen, Burgess, & McKenna, 2013; Cullen, Jacob, & Levitt, 2006; Hoxby & Rockoff, 2005), these show mixed results in terms of student achievement and include a range of other academic (e.g. education attainment) and non-traditional (e.g. arrest rates) outcomes. Moreover, and to the best of our knowledge, there are no research efforts to rigorously and systematically consolidate the international literature available 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 11 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 this research is private scholarship programmes, that is, the offer of funding to attend a private school of choice. One of the 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 a review of the research indicates (Lubienski, 2016), the goal of having an international focus is not well achieved - it ultimately includes studies from three countries and the vast majority of them are from the U.S. - and the strategies to select the studies also end up clouding the trustworthiness and 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 seldom 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).

Given the lack of academic rigorous efforts to examine the evidence on school randomised admissions, this protocol proposes a systematic review 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 may become 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.

## 1.2. Review objectives

The review aims to, firstly, map and systematise the evidence available on the impact of randomised school admissions. Secondly, it intends to meta-analyse an overall effect of randomised school admissions on student academic performance and school socioeconomic composition measures.

The main research question guiding the systematic review is to understand 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. 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.

### 1.2.1. *Population*

The population of interest is 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). The second group of interest is composed by students at primary and secondary schools.

### 1.2.2. *Intervention*

The intervention consists of school admission processes based on a random assignment of students. This includes admission processes where all or some students are randomly selected, where other admission criteria can be combined with the school lottery, and where the random admission is part of a specific educational programme or a broader school policy.

### 1.2.3. *Comparison groups*

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

### 1.2.4. *Outcomes*

The primary outcomes of interest are focused on educational achievement and school socioeconomic composition measures. For the former, school- or student-level academic performance measures will be considered. As this is a commonly used outcome, we expect 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 are interested in school socioeconomic composition measures as indicators of segregation in education. These could take the form of a value in a socioeconomic index, a measure of variation within/between schools, or as another indicator of segregation in schools.

Secondary outcomes would be considered depending on the results of the screening process. Following the relevant literature already identified, examples of potential secondary outcomes may include – but are not limited to – school graduation rates, school absenteeism measures, or student socioemotional measures.

## 2. Methods

This review protocol is based on the guidelines of the Preferred Reporting Items for Systematic Reviews and Meta-Analyses Protocols (Moher et al., 2015; Shamseer et al., 2015). Accordingly, the proposed systematic review and meta-analysis will be conducted following the PRISMA reporting guidelines (Moher et al., 2009).

### 2.1. Eligibility of studies

The review will not restrict the search of references by publication status as we aim to be as comprehensive as possible in gathering relevant literature. Hence, we intend to target both academic and grey literature available. The eligibility of studies will be determined by the following hierarchical criteria:

#### 2.1.1. *Release timeframe*

Studies must have been published or made available from 1970 onwards. As previously discussed, the international development of school choice policies began to appear around this decade. Establishing this year limit would allow for initial evaluations to be considered in the mapping of available evidence while also keeping the number of references manageable in the context of this review.

#### 2.1.2. *Language*

Studies written in English or Spanish will be considered, following the resource constraints of the review. This eligibility criterion will be applied, when available, directly as a search limit in each information source.

#### 2.1.3. *Focus on randomised school admissions*

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

#### 2.1.4. *Empirical studies*

Studies must include empirical data analysis, either at the school or student level, and either from primary or secondary sources. The review will exclude theoretical discussions, educational policy reports, opinion pieces or similar that do not involve empirical data analyses.

#### 2.1.5. *Quantitative study design*

Studies must have a quantitative research component with a measured effect of the intervention (random allocation of students into schools). Design types to be considered include controlled experiments, before-after evaluations, matching techniques, and similar. Mixed-methods designs will not be excluded provided that the quantitative part abides by this criterion.

#### 2.1.6. *Outcomes within school years*

Studies must report at least one outcome for any stage within primary and/or secondary school education. We are aware that these education levels may differ by school system and, depending on the evidence available, we may need to account for these potential differences to have comparable outcomes. The review will not focus on preschool or higher education outcomes, however, references will not be excluded if the school(s) studied provide preschool education and the random assignment of students occurs at this level.

### 2.2. Information sources

Table 1 shows the list of databases and search engines that will be considered for the literature search phase, which includes academic catalogues, open sources, institutional databases and

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

**Table 1. List of information sources to include in literature search**

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

### 2.3. Search strategy

Free-text fields (title and abstract) searches will include the same structure to ensure a consistent and comprehensive process. The strategy will be adapted for each database/electronic source according to its searching functions and saving/retrieving system. A log of each search will be kept for future reference, including browser and keywords used, number of results returned, and date of search. The literature search will be conducted by Reviewer #1 (R1).

Table 2 exemplifies the search strategy for the development database. In its first level, the free term *random* is likely to gather references out of the scope of schools and education. By conditioning its use with the term *admission*, the search is likely to focus more on education studies. The use of the search function “NEAR” or “ADJ” (adjacent) will be used, when available and adapted accordingly, on the research databases and search engines. In its second level, the free terms *school* and *student* would be expected to capture relevant literature related to the population of interest of the review. In the third level of the strategy, alternative terms related to quantitative research will be used to detect empirical studies.

Additionally, each adapted strategy will include, when available, year and language limits according to the eligibility criteria.

**Table 2. Example of search strategy for ERIC (ProQuest) database**

Search focus	Search terms
Research topic #1	“random” NEAR3 “admission” OR “lottery” AND
Research topic #2	“school” OR “student” AND
Research design	“evaluation” OR “effect” OR “impact” OR “gain”

### 2.4. Study records

The review process will be managed with the online software EPPI-Reviewer 4, which provides tools for a collaborative work in handling references and analysing data. Table 3 indicates a proposed timeframe for the review and the distribution of tasks between the researchers involved in the study.

**Table 3. Summary of review process and author involvement**

Phases of systematic review	Months	R1	R2	R3
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Definition of research question Protocol for systematic review and meta-analysis	1	X		X
Testing search strategy Literature searching	2-3	X		X
Piloting inclusion criteria Screening by title and abstract	4-5	X	X	X
Screening full papers	6-8	X	X	X
Coding and data extraction, Critical appraisal, and Calculation of effect sizes Data analysis	9-10	X	X	X
Write up	11-12	X	X	X

R1 will conduct the references search, gather the results in one dataset and remove duplicates. To make sure the inclusion criteria is clear and adequate, R1 and R2 will carry out a pilot test before the screening by title and abstract. This exercise will consist of double-screening a random set of 100 references, aiming to achieve at least a 95% of agreement. If the disagreement is above the threshold, the inclusion criteria will be revised and modified accordingly, and the same exercise will be repeated with another random set of references.

Once the inclusion criteria have been tested and agreed, R1 will screen by title and abstract all search results and R2 will double-screen a random sample of 30%. When applicable, both researchers will identify and code the reason for excluding each reference. The level of agreement will be once again assessed according to the previous standards, and disagreements will be discussed and amended as necessary. R1 will then retrieve full papers of all included references. Records will be kept indicating the number of retrieval attempts and reasons for not retrieving a report.

Subsequently, and using the same criteria, R1 will screen by full report all included references and R2 will double-screen a random sample of 30%. In this stage, they will again identify and code the reason for each excluded reference. The level of agreement between the researchers will be assessed once more according to the previous standards, and disagreements will be discussed and amended as necessary. All decisions made will be documented and compiled by R1 for future reference.

## 2.5. Data extraction and synthesis

A three-step process will be conducted on each reference included after the screening by full report involving data extraction, critical appraisal, and calculation of effect sizes. These will be performed in parallel by R1 and R2.

To produce a systematic map, a data extraction matrix will be created with the relevant information to identify from each study. The extent of this information will depend on the screening results, but we anticipate some key fields such as - but not limited to - country, school age, participants, research design, outcome measures, and main findings. In case that more than one reference covers the same program (either as a follow-up or as another report version), the researchers will establish a general rule, contingent on the screening results, for defining a master study and all remaining versions will serve as complementary information.

The in-depth analysis stage of the review will synthesise the evidence on the effect of school randomised admissions through statistical meta-analysis, provided there is sufficient data, and through a narrative synthesis including a presentation of the range of effect sizes where calculable. All included references after the screening by full report phase will potentially be part of the in-depth analysis; however, depending on the screening results and relevant features of these studies, the inclusion criteria for the meta-analysis may be refined. A critical appraisal of the primary studies will also be conducted according to - but not limited to - the following risk of bias criteria: selection, compliance, contamination, attrition, sample reporting, and outcome reporting bias. Differences in the risk of bias will be accounted for with sensitivity and/or sub-group analyses.

Lastly, the researchers will calculate the effect size and its precision estimate for each included study in order to increase consistency and comparability between the outcomes measured across studies (Lipsey & Wilson, 2001). 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. Alternatively, we will attempt to impute such missing data under sensible assumptions.

In this process, the work of R2 and R3 are primarily aimed to be a check or comparison point for the work of the main researcher, R1. Having an additional person checking the work can help to identify issues that could affect the meta-analysis results. By contrasting results, differences in the criteria in any of these stages can be easily identified and adjusted accordingly. Additionally, all decisions made will be documented and compiled by R1 for future reference.

#### 2.5.1. *Meta-analysis strategy*

Provided there are sufficient studies to perform the meta-analytic synthesis, we will calculate the overall effect of school randomised admissions on student academic achievement and school socioeconomic composition outcomes. We anticipate these outcomes to be measured as continuous variables, hence we would calculate standardised mean differences (with 95% CI) between treatment and control groups, being these areas, schools, or students according to the comparison groups discussed in section 1.2.3. If there are enough studies and data, we will also calculate the effect of school randomised admissions on other outcomes measured in the subset of included studies.

Since the review intends to collect evidence on school randomised admissions from a global perspective, we assume there will be random differences between the studies included in this stage. To account for this between-study heterogeneity, the meta-analysis will use a random effects model (Lipsey & Wilson, 2001).

#### 2.5.2. *Heterogeneity, sensitivity analysis and subgroup analysis*

The  $I^2$  statistic will be calculated to measure inconsistency across the subset of studies included in the in-depth analysis (Higgins, Thompson, Deeks, & Altman, 2003). Moreover, the results of the critical appraisal will inform the pertinent sensitivity analyses to evaluate the variation of findings across studies associated with their research designs and risk of bias characteristics (Gough, Oliver, & Thomas, 2012). Depending on these results, subgroup analyses will be conducted based on, but not limited to: country, school age, type of school administration, and type of educational program.

#### 2.5.3. *Meta-biases*

Although we have 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 will assess 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 (i.e., 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). Additionally, a range of other potential biases (selection, compliance, contamination, attrition, sample reporting, and outcome reporting) will be integrated into the critical appraisal assessment for all references included in the in-depth analysis. Hence, these potential biases will be assessed in the sensitivity and subgroup analyses.

#### 2.5.4. *Assessing the review's quality and relevance*

The quality and relevance assessment will follow the Weight of Evidence framework (Gough, 2007), considering the design, focus, relevance, and execution of the review to appraise the overall strength of the evidence regarding the review question.

## 2.6. Final reports

This project will produce two key outputs. 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. This extended version will be included as a chapter of R1's PhD dissertation. The second report will have a journal paper format, addressing mainly the primary research question and, therefore, focusing on the meta-analysis process. This publishable version will be co-authored with R2 and R3.

## Contributions

R1 is the guarantor. R1 drafted the manuscript and mainly developed all sections of the protocol. R2 contributed significantly to the conception of the review and eligibility criteria. R3 contributed substantially to the overall development of the review proposal. All authors read, provided feedback and approved the final manuscript.

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