Systematic review

A systematic review of the evidence of the impact of school voucher programmes in developing countries

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Contents

List of abbreviations

3ie	International Initiative for Impact Evaluation
CCT	conditional cash transfer
DFID	Department for International Development (UK)
ERIC	Education Resources Information Center (USA)
ES	effect size
FFE	Food For Education (Bangladesh)
HUD USER	Housing and Urban Development (US)
IDEAS	Internet Documents in Economics Access Service, Department of Economics, University of Connecticut (US)
IV	instrumental variables
JOLIS	Joint Bank–Fund Library Network
NCCAN	National Clearinghouse of Child Abuse and Neglect (US)
NGO	non-governmental organisation
OLS	ordinary least squares (regression)
PAIS	Public Affairs Information Services
PISA	Program for International Student Assessment
PSM	propensity score matching
PPPs	public-private partnerships
QED	quasi-experimental design
RCT	randomised controlled trial
RDD	regression discontinuity design
SES	socio-economic status
UNESCO	United Nations Educational, Scientific and Cultural Organization
USAID	United States Agency for International Development

Abstract

Background

Demand-side financing is based on the principle of governments either channelling education resources through students and their parents, or basing school funding on enrolments or attendance. Regardless of the direct path of funding, demand-side financing is viewed as a way of addressing inequities that prevent poor children from continuing their education, as well as a means of introducing school choice. Education vouchers, a demand-side financing intervention involving the public subsidy of private schooling based on the number of eligible voucher students per school, generally aim to expand parental school choice, which is often promoted to increase competition in the school system. Opponents of vouchers argue that private schools do not necessarily provide a higher-quality education; affluent families with more social capital and access to voucher programme information are more likely to find the best schools; and it is very difficult to set up effective systems of accountability to guard against 'cream skimming' and sorting. In this paper, we report on a systematic review of evaluations of education voucher programmes in developing countries.

Methods

Through extensive searching, including electronic keyword searches of bibliographic databases, handsearches of relevant journals, examinations of online holdings of international development organisations and research firms, citation chasing, examining grey literature, and contacting experts in the field, we identified studies that responded to the following question:

> *What is the evidence of the impact of school vouchers in developing countries?*

Eligible studies had to meet the following criteria: The evaluation took place in a low-income developing nation as defined by the World Bank at the time of the intervention; and the evaluation directly assessed the impact of a school voucher programme on participants' educational outcomes. With the intent to conduct meta-analysis, we focused on identifying randomised controlled trials (RCTs) or quasi-experimental evaluations (QEDs) with some evidence that the groups being compared were equivalent. In addition, we sought for contextual information – but not for inclusion in effect size estimates – i.e., quasi-experiments without pre-test group equivalency, and other quantitative and qualitative studies that shed light on implementation and context issues.

Each RCT or QED located in the search that appeared to be a possibility for inclusion was carefully reviewed by two authors and a structured abstract was prepared for each study, detailing the context, methodology and findings. For each study deemed eligible for inclusion following this screening process, a coding instrument was completed that included items in the following areas: researcher and study characteristics, study methods and methodological quality, intervention and control conditions data, participants in the study and outcome data. To evaluate study quality, we recorded details on three key implementation issues: how the groups were equated and whether any problems with equating were reported, information on attrition, and whether the programme experienced significant implementation or fidelity problems.

Results

We identified two studies that met our inclusion criteria – one examining the Colombia PACES programme and the other evaluating the Quetta, Pakistan Urban Fellowship programme. We also identified four quantitative studies on the Chile voucher system that did not meet our criteria for inclusion in effect size estimates but were examined to shed light on possible theory, implementation and context issues. Given the very small number of studies that met our inclusion criteria, we provide the results in a narrative fashion, rather than through meta-analysis. Both the Colombia and Pakistan programmes increased private school enrolment amongst the countries' poorest income groups, thus probably improving equity. The Pakistan programme resulted in girls being educated for less than it would have cost for the government to create public school spaces, while the Colombia programme cost rather more, but will most likely prove cost-effective in terms of long-term economic gains.

Conclusions

Clearly, more rigorous research in developing country contexts is necessary to determine whether the gains from these two programmes can be replicated and enhanced and to elucidate the many issues surrounding vouchers. Pilot programmes employing random assignment or lotteries should be accompanied by rigorous impact evaluation. This approach would enable governments to design innovative initiatives and target resources most efficiently and equitably.

1. Introduction

1.1 Background

Two billion children, 85 percent of the world's total child population, live in developing countries (Kremer and Holla, 2008), where public education systems face enormous challenges (Gauri and Vawda, 2003), such as lack of infrastructure and low teaching standards (Thapa and Mahendra, 2010). The limited and unbalanced availability of affordable schooling in the developing world can lead to overcrowding and poor educational quality for children who arguably need the most help (Angrist *et al.*, 2002). Low educational attainment in developing countries is attributed in part to the private costs associated with sending children to public and private schools (Filmer and Pritchett, 1998, 2001; Bentaouet Kattan and Burnett, 2004; Yardley, 2005). Educational costs may also exclude low-income households from private schools or better-quality public schools. Children of the poorest income quintile in developing countries consistently represent the lowest percentage of eligible students attending school (Thapa and Mahendra, 2010).

Demand-side financing, based on the principle of governments either channelling education resources through students and their parents or basing school funding on enrolments or attendance, is a way of addressing inequities that prevent poor children from continuing their education, as well as a means of introducing school choice (Patrinos, 2007). Education vouchers[1] are a demand-side intervention involving the public subsidy of private schooling based on the number of eligible voucher students per school (Arenas, 2004). The term private school generally refers to for-profit or non-profit privately operated non-government-subsidised schools that charge tuition to all students. These may include NGO and faith-based schools, as well as low-cost private schools for the poor – and these represent a relatively new and rapidly-expanding education sector in many developing countries.

School vouchers generally aim to expand parental school choice (Gauri and Vawda, 2003; Patrinos, 2007; Oosterbeek and Patrinos, 2008; Thapa and Mahendra, 2010), which is often promoted to increase competition in the school system (Friedman, 1955). It is argued that the presence of more private schools leads to both public and private schools improving quality and learning outcomes, and thereby increasing efficiency to attract students and their accompanying resources (Patrinos, 2005). However, when the main determinant of school choice is poverty, marketisation of the education sector alone will not improve equity (see e.g. Härmä, 2010; Woodhead, *et al*. 2012). That is, the most disadvantaged students will remain marginalised in the lowest-quality government schools.

There are different voucher models for compulsory education (e.g., Friedman 1955; Jencks, 1971; Levin 1983) and the form vouchers take in each country may be radically different (Arenas, 2004). Generally, however, voucher programmes involve payment made by the government to a parent or to a school chosen by the

[1] Definition of 'voucher' for the purposes of this review: an education voucher system 'in the broadest sense is a payment made by the government to a school [or directly to the parent] chosen by the parent of the child being educated; the voucher finances all or most of the tuition being charged. The system introduces competition among public schools and between public and private schools; and it enables schools to offer diverse educational packages to meet the different preferences of parents' (West, 1997, p. 83).

parent; the voucher finances all or most of the tuition being charged (West, 1997).[2] A school voucher system introduces three simultaneous reforms: (1) allowing parents to choose schools; (2) creating incentives for schools to increase enrolment; and (3) granting schools management autonomy to respond to demand (Gauri and Vawda, 2003). In a developing country context, in which public school students are often assessed tuition and other fees, school vouchers may be used for participating public, as well as private, schools.

There are various arguments in favour of voucher programmes in developing countries. For example, vouchers may improve quality in both public and private schools, which now have to compete to attract students (Arenas, 2004; Kremer and Holla, 2008). Vouchers may increase equity because they provide poor families with access to private education (Chubb and Moe, 1990) and all parents, regardless of income, are now able to choose their child's school (Patrinos, 2005). Theoretically, systems of accountability can be instituted to limit segregation by socio-economic status, ethnicity and academic ability (Arenas, 2004) that may arise as the result of voucher implementation. Advocates for voucher programmes also stress that in low-income countries, private enrolment is generally more desirable than public. In fact, 'private enrolment as a proportion of total enrolment is 2-3 times higher than in industrialized nations', because of the typically low-quality education available in most public schools (Angrist et al., 2002, p.1535). High teacher and administrator absenteeism exacerbate low quality, which lead to many families removing their children from public schools (Gauri and Vawda, 2003).

Opponents of vouchers argue that private schools do not necessarily provide a higher-quality education and that many are more interested in maximising profit than in providing a responsible civic education; affluent families with more social capital and access to voucher programme information are more likely to find the best schools; and it is very difficult to set up effective systems of accountability (Arenas, 2004). Increased school choice may thus lead to 'cream skimming' – in which private schools select the most academically able and advantaged students (particularly because parents often conflate student body composition with educational quality) – and sorting, in which the most disadvantaged students are isolated in the lowest-quality schools (Lara *et al.*, 2010). In addition, peer effects (i.e. the influence of high-achieving peers) may explain improved academic outcomes for students in private schools under a voucher system (Arenas, 2004; Kremer and Holla, 2008). Moreover, vouchers may not cover all schooling costs, restricting take-up to only those households who can afford to 'top up' the voucher at a more desirable school, and quality schools, whether public or private, are not available in all areas. Thus, a voucher programme may not improve educational outcomes for all children and, in some cases, may even increase inequality.

Developing countries that have adopted voucher programmes have reported changes in school enrolment. For example, in Chile (described in more detail below), private voucher school enrolment increased from 15 percent to

[2] This review focuses on general education voucher programmes, as defined above – government-sponsored programmes that provide vouchers or similar subsidies for private education. These include voucher-like programmes, such as the Quetta Girls Fellowship Programme, that may not be 'pure' voucher programmes, but that have the same aims of subsidising private education to introduce school choice. There are incentive-based models that do not necessarily offer public/private school choice, which is typically the emphasis of voucher programmes, and are beyond the scope of this review. For example, many developing (and industrialised) nations have adopted various incentive-based programmes to promote primary and secondary education among marginalised populations (e.g., conditional cash transfer [CCT], food for education, stipends). See Petrosino, *et al.* (2012).

approximately 33 percent of total enrolment, between 1981 – the voucher scheme inception – and 1996 (McEwan and Carnoy, 2000). Bangladesh also saw female enrolment increase by nearly 65 percent during its programme pilot study (King et al., 1997). An evaluation of Colombia's programme (also described in more detail below) showed a 15 percent increase in private school enrolment among voucher awardees and statistically significant improvement among females for educational attainment (Angrist et al., 2002).

1.2 School voucher programmes implemented in developing countries

School voucher programmes have been implemented in some developing countries, including Bangladesh, Cote d'Ivoire and some Latin America countries. The most extensive voucher programmes have operated in Chile and Colombia. In Chile, arguably the best-known voucher system in the developing world has been in place since the early 1980s. The voucher programme was instituted as part of an educational reform process, which began in 1980 and included the transfer of school management from the central government to local municipalities, and the equal financing of public (municipal) and non-fee charging private schools based on student enrolment. A nationwide programme, all school-age children can receive vouchers to attend any school of choice (municipal, private subsidised or private non-subsidised). Municipal and subsidised private schools may charge an additional small tuition fee, but both are mainly financed through the voucher system. Private non-subsidised schools are financed solely through payment of private tuition. Families can choose any school, but private schools may have a more selective admissions process, whereas municipal schools can only be selective if demand exceeds supply (Behrman *et al.*, 2010).

Most Chilean schooling is now voucher-financed (Hsieh and Urquiola, 2003), as the programme covers more than 90 percent of the school-age population and is the most extensive programme of its kind in Latin America (Arenas, 2004). Many new private schools have opened as a result of the programme (Lara *et al.*, 2010), and there has been a massive transfer of students from public to private voucher schools (Gauri and Vawda, 2003; Arenas, 2004; Lara *et al.*, 2010). Private school enrolment increased from 15 percent of students in the early 1980s to approximately 50 percent in 2010 (Thapa and Mahendra, 2010). However, the private school advantage is mostly limited to urban areas (Thapa and Mahendra, 2010), where such schools are concentrated. In rural areas, 81 percent of schools are public (Tokman-Ramos, 2002) and in some areas of Chile, private school enrolment is nearly zero (McEwan *et al.*, 2008).

The Chilean model is an example of an unrestricted voucher model. The Colombian model, on the other hand, is considered a targeted voucher scheme. In Colombia, the government instituted the 'PACES' voucher programme in the 1990s to increase secondary school enrolment rates by expanding private provision (Bettinger *et al.*, 2010). The vouchers were co-financed by the central government and participating municipalities and assigned by lottery when demand exceeded available places. Participating schools tended to serve lower-income pupils and have lower tuition fees than non-participating private schools (Bettinger *et al.*, 2008) and most elite schools elected not to accept vouchers. The programme operated in all large cities, in areas where public schools were filled to capacity, and targeted students from low-income families. Parents could use the vouchers at any participating school, and the vouchers were renewable through to the end of secondary school, dependent upon satisfactory academic performance and promotion to the next grade. From 1991-1997, 125,000 vouchers (1 percent of national secondary school enrolment – Gauri and Vawda, 2003) were awarded that partly covered tuition costs. PACES led to enrolment increases (Angrist *et al.*, 2004). There was no

massive migration from public to private schools as occurred in Chile, mainly because parents viewed the public schools positively (Arenas, 2004).

1.3 Prior research on school vouchers in developing countries

So far, little systematic research has been conducted on the effects of voucher programmes in developing countries (Gauri and Vawda, 2003; Patrinos, 2005; Thapa and Mahendra, 2010). Shafiq (2010) further suggests that although there has been rapid growth in low-cost non-public schools in developing countries, there is little scientific evidence of the impact of educational vouchers specifically targeted at the poorest populations.

There is a relatively large amount of literature on voucher programmes in general, but the evaluative research is much less extensive. In the case of Chile, moreover, the universality of the voucher programme means that treatment and control groups are extremely hard to build (Lara *et al.*, 2010). Lacking randomised designs and panel data, most of the available empirical research for Chile focuses on the relative effectiveness of public and private schools under the voucher policy (Lara *et al.*, 2010). These studies have generally found that students attending private voucher schools have higher educational outcomes than those attending public schools (Henriquez *et al.*, 2010). However, as mentioned previously, because households choose schools, rather than being randomly assigned, it is difficult to account for selection bias. Also, private schools may choose which students to enrol. At any rate, because vouchers are available to all students, studies of the relative effectiveness of different types of schools in Chile do not measure the impact of the voucher policy per se.

Non-experimental research on Chile suggests that the voucher programme increased social segregation. Enrolment at private schools was shown to be higher amongst affluent households (McEwan *et al.*, 2008) and it was suggested that the best students moved to private schools (Carnoy and McEwan, 1998). This segregation has been described as being 'large-scale' – the result of private schools responding to competition by selecting the best and most advantaged students rather than improving quality (Hsieh and Urquiola, 2006; Contreras *et al.*, 2010) – selection which has taken place extensively (Epple and Romano, 1998; Narodowski and Nores, 2002; Bellei, 2005; Contreras *et al.*, 2009). Overall, the available evidence on school choice in Chile is not sufficient to evaluate the true impacts of the voucher programme (Bellei, 2005).

To our knowledge, the impact of school vouchers in developing countries has not been systematically reviewed. A systematic review of rigorous impact evaluations is essential for understanding the nature and quality of evidence in an area receiving attention by development agencies, non-governmental organisations (NGOs) and academic institutions.

1.4 Objectives

In this paper, we report on a systematic review of evaluations of education voucher programmes in developing countries. We employed extensive searching, including handsearches, examining grey literature, and contacting experts in the field. Through examination and coding of the eligible studies, we have assessed the breadth and quality of the literature base and synthesised the available evidence of the impact of education voucher programmes on outcomes such as primary and secondary school enrolment and educational quality and equity.

1.5 Conceptual framework

The review draws its conceptual framework from the growing body of literature on demand-side education financing programmes – particularly on incentive-based

models (e.g., King *et al*., 1997). In its most general sense, a voucher programme involves government providing resources to households (or schools) to offset enrolment costs at eligible primary and/or secondary schools. Vouchers, in essence, aim to manage current poverty by lessening the financial burden of primary and/or secondary school attendance for households, and ease future poverty, by increasing human capital through higher educational attainment of children from poor households, leading to increased future earnings (West, 1997).

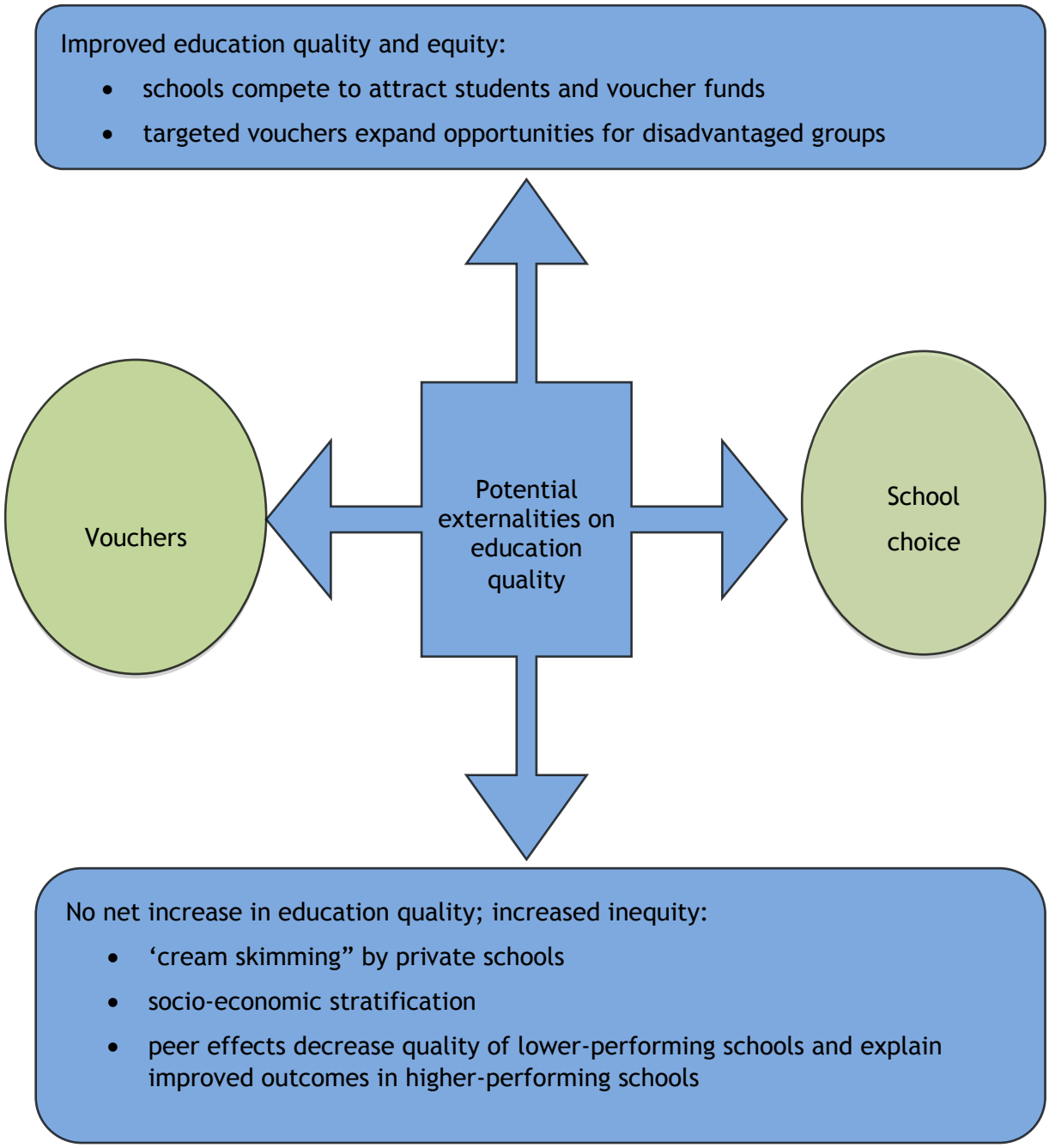
The educational voucher scheme was initially conceived by the economist Milton Friedman (1955), as a way to foster competition amongst schools, which in turn should increase overall educational quality and eliminate those schools that cannot keep pace. This competition, along with the voucher, should increase school choice for participating children (Levin, 2002). West (1997) explains the conceptual framework of voucher models as being comprised of four principles: (1) consumer choice, (2) personal advancement or opportunity to choose, (3) the promotion of competition, and (4) equal opportunity. He further explains that the last principle, equal opportunity, is the logical outcome of the first three principles. It is often represented in voucher models as the end result of increasing access to private schools and theoretically higher-quality education. Equal opportunity is exemplified in vouchers that target poor households. Some argue that unless the programme is specifically targeted to poor households, competition will not ensure equal opportunity.

Levin's (2002) comprehensive school voucher framework also focuses on four criteria of voucher programmes: 1) freedom to choose, 2) productive efficiency, 3) equity, and 4) social cohesion. He notes that all voucher programmes are not the same, nor are their overarching objectives. Voucher programmes may place particular emphasis on one criterion over another by using three 'policy instruments': finance, regulation and support services; these are what define and distinguish individual voucher programmes. Although some voucher schemes, including Friedman's (1955) original proposal, focus mainly on freedom of choice and productive efficiency, advocates of targeted vouchers propose that placing emphasis on equity, social cohesion and freedom of choice will provide the greatest good for those who need it most (Levin, 2002).

As shown in Figure 1.5, vouchers introduce school choice, which may impact education on quality and equity. These effects should be examined by evaluations of education voucher programmes, and may include, as described above:

- improved educational offerings as schools compete for students
- improved equity as lower-income families access better-quality schools
- and/or sorting and decreased educational quality as higher-ability students desert to private schools.

Figure 1.5: Conceptual framework



2. Methods

2.1 Identifying and describing studies

For assessing the impact of school voucher programmes, we only included studies with the following characteristics:

1. The evaluation was conducted in a country classified as a low-, lower-middle- or upper-middle-income country as defined by the World Bank at the time the programme was implemented.
2. The evaluation includes outcomes that measure the impact of school vouchers on schooling outcomes, such as enrolment, student achievement and attainment, and school quality.
3. The evaluation study report was published or became available between January 1991 and November 2011, without regard to language or publication type.
4. The study was a randomised controlled trial (RCT) or quasi-experimental evaluation with some evidence that the groups being compared were equivalent. Because randomisation is not possible for certain polices or programmes (e.g., in retrospective evaluations or when the programme has already been implemented), our criteria included evaluative studies that used quasi-experimental designs that included controls for baseline or pre-test measures of a primary outcome. We prioritised this based on prior research examining the alignment of estimates from quasi-experiments to randomised experiments, finding that controls for baseline in the matching or other process were most important to closer approximations . Quasi-experiments that were initially eligible usually included: (1) regression discontinuity designs that tested programmes that used an actual score to assign programme eligibility and that examined programme impact around the cut-off score (and in which participants were likely to be very similar); (2) quasi-experiments that used matching methods such as propensity scores that explicitly included baseline measures of enrolment to 'force' equity between groups on the baseline measure; or (3) designs that included a variety of methods such as a combination of propensity score or covariate matching with difference-in-difference techniques. Thus, quasi-experiments that relied solely on cross-sectional designs without baseline controls on at least one of the primary outcomes of interest were excluded.
5. Although only RCTs and rigorous QEDs could be included in effect size estimates, we also searched for the following designs to further illuminate school voucher policy implementation issues:
    - quasi-experimental studies without pre-test group equivalency
    - non-causal quantitative and qualitative studies. To be included in the review, non-causal studies had to be identified as evaluative (e.g. impact study, assessment, evaluation) and to provide sufficient methodological detail to be in some degree replicable – that is, to describe the data analysed and clearly outline the specific methods employed to evaluate the vouchers programme.

2.2 Search strategy

Our goal was to identify both published and grey literature. Many of the databases searched include grey literature (e.g. ERIC). The British Library indexes conference

proceedings and makes these available in its Integrated Catalogue. Internet searches will often identify literature that is made available on websites but not published in journals. Our contact with colleagues was also designed to obtain more of the grey literature. To accomplish our goal, we used five major strategies:

1. *Electronic searches of bibliographic databases*. We searched online databases available through WestEd and the University of Pennsylvania, including British Education Index, PAIS International/Archive and Sociological Abstracts, as well as conducting broader searches of the internet (see Appendix 2.1 for a full list of databases searched). It is important to note that we conducted new searches for the current project, but also had the benefit of relying on completed searches for experiments and quasi-experiments for a larger project on school enrolment funded by 3ie (Petrosino *et al.*, 2012).
2. *Handsearches of relevant journals*. Because electronic searches often miss relevant studies, we handsearched the tables of contents, and the abstracts when necessary, of all issues of five journals that publish studies relevant to this topic: *Economic Development and Cultural Change*, *International Journal of Educational Development*, *Journal of Development Economics*, *World Bank Research Observer*, and the *World Bank Economic Review*. These five journals were identified as publishing the most experimental and quasi-experimental research relevant to developing nations and education, from our larger review on school enrolment funded by 3ie.
3. *Specific examinations of online holdings of international development organisations and research firms.* This included international agencies that either conduct or would be aware of possibly relevant evaluations in developing nations, including the UK DFID and the United States Agency for International Development (USAID) (see Appendix 2.1 for a list of organisation websites searched).
4. *Citation chasing*. We checked the reference section of every retrieved report to determine whether any possible eligible evaluations were listed.
5. *Contacting the 'informal college' of researchers in this area.* We contacted several researchers who were conducting or were aware of experimental and quasi-experimental studies relevant to education issues in developing nations so that they could alert us to any missed studies (see Appendix 2.2 for a list of persons contacted). These persons were lead or contributing authors to experimental or quasi-experimental research in the developing nation and education area, identified from our overarching review of school enrolment funded by 3ie. (We choose not to reveal who responded as this might be embarrassing to our colleagues who may have been busy, on vacation, travelling, or otherwise occupied with other matters and unable to respond.)

Using specific search terms to identify school voucher studies reported on between 1991 and 2011 in developing countries, we searched 37 databases, websites and journals (see Appendices 2.1 and 2.3 for search terms and sources searched). Keywords were selected based on a review of relevant voucher studies identified during the 3ie project (e.g. looking at the titles and abstracts to see what terms were used to identify vouchers) and an examination of overarching review and overview papers in the area of education vouchers in developing nations. We reviewed all citations to determine if the cited study should proceed to a second screening, i.e. was a potentially relevant study.

2.3 Coding

For each included study, we completed a coding instrument (Appendix 2.4) comprised of items in the following areas:

1. *Researcher and study characteristics:* We extracted data about the type of publication the study was reported in and the setting and context in which the trial was conducted.
2. *Study methods:* We extracted information about the randomisation, quasi-experimental assignment and other methodological aspects of the evaluation. The level of assignment and whether the study included multiple analyses at different levels was also coded. In addition, we recorded details on the three key implementation issues:
    - *How the groups were equated and whether any problems with equating were reported*. The integrity of a randomised experiment or a quasi-experiment largely rests on how faithfully the equating procedures were implemented. We recorded information about randomisation or the quasi-experimental matching or equating procedures that were used in the study. In randomised experiments, this included how much of the originally randomly assigned sample actually received the treatment (slippage from the 'intention to treat' sample).
    - Whether the researchers reported a loss of participants from the initial assigned sample at the end of the study, how much attrition was reported, and whether the attrition differentially affected one group or the other. Such attrition, if it is significant, can compromise the equating of groups, particularly if different types of people drop out from the intervention group than from other groups. We recorded specific information on the amount of attrition (if it occurred) and whether it was differential in nature.
    - *Whether the programme experienced significant implementation and fidelity problems.* The first two issues deal with the implementation of the evaluation. This issue deals with the implementation of the programme; there may be no observable programme impact because no 'real programme' was ever implemented. We recorded in descriptive and qualitative form any implementation problems noted by the investigators.
3. *Methodological quality:* For reports of RCTs and quasi-experimental studies, we examined and recorded the information described above to make a determination about methodological quality. For each study, we captured information about any issues with crossovers (persons receiving a condition they were not assigned to), selection bias (e.g. breakdowns in randomisation or unusual unequal distributions in groups), loss of participants due to attrition or database matching issues, and intervention fidelity and implementation issues. We also rated each study according to our perception of whether the problems presented a threat to the findings reported in the study. These ratings were categorised as 'low', 'moderate', or 'high'. If there were no indicated problems, the threat to the study was rated as 'none'.
4. *Intervention and control conditions data:* These items solicited detailed descriptions of the intervention and control condition, including the 'dosage' of the treatment being implemented, and the number of participants assigned to each group. In cases in which more than one treatment and control group was present, we selected the groups that experienced the greatest contrast between conditions, i.e., the most intensive intervention condition versus the

least intensive control condition. We documented these decisions for full transparency.

5. *Participants in the study:* These items solicited detail about the type of participants in the trials, including information on the country in which the study took place, the age, school level and gender targeted, whether an urban or rural setting was involved, and the socio-economic status (SES) of the students.
6. *Outcome data:* We extracted information on reported outcomes, including impacts on access to education, persistence, learning and equity. We also coded economic data, where present, and any other outputs or data on key 'mechanisms' that provided clues as to why the intervention did or did not have its intended impact. We also coded the time intervals of the various outcomes and subgroup effects.

2.4 Reviewing other quantitative and qualitative studies

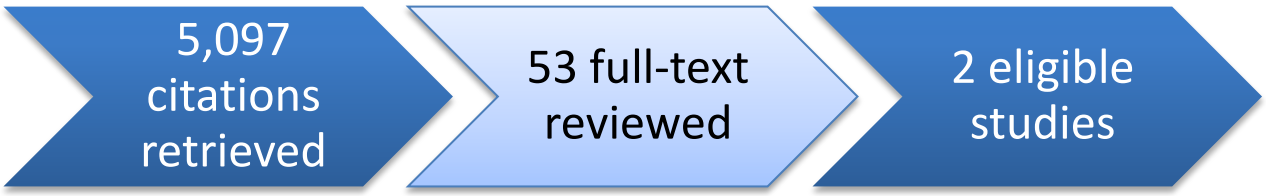
For other quantitative and qualitative studies to be reviewed for contextual information, they had to evaluate a school voucher programme and to include sufficient methodological detail as to be replicable. For the four QEDs without evidence of equating identified in the searches, we completed structured cases (see Appendix 2.5), rather than the full coding document used for impact evaluations. These cases describe in narrative form important information from the descriptive studies including, background and rationale, setting and participants, intervention type and methodology, data collected and analyses conducted, key outcomes and practice/policy implications. Structured cases were also prepared for the two included impact studies.

2.5 Results

As shown in Figure 2.5, the 37 databases, websites and handsearched journals yielded 5,097 potentially relevant citations and abstracts (including duplicates). Most of these were eliminated after careful screening of the abstracts or full text. Reasons for elimination included not being evaluative studies of school voucher programmes and not being conducted in a lower-income country. During a second screening process, we carefully examined the full text of 53 quantitative studies (see Appendix 2.6), 51 of which were ultimately excluded because they did not provide baseline equating of groups and/or did not directly evaluate the impact of a voucher programme. Four quasi-experimental designs underwent a final screening process but did not meet the inclusion criteria. Although we specifically searched for qualitative evidence as well, such as case studies, our searches did not yield any primary non-quantitative studies that directly evaluated a school voucher programme in a developing country.[3] We identified two experiments that met the inclusion criteria. Although we intended to quantitatively synthesise the results from the impact evaluations in a meta-analysis, given the very small number of studies that met our inclusion criteria, we elected to provide the results in a narrative fashion. Given the dearth of rigorous primary studies, caution should be taken about generalising results from this systematic review.

[3] One descriptive report identified in the search (Salman, 2009) does include qualitative feedback from a survey of voucher school administrators in Lahore, Pakistan. However, we did not include this study because it did not provide sufficient methodological detail to be replicable (e.g., it did not describe the instrument, how many administrators participated, how participants were identified, etc.).

Figure 2.5: Flow of studies diagram



3. Quantitative studies excluded in final screening

As shown in Table 3.1, four studies were reviewed in final screening but were ultimately excluded because they do not demonstrate baseline equating of groups (See Appendix 2.6 for a full list of excluded studies). Furthermore, two of the studies (Henriquez *et al.*, 2010 and Gallego, 2004) do not directly evaluate the impact of the voucher programme on school access; rather, they compare outcomes in different types of schools (all voucher subsidised). After reviewing the abstracts for the many Chile studies that were identified in the searches, four were chosen for final screening because they appeared to have the most rigorous designs. For the final screening process, two authors carefully reviewed the full text of the studies and structured cases were written for each study detailing the methodology, findings, and contextual issues (see Appendix 2.5).

Henriquez *et al.* (2010) used propensity score based methods to compare the performance of students in public voucher schools with that of students attending SIP schools – a network of private voucher schools. They found that SIP students outperformed public school students, but did not provide evidence of baseline group equivalency or having accounted for clustering at the school level.

Hsieh and Urquiola (2006) employed panel data for about 150 municipalities and OLS regression methods to measure the impact of the voucher programme on average test scores. No evidence was found that choice improved average educational outcomes as measured by test scores, repetition rates, and years of schooling. They also compared the performance of Chilean students in international tests in science and mathematics, which showed that after 20 years' unrestricted school choice, the performance of the median Chilean student has not improved relative to that of the median student in other countries. The study concluded that this lack of achievement might be caused by the following: public schools not having significant incentives to compete; authorities not providing enough information for parents to determine a school's quality; private schools responding to competitive pressures by choosing better students rather than by raising productivity. Hsieh and Urquiola also found that the voucher programme had led to increased sorting, in which the 'best' public school students left public schools for the private sector. While they did provide baseline measures for one outcome (years of schooling), this study was ineligible because, as with the other Chile studies, it did not identify clear treatment and control groups.

Bravo *et al.* (2010) also employed OLS regression to develop a behavioural model of school attendance and work decisions that incorporated multiple channels through which voucher reforms could operate. They then used the model to evaluate how the introduction of vouchers affected school choice, educational attainment, earnings and labour market participation by individuals differentially exposed to the voucher reform. Bravo and colleagues found large effects of Chile's voucher programme on graduation rates and college attendance, and reduced labour force participation for high school and college-age students. However, they did not demonstrate baseline equivalence.

Finally, Gallego (2004) used an instrumental variables analysis to examine the effects of voucher school competition on expenditures, productivity and student composition at the school level, and whether competition improved test scores by increasing expenditures on education. Gallego found that once the ratio of voucher-to-public schools in an area was instrumented for, one additional voucher school per public school increased test scores by about 0.14 standard deviations. These results were roughly similar for students attending both public and non-

Catholic voucher schools. The study did not establish baseline equivalency and was thus excluded.

These four studies highlight and begin to explore some important issues surrounding the use of school vouchers to increase supply through partnering with the private education sector and to attempt to raise educational quality and student achievement. These issues include the role of competition among schools in increasing productivity, drivers of parental decision-making in an environment of school choice, selection practices of private schools, and resulting socio-economic sorting. Overall, it is very difficult to draw definitive conclusions from the literature on the Chile vouchers experience. For example, the designs used in these studies can be problematic. Kremer and Holla (2008) warn that creative econometric techniques used to address omitted variables bias may lead to misleading results and the risk of data mining for statistically significant results.

Table 3.1: Quantitative studies excluded in final screening

Study	Methodology	Outcomes
Henriquez *et al*. (2010)	Propensity score based estimation	Students at SIP schools (a network of private voucher schools) performed much better than similar students at other schools.
Hsieh and Urquiola (2006)	OLS	There was no evidence of improved educational outcomes as measured by test scores, repetition rates and years of schooling. The voucher programme led to public school students leaving for the private sector.
Bravo *et al*. (2010)	OLS	Over the entire schooling career: primary school graduation rates increased by 0.6%; high school graduation by 3.6%; college attendance by 3.1%; and 4-year college completion by 1.8%. The number of students aged 16-25 in the labour force was reduced by about 2%.
Gallego (2004)	IV	Once the ratio of voucher-to-public schools in an area was instrumented for, 1 additional voucher school per public school increased test scores by about 0.14 standard deviations. The results were roughly similar for students attending public and non-Catholic voucher schools.

Note: In all the studies, the country involved was Chile and the intervention was per-student vouchers paid directly to the school by the central government.

4. Included impact studies

As described earlier, we searched for evaluative studies of school voucher programmes in developing countries and identified only two that met our inclusion criteria. One reason for the shortage of evaluative studies is that relatively few school voucher programmes have been implemented in the developing world. In an international review of public-private partnerships in basic education, LaRocque (2008) identifies only four initiatives in developing countries that meet our definition of school voucher programmes, including those in Chile, Colombia, and Pakistan discussed here. (The fourth, a voucher-like scheme in Côte d'Ivoire, has, to our knowledge, not been empirically evaluated.) As mentioned previously, in the case of Chile – arguably the largest and most widely known education voucher programme in the developing world – the national scale of the programme does not facilitate rigorous comparative evaluation, as it is very difficult to identify an appropriate control group. Although we came across a large number of studies related to the Chile voucher experience, due to the nature of the data available, these studies do not demonstrate adequate baseline controls and do not evaluate the impact of vouchers on access to education; rather, the majority compare educational outcomes in public vs. private schools (both voucher-financed). Table 4.1 summarises the two included impact evaluations.

4.1 Colombia – PACES

Angrist *et al*. (2002) evaluated the Colombian PACES programme, which used a lottery system (when demand exceeded supply) to provide over 125,000 students with vouchers covering somewhat more than half the cost of tuition in private secondary schools. The PACES programme was intended to expand school capacity and to raise secondary enrolment rates of students from neighbourhoods in Colombia's two lowest (of six) socio-economic strata. In most cases, the vouchers did not cover the full cost of tuition in participating private secondary schools and participants had to supplement with additional household funds.

Students received their vouchers upon entering grade 6 and could renew them through to the end of high school, provided they were promoted each year. To receive a voucher through the PACES programme, students had to have attended public primary school and have already been accepted into a participating private secondary school. Such schools tended to serve lower-income students and to have less expensive tuition than non-participating private schools. They also had test scores, facilities and student-teacher ratios that were similar to public schools, but significantly lower than those of non-participating private schools. Thus, the programme facilitated the access of the lowest-income public school students to lower-tier private schools serving low-income populations.

Angrist and colleagues (2002) took advantage of the PACES lottery system to employ an experimental design comparing outcomes of lottery winners and losers. After three years, there were no significant enrolment differences between lottery winners and losers – most students in both groups were still enrolled – but lottery winners were 15 percentage points more likely to attend a private school, had completed 0.1 additional years of schooling, and were 10 percentage points more likely to have completed 8th grade than lottery losers (due to decreased grade repetition). On achievement tests administered by the researchers, lottery winners scored 0.2 standard deviations higher than losers, with the effect for girls larger than that for boys. Non-academic outcomes included lottery winners being less likely to be married or cohabiting and working 1.2 fewer hours per week. The results suggested that the choice between public and private schooling was price-

sensitive, while the decision to attend school at all was not (as mentioned above, to be eligible for the programme, students had to already be accepted at a participating private school; thus, these students already intended to enrol in secondary school). However, while the programme may not have increased the overall enrolment rate in Colombia, it may have opened up public school spaces in schools that had to turn students away due to overcrowding.

In their later report of a six-year follow-up study, Angrist *et al*. (2004) found that the PACES programme increased secondary school completion rates by 15-20 percentage points and increased test scores by 0.2 standard deviations in the distribution of potential test scores, particularly for boys in mathematics. Lamarche (2011) conducted additional analyses indicating that the incentive effect of the programme increased weak students' test scores by at least 0.1 standard deviations, roughly the score gain associated with a half year of school learning.

Further analyses by Bettinger *et al*. (2010) cast doubt on an argument by sceptics that achievement gains by voucher students could be attributed to the peer effect – that is, the voucher gave the student access to better peers, who then influenced them. Thus, re-sorting does not have an overall positive effect on average scores of the general population. However, in examining outcomes of PACES lottery winners who chose to use their vouchers at private vocational schools (which are generally lower quality) rather than private academic schools, Bettinger, *et al*. found that lottery winners still had better educational outcomes than lottery losers.

Angrist and colleagues (2002) interpreted their findings to suggest that demand-side subsidies such as PACES could cost-effectively increase education outcomes for students in poor countries with a well-established private education sector. Channels through which the PACES intervention may have effected positive academic change included offering students the opportunity to move from public to possibly a higher-quality private secondary school or to a more elite private school than they would have otherwise attended. In addition, the voucher system might have incentivised participants to work harder in school because only those students who were not retained in their grade could renew the voucher.

In terms of cost-effectiveness of the PACES programme, Angrist and colleagues (2002) estimated that voucher winners had to 'top up' the voucher by 70 percent of its value to cover private school costs and also worked less; thus lottery winners contributed more than the value of the voucher to their education. They also estimated that the government paid $24 more per voucher winner than it would have cost to create a public school placement for that student. In their initial cost-benefit analysis, Angrist *et al*. concluded that these costs were more than offset by the value of participants' future earnings. However, Angrist and colleagues pointed out that to more fully calculate the true costs of the programme, its effect on non-participants would need to be examined. For example, did decreased peer quality negatively affect students remaining in public schools? Or, possibly, could increased competition for students have induced public schools to improve education quality?

4.2 Pakistan – Quetta Urban Fellowship Programme

Although voucher programmes in developed countries generally have the goal of improving overall school quality by introducing competition amongst public and private schools, developing countries may also view public-private partnerships as a way to relieve pressure on a weak public education infrastructure by increasing enrolment in private schools. Introducing competition through vouchers may be a goal for such systems as well, but secondary to that of increasing overall enrolment by providing spaces in existing private schools or incentivising the creation of new

schools by the private sector. In the case of Pakistan, which has a large gender gap favouring boys, supply constraints are enhanced by the fact that more girls' schools and schools with female teachers are needed to increase girls' enrolment (Kim *et al.*, 1999).

To address this reality, the Balochistan Education Foundation initiated the Quetta Urban Fellowship Programme in 1995. This pilot project paid subsidies per girl enrolled directly to new private primary schools with female teachers established in poor urban areas (boys were also allowed to enrol but subsidies were not provided for them). Kim and colleagues (1999) conducted an experiment to evaluate the programme, using randomisation to identify treatment and control neighbourhoods. The study found that girls' enrolment increased by 33 percentage points; boys' enrolment rose as well. Household income had only a moderate impact on participation. In terms of cost-effectiveness, the researchers estimated that establishing a fellowship school was substantially less expensive than establishing a government school, and that it would take a sizeable income transfer to households to achieve the same enrolment gain generated by the programme. Insofar as the impact resulted from reducing the distance to schools, a fourfold increase in the number of schools would be needed to achieve the same effect. Thus, the programme appeared to be cost-effective relative to alternative interventions. However, education quality and school sustainability issues would need to be examined to fully determine its effectiveness.

Table 4.1: Characteristics of included evaluations

Study	Country	Inter-vention	Group targeted	Sample size	Study design	Method-ological threats to evaluation design	Follow-up period	Outcomes: school enrolment	Outcomes: progression	Outcomes: achievement
Angrist *et al.* (2002)	Columbia	Voucher programme for children to attend private secondary school	Children living in poor neighbour-hoods (2 lowest socio-economic groups)	Treatment: 583 children Control: 593 children	QED (IV – 2SLS)	Low	3 years (6-year follow-up by Angrist *et al.*, 2004)	ES: 0.03 (−0.12, 0.18) No significant difference between children who won voucher and those who did not 3 years after applying for it	ES: 0.18 (0.04, 0.31) Lottery winners completed an additional 0.1 years of school, were about 10% more likely to complete 8th grade and scored about 0.2 standard deviations higher than lottery losers	ES Mathematics: 0.15 (−0.07, 0.38) ES Language: 0.17 (−0.05, 0.38) Lottery winners less likely to be married or cohabiting and worked 1.2 fewer hours per week than lottery losers. More effect on girls than boys
Kim *et al.* (1999)	Pakistan	Subsidy paid directly to schools for 3 years	Low-income primary school girls in Quetta, Balochistan Province	Treatment: 1,310 children (781 girls, 529 boys). Control: 1,358 children (697 girls,	RCT	Low	2 years	ES: 0.14 (0.10, 0.19) 33% increased enrolment for boys and girls; effect mostly larger for girls	Pilot achievement test in 3rd grade showed no significant differences between subsidy and government schools, but results were not	N/A

Study	Country	Inter-vention	Group targeted	Sample size	Study design	Method-ological threats to evaluation design	Follow-up period	Outcomes: school enrolment	Outcomes: progression	Outcomes: achievement
				661 boys)					definitive due to small sample size	

5. Limitations and future research direction

Using a systematic and rigorous searching, identification and screening process, this review has highlighted the dearth of rigorous impact evidence on which to base school voucher policy in the developing nation context. Although our inclusion criteria encompassed descriptive quantitative and qualitative methodologies, in addition to more rigorous experimental and quasi-experimental designs, we did not identify such studies in our search. The vast majority of developing nation voucher studies evaluate Chile's long-running nationwide voucher programme, focusing primarily, in the absence of adequate baseline data or control groups, on the make-up and relative effectiveness of different types of public and private voucher-financed schools. Of the 53 studies that were reviewed in the secondary and final screening processes, only two examined a programme other than the Chilean voucher system. This is partly due to the fact that relatively few school voucher programmes have been implemented in developing countries.

The two included evaluations examine interventions that differ in the level of schooling targeted (secondary/primary) and the means of providing the subsidy (directly to students/directly to schools). In addition, the Pakistan programme targeted girls with the goal of increasing their primary enrolment, while the Colombia programme encouraged secondary school students to switch to more desirable schools and remain enrolled. The Pakistan programme resulted in girls being educated for less than it would have cost for the government to create public school spaces, while the Colombia programme cost rather more, but is speculated to be cost-effective in terms of long-term economic gains. Both programmes increased private school enrolment amongst the countries' poorest income groups, thus probably improving equity. It would appear that both programmes could be successful if repeated – the Quetta programme in other poor neighbourhoods of Pakistan and countries facing similar gender-specific supply constraints and a programme like PACES in countries similarly confronting a weak public education infrastructure with a well-developed private education sector.

However, these evaluations do not examine how the voucher programmes affect public education providers and whether quality and intake is affected as a result of students leaving for private schools. Additionally, the studies do not explore access issues, such as detailed characteristics of students that access private schooling as a result of the voucher programmes, and which groups remain in government schools. Nor do they address the impact of schooling costs not covered by the vouchers, such as uniforms, transportation and additional school fees assessed to families.

Clearly, more rigorous research in developing country contexts is necessary to explore these related issues and to determine whether the gains from these two programmes can be replicated and enhanced. In addition, future research should attempt to pinpoint the source of any achievement gains in both public and private schools associated with voucher programmes – whether due to schools improving in response to increased competition, to peer effects, or to inherent efficiencies of private schools. For example, voucher programmes may not reward the pedagogical innovation that would improve educational quality if parents prefer the status quo (Gauri and Vawda, 2003). Or schools may demonstrate spurious quality improvement by selecting the best students – introducing significant bias into the system (Gonzalez, *et al.*, 2004). Furthermore, as in the case of Chile, if voucher schools have access to several alternative sources of funding, the need for raising

competitiveness through improved productivity could be diminished (Sapelli and Vial, 2002).

Elucidating these complex issues surrounding voucher programmes in developing country contexts will enable governments to design innovative initiatives and target resources most efficiently. These issues include the following important areas highlighted in the conceptual framework as potential externalities of voucher programmes to be explored in additional rigorous piloting and research (see Arenas, 2004):

- *Educational quality*: Do voucher recipients improve academically? How does the departure of voucher students affect educational outcomes for the remaining students? Do outcomes improve for the population overall?
- *Sorting*: Will voucher schools select only the most advantaged students? Will public schools become overattended by disadvantaged students?
- *Equity*: Do vouchers improve equity by providing poor families with access to better education, generally restricted to higher-income groups? Or, will families with the greatest social capital and with the least need for vouchers be most likely to benefit, as such families have better access to information and the time and motivation to seek the best schools for their children?

In terms of equity, the Chilean experience with a flat voucher system (i.e. the school receives the same per-pupil amount, regardless of the SES of the student) is generally considered to perpetuate educational inequity, with the poorest students concentrated in low-performing municipal schools (see, e.g. Gonzalez, *et al.*, 2004). On the other hand, an income-related 'means-tested' voucher system compensates schools for the higher challenge of educating disadvantaged students.

Drawing on emerging research on public-private partnerships (PPPs) for education (e.g. Wössmann, 2006;[4] LaRocque, 2008), Muralidharan (2006) proposes a voucher programme for India that would leverage what he finds to be the efficiencies of the burgeoning low-cost private sector and would give low-income families access to the same set of education choices available to more advantaged children. The amount of the voucher would be determined by a composite measure (based on income, family educational background, gender, caste), with larger vouchers allocated to socio-economically disadvantaged students and students with special needs, and could include incentives for attendance and achievement. The progressive, or income-related, voucher would ensure that the poorest students do not remain marginalised in 'ghettoised' government schools that are deserted by relatively better-off students – a phenomenon reported by observers of the growth in low-cost private schools for the poor (e.g. Härmä, 2010).

Muralidharan (2006) further advocates the forming of partnerships among governments, academics and the donor community to pilot and evaluate small voucher programmes of various configurations to generate evidence and understanding on which to base policy and practice. It has been shown in recent years that academics can work with NGOs relatively cheaply to test the effectiveness of education interventions in developing countries (Kremer and Holla, 2008). To facilitate rigorous and valid impact evaluation, random assignment of

[4] Wössmann (2006) uses the results of the PISA student achievement test to compare outcomes for different configurations of public-private education partnerships across countries, and finds that PPP systems that combine public funding with private operation have the best student outcomes.

groups is key. Cost-effective data collection and comparison across programme variations can be facilitated by conducting a series of related evaluations in similar settings and with cross-cutting treatment designs (Kremer and Holla, 2008).

Keeping voucher programmes relatively small and targeted may be advisable, as educational improvement among large numbers of voucher students may be difficult in developing countries facing institutional constraints (Gauri and Vawda, 2003). It would appear that implementing pilot programmes similar to the Quetta fellowship programme, or to PACES on a smaller scale, would provide a means to examine these issues and to determine if scale-up could be desirable and sustainable.

Some promising avenues for developing countries seeking to pilot equitable voucher programmes might include the following (see Arenas, 2004):

- Target poor students;
- Provide vouchers that cover the entire cost of tuition and prohibit assessment of supplemental fees;
- Offer schools larger vouchers for less advantaged students;
- Subsidise transportation to facilitate school choice;
- Establish monitoring systems to ensure that schools do not misrepresent enrolments and that they accept students of all profiles on a lottery system; and
- Provide meaningful and accessible information to parents.

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Appendices

Appendix 1.1: Authorship of this report

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Appendix 2.1: Databases and organisations searched for this project

- 3ie
- African Studies Centre Catalogue
- African Women Bibliographic Database
- Africana Periodical Literature
- Association for the Development of Education in Africa
- British Education Index
- British Library for Development Studies Catalogue
- CREATE
- DFID
- EBSCO
- EconLit
- Education FullText
- Eldis
- ERIC
- Google Scholar
- IDEAS
- Index to Current Urban Documents
- International Bibliography of the Social Sciences
- ISI Web of Knowledge
- JOLIS
- National Bureau of Economic Research Working Papers (NBER)
- PAIS
- Periodicals Index Online
- PolicyFile
- Proquest Dissertations and Theses
- Social Science Research Network
- Sociological Abstracts
- Southern and Eastern Africa Consortium for Monitoring Educational Quality (SACMEQ) Papers and Theses
- UNESCO
- USAID
- World Bank
- Worldwide Political Science Abstracts

Journals handsearched

- Economic Development and Cultural Change
- International Journal of Education Development
- Journal of Development Economics
- World Bank Economic Review
- World Bank Research Observer

Additional bibliographic databases that were searched for the 3ie project

- Academic Search Premiere
- Education Index
- Expanded Academic ASAP
- Full-Text of Ovid Journals
- Health Technology Assessment Register
- Housing and Urban Development (U.S.) 'HUD USER'
- Medline
- National Clearinghouse of Child Abuse and Neglect (NCCAN)
- Psychology and Behavioural Sciences Collection
- PsycInfo (includes PsychLit)
- Sage Family Studies Abstracts
- Sage Journals Management and Organizational Studies Full-Text Collection
- Sage Journals Political Science Full-Text Collection
- Sage Journals Sociology Full-Text Collection
- Sage Urban Studies Abstracts
- Social Service Abstracts
- Social Service Research Network
- Social Work Abs
- tracts

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Appendix 2.3: Search terms and results

Search terms

We searched titles and abstracts and, where possible, and used thesaurus and other controlled vocabulary terms related to vouchers identified in the databases. Where possible, we combined voucher terms – such as 'voucher', 'subsidy', 'subsidies', 'subsidize', 'subsidise', 'school choice', 'chit', 'tax credit', 'tax stipend', 'stipend' – with the names of the developing countries of focus. We also combined the voucher terms and country names with general terms for developing nations, such as 'developing nation', 'developing region', 'developing country', 'developing countries', 'third world nation', 'third world country', 'third world countries', 'third world region', 'low income nation', 'low income country', 'low income countries', 'low income region', 'LIC country', 'LIC countries', 'emerging nations', 'underdeveloped nations'. In addition, where possible in the databases, results were restricted to primary and secondary education topics.

The specific search term combinations used and the yield obtained for each database follow. The years searched were 1991–2011. Where possible, search strings are copied directly from the search results.

Source	Search terms	Yield	Hits
3ie	voucher	7	5
	credit	6	0
	aid	13	0
	private	13	1
	grant*	15	0
	subsid*	16	2
	choice	6	0
	fellowship	1	0
	stipend	2	0
	scholarship	11	1
Africana Periodical Literature	'educational vouchers'	1	0
	grant	2	0
	credit	8	0
	aid	10	0
	'private school'	2	0
	'school choice'	5	0
	subsid*	3	0
African Studies Centre Catalogue	search [and] (words from [the] abstract) education subsidies	20	0
	search [and] (words from [the] abstract) private school	30	0
African Women	grant	3	0

Source	Search terms	Yield	Hits
Bibliographic Database	aid	2	0
	private	11	0
	'school choice'	3	0
	credit	9	0
Association for the Development of Education in Africa	voucher	0	0
	subsidy	0	0
British Education Index	([Education Vouchers], [Education], [Fellowships], [Grants], [Scholarships], [Tuition Grants])	9	0
British Library for Development Studies catalogue	vouchers	6	1
	credit	87	0
	fellowship	4	0
	private	175	1
	choice	43	1
	subsid*	23	0
	grant*	23	0
	stipend	1	0
CREATE	vouchers	8	0
	stipends	2	0
	grant	4	0
	scholarship	5	0
	choice	6	0
	private	95	0
DFID	'education voucher'	5	0
	education AND subsid*	27	0
	education AND grant	14	0
EBSCO	SU 'educational vouchers' AND TX (Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR Armenia OR Azerbaijan OR Bangladesh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR Byelorussia OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon OR 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad		

Source	Search terms	Yield	Hits
	OR Chile OR Colombia OR China OR Comoros OR 'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR Cuba OR Djibouti OR 'French Somaliland' OR 'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR Abyssinia OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR Ghana OR 'Gold Coast' OR Grenada OR Guinea 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana' Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR 'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal States Micronesia' Moldova OR Moldavia OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR Myanmar OR Burma OR Namibia OR 'South-West Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines OR Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana' OR Swaziland 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR Tanzania OR Thailand OR Siam OR Timor-Leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland' OR Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank and Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe)		

Source	Search terms	Yield	Hits
	SU Tuition tax credits AND (Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR Armenia OR Azerbaijan OR Bangladesh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR Byelorussia OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon OR 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad OR Chile OR Colombia OR China OR Comoros OR 'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR Cuba OR Djibouti OR 'French Somaliland' OR 'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR Abyssinia OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR Ghana OR 'Gold Coast' OR Grenada OR Guinea 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana' Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR 'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal States Micronesia' Moldova OR Moldavia OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR Myanmar OR Burma OR Namibia OR 'South-West Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines OR Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana'	41	0

Source	Search terms	Yield	Hits
	OR Swaziland 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR Tanzania OR Thailand OR Siam OR Timor-Leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland' OR Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank and Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe)		
EconLit	all((voucher OR subsid*) AND (education OR school)) AND all((chile OR colombia))	100	29
	all((voucher OR subsidy*) AND (education OR school)) AND all(Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR Armenia OR Azerbaijan OR bangla desh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR byelorussian OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon OR 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad OR China OR Comoros OR 'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR 'Ivory Coast' OR Cuba OR Djibouti OR 'French Somaliland' OR 'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR abyssinian OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR Ghana OR 'Gold Coast' OR Grenada OR Guinea OR 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana' OR Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR 'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania OR Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal	559	4

Source	Search terms	Yield	Hits
	States Micronesia' OR Moldova OR moldavian OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR Myanmar OR Burma OR Namibia OR 'South- West Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria OR Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines OR Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana' OR Swaziland OR 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR Tanzania OR Thailand OR Siam OR timor leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland' OR Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank AND Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe)		
	all((voucher OR subsidy*) AND (education OR school)) AND all('developing nation' OR 'developing region' OR 'developing country' OR 'third world nation' OR 'third world country' OR 'third world region' OR 'low income nation' OR 'low income country' OR 'low income region' OR 'upper middle income nation' OR 'upper middle income country' OR 'upper middle income region' OR 'lower middle income nation' OR 'lower middle income country' OR 'lower middle income region' OR 'impoverished country' OR 'impoverished region')	27	0
Education FullText	DE 'Educational vouchers' AND TX (Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR Armenia OR Azerbaijan OR Bangladesh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR Byelorussia OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon OR 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad OR Chile OR Colombia OR China OR Comoros OR	58	1

Source	Search terms	Yield	Hits
	'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR Cuba OR Djibouti OR 'French Somaliland' OR 'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR Abyssinia OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR Ghana OR 'Gold Coast' OR Grenada OR Guinea 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana' Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR 'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal States Micronesia' Moldova OR Moldavia OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR Myanmar OR Burma OR Namibia OR 'South-West Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines OR Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana' OR Swaziland 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR Tanzania OR Thailand OR Siam OR Timor-Leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland' OR Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank and Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe		
	Voucher AND (Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR	22	6

Source	Search terms	Yield	Hits
	Armenia OR Azerbaijan OR Bangladesh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR Byelorussia OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon OR 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad OR Chile OR Colombia OR China OR Comoros OR 'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR Cuba OR Djibouti OR 'French Somaliland' OR 'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR Abyssinia OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR Ghana OR 'Gold Coast' OR Grenada OR Guinea 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana' Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR 'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal States Micronesia' Moldova OR Moldavia OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR Myanmar OR Burma OR Namibia OR 'South-West Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines OR Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana' OR Swaziland 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR		

Source	Search terms	Yield	Hits
	Tanzania OR Thailand OR Siam OR Timor-Leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland' OR Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank and Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe)		
Eldis	vouchers	74	4
	education voucher	22	3
	subsidy	232	2
ERIC	((Thesaurus Descriptors:'Educational Vouchers' OR Thesaurus Descriptors:'School Vouchers' OR Thesaurus Descriptors:'tuition tax credits' OR Thesaurus Descriptors:'tuition grants' OR Thesaurus Descriptors:'private school aid' OR Thesaurus Descriptors:'school choice' OR Thesaurus Descriptors:'fellowships') and (Thesaurus Descriptors:'third world countries' OR Thesaurus Descriptors:'developing nations' OR Thesaurus Descriptors:'emerging nations')) and (Education Level:'Elementary Education' OR Education Level:'Elementary Secondary Education' OR Education Level:'Grade 1' OR Education Level:'Grade 2' OR Education Level:'Grade 3' OR Education Level:'Grade 4' OR Education Level:'Grade 5' OR Education Level:'Grade 6' OR Education Level:'Grade 7' OR Education Level:'Grade 8' OR Education Level:'Grade 9' OR Education Level:'Grade 10' OR Education Level:'Grade 11' OR Education Level:'Grade 12' OR Education Level:'High Schools' OR Education Level:'Intermediate Grades' OR Education Level:'Junior High Schools' OR Education Level:'Kindergarten' OR Education Level:'Middle Schools' OR Education Level:'Primary Education' OR Education Level:'Secondary Education')Publication Date:1991-2012	18	0
	((Thesaurus Descriptors:'educational vouchers' OR Thesaurus Descriptors:'school vouchers' OR Thesaurus Descriptors:'tuition tax credits' OR Thesaurus Descriptors:'tuition grants' OR Thesaurus Descriptors:'private school aid' OR Thesaurus Descriptors:'school choice' OR Thesaurus Descriptors:'fellowships') and (Keywords:Chile OR Keywords:Colombia)) and (Education Level:'Elementary Education' OR	34	14

Source	Search terms	Yield	Hits
	Education Level:'Elementary Secondary Education' OR Education Level:'Grade 1' OR Education Level:'Grade 2' OR Education Level:'Grade 3' OR Education Level:'Grade 4' OR Education Level:'Grade 5' OR Education Level:'Grade 6' OR Education Level:'Grade 7' OR Education Level:'Grade 8' OR Education Level:'Grade 9' OR Education Level:'Grade 10' OR Education Level:'Grade 11' OR Education Level:'Grade 12' OR Education Level:'High Schools' OR Education Level:'Intermediate Grades' OR Education Level:'Junior High Schools' OR Education Level:'Kindergarten' OR Education Level:'Middle Schools' OR Education Level:'Primary Education' OR Education Level:'Secondary Education')Publication Date:1990-2012		
Google Scholar	(school AND education AND voucher AND choice AND private) AND ('developing nation' OR 'developing region' OR 'developing country' OR 'developing countries' OR 'third world countries' OR 'third world country')	170	19
	(voucher OR subsid*) AND (education OR school) AND ('developing nation' OR 'developing region' OR 'developing country' OR 'developing countries' OR 'third world countries' OR 'third world country')	500	5
International Bibliography of the Social Sciences	'voucher and Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR Armenia OR Azerbaijan OR Bangladesh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR Byelorussia OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon'	13	0
	'voucher and 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad OR Chile OR China OR Colombia OR Comoros OR 'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR Cuba OR Djibouti'	20	10
	voucher and 'French Somaliland' OR 'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR Abyssinia OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR	8	0

Source	Search terms	Yield	Hits
	Ghana OR 'Gold Coast' OR Grenada OR Guinea OR 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana'		
	voucher and Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR 'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania	6	0
	voucher and Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal States Micronesia' OR Moldova OR Moldavia OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR Myanmar OR Burma OR Namibia OR 'South-West Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria OR Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines	9	0
	voucher and Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana' OR Swaziland OR 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR Tanzania OR Thailand OR Siam OR Timor-Leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland'	19	0
	voucher and Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank and Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe	3	0
	voucher and 'developing nation' OR 'developing region' OR 'developing country' OR 'third world nation' OR 'third world country' OR 'third world region' OR 'low income nation' OR 'low income country' OR 'low income region' OR 'upper middle	4	0

Source	Search terms	Yield	Hits
	income nation' OR 'upper middle income country' OR 'upper middle income region' OR 'lower middle income nation' OR 'lower middle income country' OR 'lower middle income region' OR 'impoverished country' OR 'impoverished region'		
IDEAS	school AND voucher	256	24
Index to Current Urban Documents	voucher	14	0
	subsidy AND education	0	0
ISI Web of Knowledge	voucher AND (Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR Armenia OR Azerbaijan OR Bangladesh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR Byelorussia OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon OR 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad OR Chile OR Colombia OR China OR Comoros OR 'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR Cuba OR Djibouti OR 'French Somaliland' OR 'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR Abyssinia OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR Ghana OR 'Gold Coast' OR Grenada OR Guinea 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana' Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR 'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal States Micronesia' Moldova OR Moldavia OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR Myanmar OR Burma OR Namibia OR 'South-West	397	13

Source	Search terms	Yield	Hits
	Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines OR Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana' OR Swaziland 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR Tanzania OR Thailand OR Siam OR Timor-Leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland' OR Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank and Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe)		
JOLIS	Keywords anywhere 'voucher or subsid*' AND Subject 'education' 'tax credit' AND Keywords anywhere 'education'	16 14	3 0
National Bureau of Economic Research Working Papers (NBER)	'school voucher' 'educational voucher' 'education subsidies'	48 2 40	5 0 0
PAIS	all((voucher OR subsid*)) AND all((school OR education)) AND all(Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR Armenia OR Azerbaijan OR bangla desh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR byelorussian OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon OR 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad OR Chile OR China OR Colombia OR Comoros OR 'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR 'Ivory Coast' OR Cuba OR Djibouti OR 'French Somaliland' OR	133	2

Source	Search terms	Yield	Hits
	'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR abyssinian OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR Ghana OR 'Gold Coast' OR Grenada OR Guinea OR 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana' OR Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR 'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania OR Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal States Micronesia' OR Moldova OR moldavian OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR Myanmar OR Burma OR Namibia OR 'South- West Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria OR Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines OR Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana' OR Swaziland OR 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR Tanzania OR Thailand OR Siam OR timor leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland' OR Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank AND Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe OR 'developing nation' OR 'developing region' OR 'developing country' OR 'third world nation' OR 'third world country' OR 'third world region' OR 'low income nation' OR 'low income country' OR 'low income region' OR 'upper middle income nation' OR 'upper middle income country' OR		

Source	Search terms	Yield	Hits
	'upper middle income region' OR 'lower middle income nation' OR 'lower middle income country' OR 'lower middle income region' OR 'impoverished country' OR 'impoverished region')		
Periodicals Index Online	voucher	8	0
	chit	8	0
PolicyFile	Keyword(s): voucher or subsid* Organization Type(s): 'Finance and Economics'; 'Societal'; 'U.S. Foreign'; 'International' Subject(s): 'Education'	136	4
Proquest Dissertations and Theses	'education voucher' AND (Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR Armenia OR Azerbaijan OR Bangladesh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR Byelorussia OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon OR 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad OR Chile OR Colombia OR China OR Comoros OR 'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR Cuba OR Djibouti OR 'French Somaliland' OR 'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR Abyssinia OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR Ghana OR 'Gold Coast' OR Grenada OR Guinea 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana' Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR 'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal States Micronesia' Moldova OR Moldavia OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR	139	9

Source	Search terms	Yield	Hits
	Myanmar OR Burma OR Namibia OR 'South-West Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines OR Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana' OR Swaziland 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR Tanzania OR Thailand OR Siam OR Timor-Leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland' OR Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank and Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe)		
Sociological Abstracts	voucher AND (Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR Armenia OR Azerbaijan OR Bangladesh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR Byelorussia OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon OR 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad OR Chile OR Colombia OR China OR Comoros OR 'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR Cuba OR Djibouti OR 'French Somaliland' OR 'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR Abyssinia OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR Ghana OR 'Gold Coast' OR Grenada OR Guinea 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana' Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR	62	5

Source	Search terms	Yield	Hits
	'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal States Micronesia' Moldova OR Moldavia OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR Myanmar OR Burma OR Namibia OR 'South-West Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines OR Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana' OR Swaziland 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR Tanzania OR Thailand OR Siam OR Timor-Leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland' OR Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank and Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe)		
Social Science Research Network	voucher	261	2
UNESCO	'education voucher'	137	0
	'tuition subsidy'	70	0
	'tax credit'	174	0
	'tax stipend'	12	0
USAID	'school voucher'	12	0
	'education voucher'	13	0
	voucher	37	0
World Bank	'education voucher'	1	1
	'school voucher'	1	0

Source	Search terms	Yield	Hits
	in the subtopic Economics of Education: 'education voucher' in Policy Research Working Papers: 'education voucher'	16 12	1 1
Worldwide Political Science Abstracts	all(vouchers) AND all(Afghanistan OR Albania OR 'American Samoa' OR Angola OR Argentina OR Armenia OR Azerbaijan OR bangla desh OR 'East Pakistan' OR 'East Bengal' OR Belarus OR byelorussian OR Belize OR 'British Honduras' OR Benin OR Dahomey OR Bhutan OR Bolivia OR 'Bosnia Herzegovina' OR Botswana OR Bechuanaland OR Brazil OR Bulgaria OR 'Burkina Faso' OR 'Upper Volta' OR Burundi OR Cambodia OR 'Khmer Republic' OR Kampuchea OR Cameroon OR 'Cape Verde' OR 'Central African Republic' OR Ubangi-Shari OR 'Central African Empire' OR Chad OR Chile OR China OR Colombia OR Comoros OR 'Democratic Republic Congo' OR Zaire OR 'Belgian Congo' OR 'Republic Congo' OR 'Middle Congo' OR 'Costa Rica' OR 'Cote d'Ivoire' OR 'Ivory Coast' OR Cuba OR Djibouti OR 'French Somaliland' OR 'Afars Issas' OR Dominica OR 'Dominican Republic' OR Ecuador OR Egypt OR 'Arab Republic Egypt' OR 'El Salvador' OR Eritrea OR Ethiopia OR abyssinian OR Fiji OR Gabon OR 'The Gambia' OR Georgia OR Ghana OR 'Gold Coast' OR Grenada OR Guinea OR 'French Guinea' OR Guinea-Bissau OR 'Portuguese Guinea' OR Guyana OR 'British Guiana' OR Haiti OR Honduras OR India OR Indonesia OR 'Netherlands East Indies' OR Iran OR 'Islamic Republic Iran' OR Persia OR Iraq OR Jamaica OR Jordan OR Kazakhstan OR Kenya OR Kiribati OR 'Gilbert Islands' OR 'Democratic Republic Korea' OR 'North Korea' OR Kosovo OR 'Kyrgyz Republic' OR Lao OR 'Lao PDR' OR Latvia OR Lebanon OR Lesotho OR Basutoland OR Liberia OR Libya OR Lithuania OR Macedonia OR 'Macedonia FYR' OR Madagascar OR Malawi OR Nyasaland OR Malaysia OR Malaya OR Maldives OR Mali OR 'French Sudan' OR 'Marshall Islands' OR Mauritania OR Mauritius OR Mayotte OR Mexico OR Micronesia OR 'Federal States Micronesia' OR Moldova OR Moldavian OR Mongolia OR Montenegro OR 'Federal Republic Yugoslavia' OR Morocco OR Mozambique OR Myanmar OR Burma OR Namibia OR 'South- West Africa' OR Nepal OR Nicaragua OR Niger OR Nigeria OR Pakistan OR Palau OR Panama OR 'Papua New Guinea' OR Paraguay OR Peru OR Philippines OR Poland OR Romania OR 'Russian Federation' OR Rwanda OR Samoa OR 'Western Samoa' OR 'Sao Tome Principe' OR Senegal OR	31	3

Source	Search terms	Yield	Hits
	Serbia OR Seychelles OR 'Sierra Leone' OR 'Solomon Islands' OR Somalia OR 'South Africa' OR 'Sri Lanka' OR Ceylon OR 'St. Kitts' OR Nevis OR 'St. Lucia' OR 'St. Vincent' OR Grenadines OR Sudan OR 'Anglo-Egyptian Sudan' OR Suriname OR 'Dutch Guiana' OR Swaziland OR 'Syrian Arab Republic' OR Syria OR Tajikistan OR 'Tajik Soviet Socialist Republic' OR Tanzania OR Thailand OR Siam OR Timor Leste OR 'East Timor' OR 'Portuguese Timor' OR Togo OR 'French Togoland' OR Tonga OR Tunisia OR Turkey OR Turkmenistan OR Uganda OR Ukraine OR Uruguay OR Uzbekistan OR 'Uzbek Soviet Socialist Republic' OR Vanuatu OR 'New Hebrides' OR Venezuela OR Vietnam OR 'West Bank AND Gaza' OR 'West Bank' OR Gaza OR Yemen OR Zambia OR Zimbabwe OR 'developing nation' OR 'developing region' OR 'developing country' OR 'third world nation' OR 'third world country' OR 'third world region' OR 'low income nation' OR 'low income country' OR 'low income region' OR 'upper middle income nation' OR 'upper middle income country' OR 'upper middle income region' OR 'lower middle income nation' OR 'lower middle income country' OR 'lower middle income region' OR 'impoverished country' OR 'impoverished region')		
International Journal of Education Development	Handsearch		0
Economic Development and Cultural Change	Handsearch		0
World Bank Research Observer	Handsearch		0
Journal of Development Economics	Handsearch		0
World Bank Economic Review	Handsearch		0
SACMEQ	Handsearch	0	0

Appendix 2.4: Coding instrument

DFID Review: Impact of School Vouchers in Developing Countries

Impact Evaluations CODING INSTRUMENT

Coder:

- ❑ Trevor Fronius
- ❑ Anthony Petrosino
- ❑ Claire Morgan
- ❑ Other _____

I. RESEARCHER AND STUDY CHARACTERISTICS

What year was the primary document published? _____

What was the type of document?

- o Book
- o Book Chapter
- o Government Report
- o Technical Report (reports by non-Government research firms, e.g. Mathematica)
- o NGO Report (e.g., World Bank, Poverty Action Lab)
- o Journal (peer reviewed)
- o Dissertation
- o Conference Paper
- o Other

In what country did the evaluation take place? _____

World Bank country classification at time of study

- o Lower Income
- o Lower Middle Income
- o Upper Middle Income

What was the setting for the evaluation? _____

(e.g. urban/rural; slum/non-slum, etc.)

Who conducted the evaluation? (e.g., medical researchers, economists, etc. May be an assumption based on the affiliation)

_____

Baseline enrolment data: Males _____ Females _____

(Use enrolment rates as close in proximity to intervention setting as possible, but if only national rates available, use those)

II. STUDY METHODS AND METHODOLOGICAL QUALITY

What method of assignment was used to assign or form groups?

- ❑ Random Assignment
- ❑ Non-Random Assignment (Quasi-experimental)
- ❑ Combination of Random Assignment and Non-Random Assignment (e.g., randomization only after oversubscription of available 'spots')

If non-random assignment, what procedure was used to assign or form groups?

- ❑ Regression Discontinuity Design
- ❑ Statistical Matching
- ❑ No Equating of Groups
- ❑ Other (Indicate: _____)

If statistical matching used, what procedure was used to match?

- ❑ Propensity Scores
- ❑ Covariate matching
- ❑ Other (Indicate: _____)

At what level was assignment made?

- ❑ Village/Neighbourhood
- ❑ School
- ❑ Classroom
- ❑ Household
- ❑ Individual
- ❑ Other (Indicate:_____)

Methodological Threats to Evaluation Design

Threat	Did it exist?	How extensive? (Percentage of sample)	What did authors do to address?	Rate the Threat to Evaluation Findings about Enrolment (None/Low/Moderate/High)
Crossovers	YES/NO			
Attrition from Original Study Sample	YES/NO			
Attrition of Students from Larger Aggregate Unit Assignment	YES/NO			
Differential Attrition	YES/NO	(Percentage difference between groups)		

Group Inequity at Pre-test			
Number of variables examined	Number of statistically significant differences	What did authors do to address?	Rate the threat to evaluation findings about enrolment (None/Low/Moderate/High)

What was the overall methodological quality?

- ❑ High
- ❑ Medium
- ❑ Low

III. INTERVENTION AND CONTROL CONDITIONS

What was the type of intervention?

- ❑ **Unrestricted, Flat Voucher system**
- ❑ **Targeted Voucher system**
- ❑ **Means-tested Voucher system**
- ❑ **'Follow the Child' system**
- ❑ **Other (Indicate: _____)**

Describe the intervention group below, with particular attention to the 'dosage' of the treatment:

_____
_____
_____
_____
_____

How many cases were randomized or assigned to this group? _____

Programme Implementation/Fidelity

Programme Implementation Issues Mentioned by Authors (Not Possible but Actual)	What did authors do to address?	Rating

Please provide simple programme theory (or mechanisms for why the intervention should work):

_____
_____
_____
_____

What is the control or comparison condition?

- No Treatment Group
- Wait-List Control
- Treatment as Usual Group
- Placebo
- Lesser dose of the same treatment
- Entirely different treatment than what Experimental got
- Other (Indicate:_____)

Describe the control or comparison condition (including 'dosage' and where it came from if applicable):

_____
_____
_____
_____
_____

How many cases were randomized or assigned to this group? _____

IV. PARTICIPANTS IN THE STUDY

Type of school(s):

- ❑ Public
- ❑ Private
- ❑ Religious
- ❑ NGO

Age/grade _____

School Level(s)

- ❑ Primary
- ❑ Secondary
- ❑ Unknown

Percentage of participants that were female _____

Poverty/SES

_____

V. OUTCOMES

What was the overall conclusion or investigator-reported result (IRR)?

- ❑ Positive
- ❑ Null or no effect
- ❑ Negative effect
- ❑ Mixed-Can't discern

SIMPLY INDICATE THE EDUCATION AND NON-EDUCATION OUTCOMES AND WHEN REPORTED (TIME INTERVAL)

Education/Learning Outcome	*Outcome Measurement at What Time Intervals (only those in which data points are reported, e.g., 6 months, 12 months, etc.)*	*NON-EDUCATION OUTCOMES*	*Outcome Measurement at What Time Intervals (only those in which data points are reported, e.g., 6 months, 12 months, etc.)*
Enrolment			
Attendance			
Dropout			
Test Scores			
Grades			
School Quality			
Equity			
Other (List each in a new row)			

Were subgroup effects for treatment reported? (Yes/No)

If so: List:

_____

_____

_____

Was any cost-benefit or economic analysis reported? (Yes/No)

Indicate outcome of economic analysis:

- ❑ Programme Group is more efficient option
- ❑ Comparison/Control Group is more efficient option
- ❑ Programme Group is more efficient than policy alternatives
- ❑ Policy Alternatives are more efficient than programme group
- ❑ No clear distinction between the two groups

ANY OTHER COMMENTS ON THE PROGRAMME OR EVALUATION

Appendix 2.5: Structured cases for studies in final screening[5]

Effective schools do exist: low income children's academic performance in Chile

Francisco Henríquez, Bernardo Lara, Alejandra Mizala, Andrea Repetto

December 2009

Working Document 003, Adolfo Ibanez University, School of Government, May 2010

Location of intervention

Chile

Background/rationale for the study

In the early 1980s, there was sweeping educational reform – the public sector was decentralised and local governments (municipalities) began to manage schools. A voucher-type subsidy was introduced to finance municipal and private voucher schools; hence the private sector entered the market to provide education. The voucher was paid directly to schools on a per-student basis; it was intended to cover running costs and generate competition between schools to attract and retain students, and to promote more efficient and better-quality education.

A system of standardised tests, SIMCE, was established to measure educational attainment, evaluate the success of the reforms, inform parents about the quality of schools and provide a basis for future political decisions.

The reforms led to a rapid increase in private-sector schools over the following 20 years. In 1985, there were 2,643 private voucher schools; in 2002, 3,640; in 2007, 5,054.

The system was very effective in terms of coverage. Secondary school enrolment increased from 65 percent in the early 1980s to nearly 90 percent by 2003. Graduation rates also sharply increased and secondary school dropout rates declined. But there was still a substantial gap between the average test scores of Chilean students and those from other countries in international learning assessments.

This study analyses the performance of children at SIP primary schools (a network of Chilean private voucher schools called *Sociedad de Instrucción Primaria*). This non-profit organisation had served low-income students in Santiago since 1856. Out of 17 schools, 15 were primary, and there was an overall total of 18,000 students. The schools were also known as Matte schools, in honour of the founding family.

The schools were under a central management, which set out aims and mission and left each school autonomy to attain the required goal in its own way. The Central Pedagogical Department played a key role – it defined standards, goals and expected progress, monitored the performance of each school and evaluated and measured student achievement. It organised remedial measures if the goals were not met. The Family Orientation Unit ensured the participation of families in the education process. The schools were mainly financed by state vouchers – about 80 percent of the revenue. Twenty percent of the revenue came from tuition fees and donations for projects.

[5] The structured abstracts include material excerpted from the reports.

SIP students were similar to public school students, but more likely to have a computer at home and own more books – this gap might capture differences in the motivation of parents. SIP teachers had more experience.

SIP students stood out because of their consistently superior performance in the SIMCE test; e.g., in 2002, SIP's 4th graders had outstanding performance in mathematics and language compared to children in all other types of schools. Despite disadvantaged family backgrounds, and having fewer financial resources than similar schools, SIP students outperform public and private voucher school students, and did as well as those at private non-voucher schools typically serving the most elite families.

This study aimed to shed light on the factors that contributed to better education at SIP schools and to identify school and classroom processes that might explain these good results. It looked into qualitative differences that might explain the higher achievement of SIP students.

Type/description of voucher programme

A per-student voucher was paid directly to the school by the central government.

There were three types of schools:

- Private non-voucher schools, financed by tuition fees: 8.5 percent of total enrolment in Chile and 12.7 percent total enrolment in the Metropolitan Region of Santiago in 2002.
- Private voucher schools were owned and run by the private sector and financed by per-student vouchers; parents might pay monthly fees. They accounted for 37.8 percent of total enrolment in Chile and 47.6 percent of enrolment in Santiago in 2002.
- Public schools, financed by vouchers but owned and managed by municipal authorities. They accounted for 52.1 percent of total enrolment in Chile and 37.6 percent enrolment in Santiago in 2002.

Methodology

1. Propensity score based estimation methods. The estimation methods were based on two main assumptions: (1) unconfoundedness, which implies that participation in the treatment programme was not dependent on the outcome after controlling for differences in observed variables, e.g. socio-economic status; (2) overlap – individuals should have positive probabilities of being observed in both the treatment and control groups.

   Three estimation methods were used:

   - Matching, estimating the effect using as counterfactual the observation with the closest propensity score.
   - Propensity score weighting – this weights observations using propensity score and treatment status to balance the sample between treated and non-treated individuals based on the probability of treatment.
   - Propensity score weighted regression – this enables direct accounting for the correlation between the covariates and the outcomes.

2. A number of interviews with SIP schools and other neighbouring schools were performed.

How was the comparison/control group formed?

Not applicable.

How were units assigned to groups?

Not applicable.

Was there pre-test or baseline data?

No.

What methodological problems were reported?

None were reported.

Research questions

Whether the gap in performance between SIMCE and otherwise similar students is robust in controlling for selection on observables and for the characteristics of schools, students and families.

Setting/participants

See below.

Data collected

- 2002 SIMCE standardised test scores – 4th grade test.
- This was complemented with data from a questionnaire to parents of the tested students that provided information on socio-economic characteristics and educational history.
- The Ministry of Education and Under-secretary of Regional Development data were used to calculate what per-pupil resources were available to each school.

Some database variables were modified to make them compatible with the analyses:

- SIP schools were located in urban areas of the Metropolitan Region of Santiago, so only students living there were analysed.
- The highest level of parental education reported was converted into the corresponding number of years in formal education.
- The range of monthly income reported was replaced with the midpoint of the range and divided by 1,000 to simplify the interpretation of the results.

What kinds of analyses were conducted?

The performance of children at SIP and other schools in the Metropolitan Region of Santiago was compared:

- The study investigated whether the gap in performance between SIMCE and otherwise similar students was robust in controlling for selection on observables and for the characteristics of schools, students and families.
- The treatment effect of attending a SIP school on students' performance was evaluated. A number of propensity score based techniques were used, identifying groups of similar students attending different types of schools to deal with the main challenge of addressing selection bias. In all cases, treatment was defined as having attended a SIP school, and then the outcome of the treatment group students was compared with that of students at municipal, private voucher or private non-voucher schools. So three sets of comparisons were performed across treatment and control groups, with each control group defined by students attending a different type of school. The propensity score was estimated using all observations,

then the observations with a high or low predicted propensity score were discarded to re-estimate the probability that the student attended a SIP school; then the effect of attending a SIP school was estimated. Then the effect of attending a SIP school was estimated relative to a private non-voucher school.

- A series of interviews with principals was conducted at SIP and other similar schools to understand the estimated differences in performance. This identified school and classroom procedures that might explain the reasons underlying the successful performance of SIP students. A qualitative analysis was needed because the variables correlated with high achievement were not easy to measure and were usually not observed by an econometrician:
    - A number of interviews were undertaken with the director of the Pedagogical Department and the principals of each school in the network. Also, principals of schools serving populations with similar observable characteristics within the same neighbourhood were interviewed.
    - Interview guideline were based on previous research about effective schools.
    - The survey questions included: use of specific teaching methodologies, directors' goals and tasks, characteristics of teachers and students, and school practices generally.

How long were participants followed?

They were not followed.

Results: key outcomes reported

- The main result was that schools that successfully serve low-income students do exist. The success of these schools does not depend on better access to resources or selection, but on a number of strategies that, if systematically applied, might improve the performance of students attending low-achievement schools.
- Students attending SIP schools perform much better than similar students at other schools, even after controlling for observable characteristics and after dealing with selection on measured variables. The observed differences in students' performance cannot be fully explained by family socio-economic background, teachers' experience or resources within the school.
- The qualitative analysis suggests that having children's learning as the central and permanent goal, a goal that is shared and drives the community's efforts, is what makes SIP schools special.
- Although the interviews do not pinpoint one key aspect that fully explains the difference, differences were observed in the following:
    - the method of selection of directors and teachers
    - the tasks and autonomy assigned to directors
    - the directors' autonomy to hire and fire teachers
    - having the academic performance of students as the main goal, with an emphasis on the directorial team's role towards learning over administrative tasks
    - a clear and shared methodology

- team work and collaboration between teachers and directors; sharing of best practices
- the presence of an under-director
- the systematic evaluation of teachers and students; actions taken based on this
- incentive pay and recognition of the best teachers
- continuous training of teachers
- strategies and resources devoted to bringing on children who lag behind
- the possibility of exploiting economies of scale, in particular through the Pedagogical Department.

Some low-performing schools implemented a number of the above strategies. This suggests that there are characteristics that also matter but are not fully captured by the interviews; these may include intensity and perseverance in applying these strategies, their coherence and how strongly they are they shared by teachers, staff, parents and children.

Alternatively, it could be argued that the SIP's results are, at least in part, driven by the fact that it is a network, and many of the practices that make it successful are too expensive for stand-alone schools. However, public/municipal schools also constitute a network, with central management at the municipal level that can take advantage of economies of scale, and they had significantly lower academic outcomes than SIP schools.

Degree of threat to validity of conclusions

Low.

What were the major implications for future research?

None were stated.

Practice/policy implications

It could be interesting for public/municipal schools to replicate the SIP model.

Does the scheme specifically attempt to target recipients based on socio-economic status, limit the locality (if it is relative to poverty) of the programme, use any type of means-tested model, or scale voucher awards based on income if it is an unrestricted model?

No.

The effects of generalized school choice on achievement and stratification: evidence from Chile's voucher program

Chang-Tai Hsieh, Miguel Urquiola

Received 14 April 2005; received in revised form 2 November 2005; accepted 8 November 2005, available online 18 January 2006

Journal article: *Journal of Public Economics* 90: 1477–1503 (2006).

Location of intervention

Chile

Background/rationale for the study

Chile provides a unique opportunity to analyse the transition from a centrally controlled public school system to one where all families can freely choose between public and private schools.

As result of the education reform in 1981, a dynamic market was created; more than 1,000 private schools entered the market and the private enrolment rate had increased from 20 to 40 percent by 1988, with greater impacts in larger, more urban and wealthier communities. In about 40 percent of urban communes, the public sector was now a minority player; in extreme cases, it accounted for 20-25 percent of all enrolments. However, public schools were not closed, which may suggest that they did not face strong incentives to compete. Voucher schools existing before 1982 were almost entirely religious, whereas those established after that were mostly for-profit; voucher schools accounted for 84 percent in 1988, and their students were from families with less schooling, lower incomes and lower test scores than pre-1982 schools.

This differential impact was used to measure the effects of unrestricted choice on educational outcomes.

Type/description of voucher programme

In 1981, the government introduced a nationwide school voucher programme with financial incentives for both public and private schools. Public schools were decentralised and run by the municipalities; they continued to be funded centrally but received a voucher for each student attending.

Enrolment losses began to have a direct effect on education budgets. Private schools that did not charge tuition fees received the same voucher – these were 'voucher private schools'. Tuition-charging private schools mostly continued without public funding. The essential features have not changed since then.

Methodology

OLS regressions.

How was the comparison/control group formed?

Not applicable

How were units assigned to groups?

Not applicable.

Was there pre-test or baseline data?

No.

What methodological problems were reported?

None reported.

Research questions

Not stated.

Setting/participants

See below.

Data collected

Panel data were used for about 150 municipalities; changes in educational outcomes were measured at the aggregate market level.

1. Chile has approximately 300 communes, and these were used as proxies for educational markets. They had a median area of about 55 km^2 and an average population of 39,000. In 1988, the average commune had 27 schools, 18 of which were public, 7 private voucher, and 2 tuition charging. Each commune had an autonomous government that manages schools and other public services.
2. Three types of outcome measures were used:
    a. The average mathematics and language test score in each commune, which the PER (Programa de Evaluacion del Rendimiento Escolar) testing programme provided for 1982, and SIMCE (the national standardised test) for later years. Information was provided at school level, and this was aggregated to create weighted averages for each commune. A potential problem was that several rural communes were not covered in the initial year (1982). However, the testing still reached 90 percent of all students, and if the test was administered in a given commune, all schools in the commune participated.
    b. The average repetition rate, defined as the fraction of students who took the same grade at least twice, which was the official measure of repetition. Data were compiled from school-level administrative records collected by the Ministry of Education for 1982 and 1988; this covers all schools, so it is possible to check that results with test scores were not driven by the choice of communes.
    c. The average years of schooling among 10–15-year olds. This captures several dimensions of the educational system's performance, since it reflects factors like age at entry, repetition and dropout patterns. It was compiled from the population census and the Chilean National Household Survey (CASEN) household survey micro-data.
3. Two data sources were used to measure students' socio-economic status:
    a. The Ministry of Education classification of each school into 3-4 categories, based on parents' educational background.
    b. CASEN data, which identify the precise school attended by the children surveyed; this information can be linked to administrative records in order to obtain detailed information on the SES profile of individual schools.

What kinds of analyses were conducted?

There were two issues when measuring the effects of school choice on educational outcomes: (1) how to separate effects that operated through enhanced school *productivity* from those that operated through *sorting*; (2) the need to have an adequate control group or counterfactual.

- For (1), a weighted average of these two effects was approximated by measuring the average change in academic outcomes of *all* students in a given community. This was not a perfect measure, but it netted out the 'direct' effect of changes in each sector's student composition.
- For (2) a number of controls for pre-existing and concurrent trends were introduced and the researchers looked for instrumental variables that affected the extent of private entry, but ideally were uncorrelated with trends in academic outcomes or with any productivity advantage of the private sector. These were not ideal, but by comparing how the estimate changed with these modifications, it was possible to get some sense of the magnitude and direction of the bias in the base estimates.
- To measure the effects of competitive forces unleashed by the voucher programme, the research exploited the fact that it had a greater impact in communities with larger markets and where demand for private schooling seems to have been greater. For example, between 1981 and 1988, private enrolment grew by 11 percent more in urban than in rural communities.
- A battery of controls were introduced for pre-existing and concurrent trends, and a number of pre-programme community characteristics, e.g. initial population, urbanisation rate and degree of inequality, were used as instruments for differential impact of the voucher programme. No evidence was found that choice improved school quality.
- The performance of Chilean students was compared in international tests in science and mathematics (TIMSS). Chile participated in 1970 (the TIMSS precursor, IEA) and in 1999. The comparison showed that despite nearly 20 years of unrestricted school choice, the performance of the median Chilean student had not improved relative to that of the median student in other countries.
- The impact of the voucher programme on four measures of academic achievement was determined: (1) language test scores; (2) mathematics test scores; (3) repetition rates; (4) average years of school of 10-15 year olds. The key independent variable was change in the private enrolment rate.
- The average test scores from PER and SIMCE were used to measure whether average school quality had improved. Since tuition-charging private schools were plausibly unaffected by the voucher programme, these schools were used as controls and the gap between the test scores of elite private schools and those of publicly funded (voucher and public) schools was measured. The evidence provided no indication that vouchers improved the outcomes in the schools they affected. The data showed a large gap in the test scores between the subsidised (voucher and municipal) sector and tuition-charging private schools.

How long were participants followed?

Not followed.

Results: key outcomes reported

- There was no evidence that choice improved average educational outcomes as measured by test scores, repetition rates and years of schooling.
- The voucher programme led to increased sorting; the 'best' public school students left for the private sector.

What accounts for lack of improvement on achievement?

Possibilities:

1. Public schools may not have had significant incentives to compete, and authorities might not have provided enough information for parents to determine a school's quality.
2. Private schools responded to competitive pressures by choosing better students rather than raising productivity; there was plenty of institutional evidence for this.

It is not claimed that vouchers have not produced any gains at all. For example, school choice might improve welfare, if not academic achievement.

The underlying institutions and exact details of the programme implemented are critically important when thinking about the potential impact of school choice, e.g. if the choice programme did not allow private schools to select students, or programmes provided incentives to schools to attract children from low-income groups, this might have resulted in less sorting.

The Chilean evidence provides strong support for the idea that schools do respond to incentives. The key question is, incentives for what?

Degree of threat to validity of conclusions

Low.

What were the major implications for future research?

The important topic is the design of mechanisms to preserve the competitive effects of vouchers, while forcing schools to improve by raising their value added.

Practice/policy implications

None stated.

Does the scheme specifically attempt to target recipients based on socio-economic status, limit the locality (if it is relative to poverty) of the programme, use any type of means-tested model, or scale voucher awards based on income if it is an unrestricted model?

No.

Effects of school reform on education and labour market performance: evidence from Chile's universal voucher system

David Bravo, Sankar Mukhopadhyay, Petra E. Todd

Journal article: *Quantitative Economics* 1: 47-95 (2010).

Location of intervention

Chile

Background/rationale for the study

School vouchers were adopted nationwide in 1981. Increased competition among schools was expected to stimulate improvements in the quality of instruction. The control of public schools was transferred to municipal authorities and the school funding system was converted to a per-capita voucher system, with public and private schools receiving the same voucher amounts. A greatly increased level of support went to private schools. In the first five years after it was introduced, the percentage of students enrolled in private subsidised schools increased from 15 percent to over 30 percent, with a corresponding decline in public school enrolment. Subsequently, this change was more gradual. The market share of private non-subsidised schools varied little, from 5.5 to 9.5 percent.

There were three main types of schools: municipal, private subsidised (for-profit or not-for-profit) and private non-subsidised (fee-paying; religious, mainly Catholic; and lay, usually for profit).

Municipal and private subsidised schools were financed mainly through vouchers until 1994, when private schools and municipal high schools were allowed to charge small add-on tuition fees. Private non-subsidised schools were financed from tuition fees. Parents could choose from all three types.

Private schools could be selective but public schools could only be selective if there was excess demand. In all types, students were required to take standardised tests (SIMCE tests) in the 4th, 8th and 10th grades. The average test results were published annually and parents could compare performance.

The previous literature on voucher reforms had focused on test score impacts using test score data collected only after the reforms were introduced. This study looked at the longer-term effects of school voucher reforms on schooling attainment, employment and earnings over the life cycle. It used data on people educated before, during and after the voucher reforms, so it could capture reform-related changes in both public and private schools.

Type/description of voucher programme

Vouchers were publicly funded and voucher funds followed the child to the school selected. Private subsidised schools had to accept the amount of the voucher as full payment for tuition. The voucher amount decreased in real terms until 1990, when it increased.

Methodology

OLS

A dynamic behavioural model of school attendance and work decisions was developed and estimated that incorporated multiple channels through which voucher reforms could operate. The model built on the labour economics literature, specifically dynamic sector selection and human capital pricing equations, which analysed labour market outcomes in the presence of self-selection into educational and/or occupational sectors. The model also explicitly

controlled for both observed and unobserved sources of heterogeneity that might affect selection into different types of schools as well as earnings offers and preference parameters. The framework imbedded the human capital pricing equation within the dynamic education and labour force selection model.

This model was then used to evaluate how the introduction of vouchers affected school choice, educational attainment, earnings and labour market participation for the subgroup exposed to vouchers. It simulated schooling and work choices over the life cycle, with pre- and post-reform estimated model parameters; this enabled direct assessment of the effects of the reform as it operated through multiple channels over the life cycle.

How was the comparison/control group formed?

Not applicable.

How were units assigned to groups?

Not applicable.

Was there pre-test or baseline data?

No.

What methodological problems were reported?

None were reported.

Research questions

None were stated.

Setting/participants

See below.

Data collected

Longitudinal data from household surveys was used, from the 2002 and 2004 rounds of the Encuesta de Proteccion Social survey (EPS), collected by the Microdata Center at the University of Chile. The 2002 survey was called Historia Laboral y Seguridad Social (HLLS).

Both data sets contain demographic and labour market information on 17,246 people aged 15+, including information on household characteristics, education, training and work history, pension plan participation and bank account savings, and more limited information on health, durable assets, disability status and utilisation of medical services. Particularly relevant information was schools attended, family background, earnings and 25 years' retrospective work history, based on questions about labour force and education/training participation, educational attainment, family background, type of primary and secondary school attended and geographic region of schools attended.

The analysis sample consisted of 3,910 males, who were at most 21 years old in 1981 and for whom the research observed educational attainment and their entire work history. A total of 107,394 person-year observations were made on these individuals. The sample includes those who attended school prior to the reform, were in middle of schooling at the time of the reform, and who attended solely after the introduction of vouchers. It could therefore exploit variation in exposure.

The sampling frame of the 2002 HLSS survey consisted of those enrolled in the social security system for at least one month during 1981–2001. This included people who in 2002 were working, unemployed, out of the labour force, receiving pensions, or deceased (in which case information was collected from surviving

relatives). The sample was drawn from a sampling frame of approximately 8.1 million current and former affiliates compiled from official databases (covering approximately 75 percent of population).

The sampling frame for the EPS 2004 survey was augmented to include those not affiliated with the social security system, so the sample is representative of the entire Chilean population aged 15 or more. People who were interviewed in 2004 but not in 2002 were asked questions about both 2002 and 2004.

What kinds of analyses were conducted?

An estimated model was used to assess how the school voucher reform influenced sorting among different types of schools, educational attainment, earnings and labour market participation. Decisions over the life cycle were simulated with and without reform to directly evaluate the cumulative effects of reform as it operated through both schooling and labour market channels.

The net effects of the voucher reform were studied by simulating the behaviour of individuals with and without reform, taking into account multiple channels through which the reforms potentially operated.

How long were participants followed?

Not applicable.

Results: key outcomes reported

- Earnings returns to municipal and private subsidised primary schooling increased in the post-voucher period, consistent with improvements in the quality of primary schooling.
- Secondary-level returns to schooling fell relative to pre-voucher levels, probably reflecting the fact that newer schools entering the secondary school market after reform were not as high quality as more established schools and that per-pupil expenditure declined in the decade after the introduction of vouchers, particularly in secondary schools.
- Model estimates suggest that there were substantial declines in the costs of attending school in regions outside of Santiago in the post-reform period.
- The combined effects of (1) decreased costs of attending school, (2) tuition vouchers and (3) changes in returns to schooling induced higher school attendance rates, with a larger proportion of students attending private schools.
- Overall, model simulations found large effects of voucher reform on schooling attainment:
    - Exposure to voucher reform over the entire schooling career increased primary school graduation rates by 0.6 percentage points, high school graduation rates by 3.6 percentage points, college attendance rates by 3.1 percentage points, and 4-year college completion rate by 1.8 percentage points.
    - The reform reduced labour force participation at ages 16–25 by about 2 percentage points, off a baseline of 58.3 percent.
- The reform did not lead to increased overall average earnings, because the earnings benefits of having greater educational attainment were partly offset by the delay in entering the workforce and a post-reform decrease in returns to secondary schooling. However, examination of earnings distribution shows that earnings increased at lower percentiles of

distribution and decreased at upper percentiles, generating a modest reduction in earnings inequality.

- The impact of voucher reform was similar in magnitude for individuals from both poor and non-poor backgrounds, alleviating concerns that voucher reforms only benefited children from wealthier families.
- Voucher reform effects on discounted lifetime utility indicate a substantial average increase of around 10 percent.

Degree of threat to validity of conclusions

Low.

What were the major implications for future research?

Not stated.

Practice/policy implications

Not stated.

Does the scheme specifically attempt to target recipients based on socio-economic status, limit the locality (if it is relative to poverty) of the programme, use any type of means-tested model, or scale voucher awards based on income if it is an unrestricted model?

No.

Voucher-school competition, incentives, and outcomes: evidence from Chile

Francisco A. Gallego, Department of Economics, MIT

Paper

Location of intervention

Chile

Background/rationale for the study

Parents decide between public schools and voucher schools. Public schools have no direct incentive to produce quality beyond meeting a minimum enrolment level. Voucher schools face competitive incentives.

This study contributes to previous literature on the effects of voucher school entry on school quality. Previous research was affected by endogeneity problems. This paper provides new estimates, using a potentially valid source of exogenous variation for voucher school entry.

This paper describes a study of the effects of inter-school competition on the academic outcomes of students who attended publicly subsidised schools, and presents additional evidence of positive effects of voucher school competition in the early 2000s. It presents a theoretical model allowing for the study of the mechanism through which competition may affect the behaviour of public schools.

It argues that the interaction of the variation in the number of priests per capita in 1950 and the institution of the voucher system in 1981 enables identification of the effects of voucher school competition on test scores. It documents that the number of Catholic priests was not correlated with educational outcomes in the pre-voucher period and was correlated with them after 1981. This means that it is possible to use the number of priests per person in 1950 as an instrument for voucher school entry during the voucher period.

Type/description of voucher programme

The 1981 reform transferred public education from central to local governments (municipalities), and voucher schools and public schools received per-student vouchers as a subsidy for funding. Parents could choose between any publicly-financed school and new schools could enter the market.

There were now three types of schools: publicly owned (managed by municipalities), voucher schools (privately owned, non-profits and for-profits) and non-voucher schools (receiving no public funds, and serving upper-income students).

The voucher schools served over 40 percent of all students. However, enrolment varied widely across areas.

Methodology

OLS, IV

How was the comparison/control group formed?

Not applicable.

How were units assigned to groups?

Not applicable.

Was there pre-test or baseline data?

No.

What methodological problems were reported?

No additional valid instruments were available to study the causal effects of peer effects on test scores, but the results suggest that peer effects were not driving the main results.

Research questions

Not stated.

Setting/participants

See below.

Data collected

1. The 2002 SIMCE standardised test administered to 4th graders – the average of the mathematics and Spanish portions of the test (standardised to have an average of 0 and a standard deviation of 1) as a measure of academic outcomes. Income per household member and mother's education were used to measure the socio-economic background of the students, as well as data on students' educational outcomes and backgrounds, parental preferences, school characteristics and the characteristics of the area where the school was situated.

   The test had been given nationwide since 1988 to 90+ percent of students, in a different grade each year (4th, 8th or 10th graders).

2. The CASEN 2000 household survey collected information on socio-economic variables for a representative cross-section of the population. High school graduation was used as a dummy as a measure of educational attainment for members of different cohorts that attended school in different places.

3. The 2002 Social Protection Survey ('Labour History and Social Security'), collected lifetime information on a sample of individuals. It provided information on high school graduation rates at market level for individuals attending school before the 1981 reform, migration decisions of parents with school-age children in 2002, and information on the type of school attended (public, subsidised private and paid private).

4. The degree of voucher school competition was measured as the ratio of voucher schools to public schools in each educational market. The 297 municipalities and the Metropolitan Area of Santiago were used as proxies for local educational markets. Municipalities were considered to be separate educational markets because, with the exception of municipalities in the Metropolitan Area of Santiago, most students attended schools in town where they lived. Data on the availability of schools in each market came from Ministry of Education files.

5. Data on religious variables at the diocese level were obtained from the Vatican yearly publication *Annuario Pontificio* (number of priests, share of Catholics, and ratio of order to total priests in each Chilean diocese).

6. For some empirical exercises, data on Catholic schools were obtained from the school directory of the Chilean Catholic Church; data on municipal variables, e.g. expenditures per student and size of public schools, came from the Chilean Municipal Dataset.

7. Information on electoral outcomes at municipality level was obtained from the Chilean Electoral Office, when analysing the interaction effects of inter-school competition.

What kinds of analyses were conducted?

1. A Hotelling-type model, in which parents have heterogeneous preferences for different schools, was used to analyse the effects on student outcomes of having two types of schools in the market – public and voucher. It incorporated three groups of agents: parents deciding among different schools, voucher school owners and public school agents.

   The model predicted positive effects of voucher school entry on the quality offered by both voucher and public schools (level effects), and predicted that the size of the response of public schools to voucher entry would depend on the minimum enrolment level needed by the public school to operate and on the size of the school-age population (interaction effects). This suggested that voucher school competition might put stronger pressure to improve quality on public schools than on voucher schools.

   Prediction: public and voucher school quality should increase as the number of voucher schools in a market increases exogenously.

   Prediction of interaction effects: the public school response to exogenous changes in voucher school competition would depend on how binding the minimum enrolment was. This was tested against the data using proxies for: (1) size of education deficit as a percentage of education revenues (proxy for non-voucher transfers); and (2) average size of public schools in different municipalities. Using these proxies, the research studied whether differences in them affected the response of public schools to voucher school competition. A short-lived 1999 change in electoral law was used that enabled identification of a short-run variation in deficits and average school size and therefore made it possible to control for potential selection bias in the estimates. Difference-in-difference regression was implemented to study the effects of the 1999 law on the two proxies for degree of bindingness of minimum enrolment level in the context of the selection model. Overall, the results showed that the proxies for the bindingness of the minimum enrolment level affected the degree of response of public schools to voucher school competition and supported the existence of heterogeneous effects of voucher school competition on public schools.

2. The research exploited the interaction of the number of Catholic priests per person in 1950 in different areas of Chile with the establishment of the voucher system in 1981 as a potentially exogenous determinant of voucher school competition in different markets.

   Identification strategy:

   - The interaction of the log of the number of Catholic priests per capita in 1950 and after the 1981 reform was exploited to identify exogenous variation in the number of voucher schools in an area, after controlling for the share of Catholic population. The reasons for this were: (1) there were direct effects of priests on Catholic schools – there was a big increase in enrolment in Catholic voucher schools after the reform, reaching almost 14 percent of the school-age population in 2002. Priests were important actors in Catholic schools. Schools might be owned by the Church or religious orders, or supported by the Church, but they always had at least one priest acting as chaplain. Priests working in schools were paid comparably to teachers. 5 percent of teaching staff and 10 percent of non-teaching staff in Catholic voucher schools were brothers and nuns; (2) Priests might have an effect on the establishment of non-Catholic schools. The formal definition of a Catholic school was restrictive, so may be some schools were

informally related to the Church; some non-Catholic voucher schools might have been established by former teachers of Catholic schools or priests/nuns retired from orders. Also, non-Catholic vouchers schools tried to mimic Catholic schools in the same area. The propensity of parents to send children to private schools might be affected by the presence of old Catholic schools and priests in the same area.

- The ratio of order priests to the total number of priests was used as a proxy for the presence of orders in different dioceses. Priests per capita in 1950 was used as the main instrument for voucher school entry in different areas during the voucher period, and ratio of order-to-total priests was used as an alternative instrument in some regressions.

The results showed that the number of priests was historically determined and varied widely across dioceses, mainly because there were more religious orders in some areas than others. The average diocese had about 0.15 priests per 1,000 people; the highest ratio was 0.23 per 1,000 people (more than in most Latin American countries). The correlation between the number of priests per capita during the 1990s and the 1950s was 0.78. This showed that number of priests per capita had little effect on educational outcomes before 1981 and had a positive effect after the voucher system was established. So the validity of the identification strategy depended on the assumption that Catholic priests were present in the pre-voucher period, but their effects on educational outcomes only became evident during the period when the voucher system was established.

3. The level of effects of the ratio of voucher-to-public schools on test scores in an educational market was estimated for a cross-section of students in 2002. This sample made it possible to test the predictions of the theoretical model using data for the post-reform period. The number of priests in different areas in 1950 was used as a potentially valid source of exogenous variation in the supply of voucher schools during the post-reform period.

Regressions were undertaken using information on test scores. The advantages of this were: (1) there was detailed information on the degree of voucher school competition in the educational market where the student attended school; (2) there was a more direct measure of test scores, which enabled more precise estimation of the effect of voucher school competition on test scores; (3) it was possible to study whether the interaction effects predicted by the model were supported by the data.

The Heckman selection model with endogenous variables was implemented to control for potential selection bias if the students included in the regressions were not randomly selected from the population. To implement this, a dummy was used in the selection equation that took a value of one if teaching of values was among the top three criteria used by parents for choosing schools. This was a proxy for the location of parents in linear city.

The effects of voucher school competition on expenditures, productivity and student composition were studied at school level to determine whether competition increased test scores by increasing expenditure on education. The way competition affected co-payment levels in voucher schools was also studied. The results implied positive and significant effects on municipal, total public and total expenditures per student, and that voucher school competition increased productivity of schools.

The way competition affected the composition of students within schools was studied, using an index of the distance of each student to the average student in

his/her school, two measures of socio-economic characteristics (mother's education and per capita income) and test scores as measures of student characteristics. The results relating to the effect of competition on test scores were not conclusive.

How long were participants followed?

Not followed.

Results: key outcomes reported

- Once the ratio of voucher-to-public schools in an area was instrumented for, one additional voucher school per public school increased test scores by about 0.14 standard deviations. The magnitude of this effect on test scores was equivalent to about half the effect of increasing the mother's attainment from primary to secondary education.
- These results were roughly similar for students attending public schools and those attending non-Catholic voucher schools.
- Estimates of the effects of school competition on test scores were smaller for students attending public schools that experienced less binding minimum enrolment. While agents operating voucher schools received higher payoffs if they increased enrolment, agents operating public schools received fixed wages and only had to meet a minimum enrolment constraint. Therefore, agents operating in areas where the minimum enrolment constraint was less binding reacted less to voucher school competition.

Overall, the evidence is consistent with a theoretical rationale that emphasised the role of incentives provided by voucher school competition.

Degree of threat to validity of conclusions

Low.

What were the major implications for future research?

None were stated.

Practice/policy implications

Controlling for the characteristics of students and markets, there were sizeable direct effects of competition on test scores. More than 20 percent of the educational markets in Chile had no voucher school in operation and there were heterogeneous effects of voucher school competition for public school students, depending on how binding minimum enrolment constraints were. Thus, the voucher system does increase educational inequality in Chile.

However, the paradox is that the Chilean system doesn't become more unequal because of the existence of voucher schools, but rather because of the absence of voucher schools in some areas, and the absence of strong incentives for some public school agents. The government could correct this inequality while preserving school choice by using the right incentives, such as letting per-student subsidies depend upon student characteristics or by creating explicit incentives that relate the welfare of public school agents to student outcomes.

Does the scheme specifically attempt to target recipients based on socio-economic status, limit the locality (if it is relative to poverty) of the programme, use any type of means-tested model, or scale voucher awards based on income if it is an unrestricted model?

No.

Vouchers for private schooling in Columbia: evidence from a randomized natural experiment

Joshua Angrist, Eric Bettinger, Erik Bloom, Elizabeth King, Michael Kremer

Journal article: *American Economic Review* 92(5): 1535-1558 (2002).

Location of intervention

Colombia: Bogota and Jamundi, a suburb of Cali.

Background/rationale for the study

Private school enrolment as proportion of total enrolment is 2-3 times higher in developing countries than in industrialised nations. In the former, problems with public schools are usually more severe. The view in developing countries that private schools function better than public schools has prompted calls for governments in poor countries to experiment with demand-side financing programmes such as vouchers.

This study presented evidence on the impact of the PACES programme (Programa de Ampliacion de Cobertura de la Educacion Secundaria), established by the Columbian government in late 1991. Partly funded by World Bank, this was one of the largest school voucher programmes at the time the study was completed, providing over 125,000 pupils with vouchers covering a little over half the cost of private secondary school.

Part of a wider decentralisation effort and an attempt to expand private provision of public services, the programme aimed to quickly expand school capacity and raise secondary school enrolment rates. In 1993, although 89 percent of primary-school age children were enrolled in school, only 75 percent of the eligible population was enrolled in secondary schools (US grades 6-11). Among children of eligible age in the poorest quintile of the population, 78 percent were enrolled in primary school, whereas 55 percent were enrolled in secondary school.

Type/description of voucher programme

- The programme targeted low-income families by only offering vouchers to children living in neighbourhoods classified as falling into the two lowest socio-economic strata (out of six possible strata).
- Targeting was enhanced by restricting vouchers to children who attended public primary schools.
- To qualify for a voucher, applicants must have been entering the Columbian secondary school cycle and be aged 15 or under. Prior to applying, students must have already been admitted to a participating secondary school.
- Participating schools had to be located in participating towns, which included all of Colombia's largest cities. In 1993, just under half of private schools in the ten largest cities accepted vouchers.
- Voucher recipients were eligible for automatic renewal through to the end of high school, provided the recipient's academic performance warranted promotion to the next grade. Students failing a grade were supposed to be dropped from the programme.
    - The data showed that an average of approximately 77 percent of recipients renewed their vouchers. In comparison, the national high-school promotion rate to the next grade was approximately 70 percent.

- Students transferring from one participating private school to another could, in principle, transfer the voucher to the new school. In practice, however, the data suggest that many students who transferred after winning vouchers lost their vouchers.

  Participating private schools tended to serve lower-income pupils and to have lower tuition fees than non-participating private schools. They included for-profit schools, religious-affiliated schools, schools run by charitable foundations, and vocational schools, the latter being overrepresented. After 1996, for-profit schools were excluded, largely due to reported problems with low-quality for-profit schools being created to exploit vouchers. Many private schools in Columbia serving low-income populations appeared to have welcomed the programme. Relatively elite private schools opted out of the programme for various reasons.

- The maximum voucher value was initially set to correspond to the average tuition fees of low- to middle-cost private schools in the three largest cities.
- Schools charging less than the vouchers' face value received only their usual tuition fees.
- Vouchers were worth approximately US$190 at the time of the survey.
    - Data from the study showed that matriculation and monthly fees for private schools attended by voucher applicants in 1998 averaged about $340. Most voucher recipients therefore supplemented the voucher with private funds. In addition, since vouchers did not keep up with inflation, recipients had to make additional payments to cover school fees. In comparison, average annual per-pupil public expenditure in Colombia's public secondary school system in 1995 was just over $350, and public-school parents in the survey sample typically paid tuition or fees of roughly $58.
- The municipal governments paid 20 percent of the voucher cost; the central government paid 80 percent.
- Each municipality decided how many vouchers to fund, subject to a maximum allocated to towns by the central government. Allocation was determined by estimating the shortfall between primary-school enrolment and available space in public secondary schools. Voucher award rates therefore varied considerably by city and year, depending on the ratio of applicants to available vouchers.
- Cities and towns used lotteries to allocate vouchers when demand exceeded supply.

Methodology

The study compared educational and other outcomes of lottery winners and losers, using a quasi-experimental research design. Estimates of lottery effects were based on a regression model. Subject to a variety of caveats, the estimates which resulted from the research design provided evidence on programme effects similar to those arising from a randomised trial. This appears to be the first study of a private-school voucher programme in a developing country to take advantage of randomly assigned treatment.

The sampling process began with lists showing applicants' ID numbers, names, addresses and phone numbers, separately for lottery winners and losers. To obtain demographic characteristics for all applicants, whether surveyed or not,

researchers coded sex from first names (for about 80 percent of applicants) and imputed age using ID numbers. Observations in which the applicant was younger than 9 or older than 25 were excluded. A dummy was used for whether the applicant reported a phone number.

How was the comparison/control group formed?

Not applicable.

How were units assigned to groups?

Not applicable.

Was there pre-test or baseline data?

No.

What methodological problems were reported?

None were reported

Research questions

No information provided.

Setting/participants

See below.

Data collected

Three applicant cohorts were surveyed: the 1995 and 1997 applicant cohorts from Bogota and the 1993 applicant cohort from Jamundi, a suburb of Cali. These years and cities were chosen for a combination of scientific and practical reasons. The survey team was based in Bogota, where the largest and longest-running voucher programme took place. Cali was important as Colombia's second-largest city. A suburb was chosen because almost no Cali applicants reported phone numbers.

The researchers tried to interview almost 3,000 of the 6,156 applicants in the three applicant cohorts. There was an overall response rate of 54 percent, and a response rate of almost 61 percent for the 1997 Bogota lottery. Beginning in summer 1998, approximately 1,600 PACES applicants were interviewed, stratifying to obtain approximately equal numbers of winners and losers. The typical applicant was about 13 years old at the time of application, while the average age on the survey date varied from 13 for 1997 applicants to 17 for 1993 applicants. About half of the applicants were male. Roughly 85 percent were still in school, enrolled in grades ranging from 6th for the 1997 cohort to the 8th or 9th for the 1993 cohort. Telephones were used for the majority of interviews, mainly to reduce costs, but also because of interviewer safety and logistical considerations.

What kinds of analyses were conducted?

Various analyses were conducted. The effect of winning the lottery on private-school scholarship receipt and the choice between public and private school were analysed, resulting in the conclusion that the most immediate effect of the lottery was to increase the likelihood of receiving a private-school place. It was also concluded that lottery winners completed more schooling than lottery losers, and were also less likely to repeat grades.

Pupils who failed a grade were supposed to forfeit vouchers. Private schools might therefore have had an incentive to promote pupils who had vouchers even if their performance did not meet promotional standards. To explore this possibility, the researchers looked at the effects on test scores and non-educational outcomes. Children from the 1995 applicant cohort in three Bogota neighbourhoods were

tested; they were chosen because they had relatively large numbers of winners and losers and because of the availability of suitable and safe testing sites. Tests were administered in 1999, approximately one year after the research was conducted and three years after children applied for the programme. Comparing the test scores of winners and losers who were promoted provided evidence that grade-repetition results were not due solely to schools lowering the bar for the promotion of winners.

The effects of winning the lottery were analysed for non-educational outcomes. Approximately 1.6 percent of lottery losers from Bogota were either married or living with a companion. This suggests that marriage and cohabitation were reduced for lottery winners, albeit with a marginally significant effect. There was some evidence from the pooled sample that lottery winners were less likely to be working than losers, the largest effects being in Bogota. There were also significant differences in the number of hours worked. In particular, lottery winners worked 1.2 fewer hours per week than losers, this effect being larger and more precisely estimated for girls. This reduction in work may have been due to the effect of winning the lottery on the household, the greater time demands of private schools relative to public schools, or increased incentives for lottery winners to spend time studying so as to avoid failing a grade and losing their voucher.

The impact of the programme on household and government budgets was analysed. It appeared that winning the lottery induced households to devote more net resources to education. It also suggested that it cost the government about $24 more per lottery winner to provide school places through PACES than through the public system.

How long were participants followed?

3 years.

Results: key outcomes reported

- It was concluded from the assessment of the impact of the programme on household, school and government budgets that the total social costs of providing additional school places through the PACES voucher system were small, and therefore dwarfed by the benefits of the programme to participants.
- There were no significant differences between lottery winners and losers in enrolment three years after application, with most pupils in both winner and loser groups still in school.
- Lottery winners were 15 percentage points more likely to attend private schools rather than public schools.
- Lottery winners had completed an additional 0.1 years of school and were about 10 percentage points more likely than losers to have completed eighth grade, mainly because they were less likely to repeat grades.
- Achievement tests administered to a subset of the pupils surveyed suggested that, on average, lottery winners scored about 0.2 standard deviations higher than lottery losers.

Conclusions

1. Lottery winners benefited from increased educational attainment, mainly as a consequence of reduced grade repetition, as well as academic achievement reflected in higher test scores.

2. Most results suggested that PACES vouchers had a stronger effect on the education of girls than of boys.
3. There was some evidence that the voucher programme affected non-educational outcomes. Lottery winners were less likely to be married or cohabiting and worked about 1.2 fewer hours per week than lottery losers; there was more of a difference for girls than for boys. Both results suggested an increased focus on schooling among lottery winners.
4. Benefits to participants were likely to have exceeded the $24 per winner additional cost to the government of supplying vouchers instead of public-school places.

Degree of threat to validity of conclusions

Low

What were the major implications for future research?

The net effect of vouchers is that their benefit is more than enough to offset the costs. The researchers are assessing the longer-term consequences of receiving vouchers in work in progress.

Practice/policy implications

It appears that demand-side programmes like PACES could be a cost-effective way to increase educational attainment and academic achievement in countries like Columbia which have a weak public-school infrastructure alongside a well-developed private-education sector.

Does the scheme specifically attempt to target recipients based on socio-economic status, limit the locality (if it is relative to poverty) of the programme, use any type of means-tested model, or scale voucher awards based on income if it is an unrestricted model?

PACES targets children living in neighbourhoods classified as falling into the two lowest socio-economic strata (out of six possible strata) and attending public primary schools.

Can private school subsidies increase enrolment for the poor? The Quetta Urban Fellowship Program

Jooseop Kim, Harold Alderman, Peter F. Orazem

1999

Working paper, World Bank

Location of intervention

Pakistan, Quetta (capital city of Balochistan Province).

Background/rationale for the study

Primary school enrolment rates were lower than other countries on the same level of economic development, including Bangladesh, India and Nepal. The national gross enrolment rate was 58 percent: 69 percent for boys and 42 percent for girls. In Balochistan, 62 percent of boys and only 29 percent of girls were enrolled.

The government target was to achieve universal primary enrolment by 2006.

Low enrolment and achievement were partly due to supply constraints, especially in rural areas and poor urban neighbourhoods; also because public budgets were inadequate and the government generally constructed, rather than rented schools. Recipient neighbourhoods needed to provide land for schools and many had squatter communities with poorly defined property rights, so there was limited ability to donate land.

More segregated girls' schools or co-educational schools with female teachers were needed for cultural reasons.

The Balochistan Education Foundation (BEF) launched the Urban Fellowship Program (UFP) in Quetta in 1999 to determine whether establishing private schools in poor neighbourhoods was a cost-effective way to expand primary education for girls in the town's lower-income neighbourhoods. Since about 77 percent girls who started school finished the primary cycle, it was thought that if girls started school, many would persist long enough to attain literacy.

The UFP encouraged private schools, controlled by the community, to establish new schools by paying subsidies directly to the schools – they were assured of government support for three years.

UFP schools could admit boys if they made up less than half of total enrolment, but they had to pay tuition fees, since schools received no subsidy for boys. The class size was limited to fewer than 50 boys and girls, and each class had to have at least one teacher.

The BEF contracted an NGO, SCSPEB (Society for Community Support of Primary Education in Balochistan), to conduct an initial census of each site to make sure there was enough girls in the target age range of 4-8 years, and to inform parents about the programme. The goal was to create a partnership between parents and the school operator. Parent committees selected the school operator from bids or could run the school themselves.

The Balochistan government needed an accurate measure of the programme's success and prognosis for expansion.

This study measured the effect of the UFP on the enrolment of boys and girls in poor neighbourhoods.

The research problem was to find an unbiased estimator of the impact of the Fellowship programme. It was a unique opportunity to apply experimental design methods to evaluate an educational policy innovation.

Enrolment growth in these randomly selected neighbourhoods was compared to enrolment growth in otherwise similar neighbourhoods that were randomly assigned to a control group.

Type/description of voucher programme

A government subsidy was paid directly to schools for three years, though at a reduced level in the second and third years; by the fourth year, schools were expected to be self-sufficient through fees and private support, but eligible for additional grants from the BEF.

Initially about 100 rupees ($3) per month was allocated per girl enrolled, with an upper scholarship limit per month of 100 rupees each for 100 girls. This was sufficient to cover tuition at the lowest-priced private schools. Each school also received 200 rupees per girl to defray start-up costs.

Methodology

Experimental: The randomised implementation of a pilot programme to generate robust estimates of the impact of the programme on enrolment.

Regressions.

How was the comparison/control group formed?

Random assignment.

How were units assigned to groups?

N/A

Was there pre-test or baseline data?

Yes.

What methodological problems were reported?

- Few treatment groups were available because only ten pilot sites were initially funded – one school in each of ten urban slum areas to make sure all the main ethnic groups received at least one school. To accommodate this, the sample included a degree of stratification, under which randomisation was based on neighbourhoods within each slum area.
- The most recent census was 14 years old, and the Quetta population had grown about 7 percent each year since then, mainly within the target population neighbourhoods. Therefore an area frame sampling strategy was chosen to define treatment and control neighbourhoods.

Research questions

To find an unbiased estimator of the impact of the UFP (indirectly stated).

Setting/participants

See below.

Data collected

- All households in the treatment neighbourhoods were surveyed in summer 1994, when the programme was promoted and before any UFP schools were opened. The data included information on household socio-economic characteristics, parents' education, and the educational attainment and

current enrolment status of all children in each home. The treatment sample included 1,310 children: 781 girls and 529 boys.

- A baseline survey of households in the control neighbourhoods was conducted in July 1995 – the difference in timing was not problematic because such data did not change over such a short period and there were no major economic events. The control sample included 1,358 children: 697 girls and 661 boys.
- Information on the enrolment status of children in control neighbourhoods was obtained for 1995 and retrospectively for 1994.
- Enrolment data in both treatment and control neighbourhoods was collected in 1996.
- The Balochistan Education Management Information System supervised all data collection and the training of those administering the surveys, to ensure compatibility of data.

What kinds of analyses were conducted?

The programme's impact was measured using estimators in order to find out if the results were robust. Each estimator was applied in two ways: (1) the change in enrolment for children in the target age of 4-8 was measured; (2) enrolment rates were measured longitudinally for children age 4-7 in the initial year of the programme.

For treatment, the sample had a dummy variable that equalled 1 if the child was enrolled in school as dependent variable. Other variables were exogenous, believed to affect parents' enrolment choices. Most variables came directly from the questionnaire. However, distance to school and annual fees were neighbourhood averages of children in school. Household income was estimated using number of adults in the household, their educational attainment and a set of household assets.

The statistical significance of differences between treatment and control group were tested as follows:

1. Equality of means of endogenous and exogenous variables were tested for to determine if randomisation yielded observationally equivalent treatment and control populations.
2. Enrolment equations were estimated using baseline data.

Because the treatment and control neighbourhoods had different characteristics believed to affect parents' choices, a simple comparison of unconditional means could yield biased estimates of programme effects. Therefore, comparisons were made using regression analysis.

How long were participants followed?

Not followed.

Results: key outcomes reported

- Regardless of how impact was measured, the UFP raised enrolment for boys and girls, around 33 percentage points.
- Most estimates showed larger effects for girls.
- Boys' enrolment rose partly because they were allowed to attend the schools and partly because parents would not send girls to school without also educating boys.

    - This suggested that programmes that targeted girls could also induce parents to invest more in boys.

- The success of the programme varied across neighbourhoods, although this was not clearly related to the relative wealth of the neighbourhood or to parents' level of education.
    - The programme hence offered great promise for increasing enrolment rates in other poor urban areas.

It was concluded that estimated programme effects were robust to differences in assumptions about possible biases arising from measured and unmeasured differences between the treatment and control neighbourhoods.

Degree of threat to validity of conclusions

Low.

What were the major implications for future research?

Future work is required to assess the long-term effects of the UFP, especially on the sustainability of schools and enrolment effects after the subsidies expire. The question of how much children are learning remains. School outcomes must also be assessed – the ultimate success of the UFP depends on whether children achieve literacy.

Practice/policy implications

- Before the project, it was not clear whether girls' low enrolment rates were due to cultural barriers or an inadequate supply of girls' schools. There is strong evidence from the UFP that subsidising primary schools for girls can sharply increase their enrolment. Also, boys' and girls' education are complementary – by encouraging parents to send girls to school, the programme had collateral benefits of raising boys' enrolment rates.
- There is mixed evidence on whether enrolment in the UFP neighbourhoods relative to control neighbourhoods continued to increase. However, even if enrolment decreased later, this was still a substantial improvement over baseline enrolment rate.
- School success seems not to depend on neighbourhood income or other observable socio-economic variables, which suggests that expanding the UFP to other poor neighbourhoods is likely to be successful also.

Does the scheme specifically attempt to target recipients based on socio-economic status, limit the locality (if it is relative to poverty) of the programme, use any type of means-tested model, or scale voucher awards based on income if it is an unrestricted model?

Yes – the locality is limited to very poor neighbourhoods.

Appendix 2.6: Excluded studies

During our search process, the full text of the following studies was downloaded and reviewed during a second screening to determine whether the studies were eligible to be included in the review. Each of the studies was ultimately excluded for the reasons noted below.

Study	**Voucher Programme**	**Reason for exclusion**		
		No baseline equating of groups	**Compares outcomes for different types of voucher schools, rather than impact of voucher programme**	**Examines behaviour of demanders and suppliers of education, rather than impact of voucher programme**
Aedo (1996)	A	X		X
Anand *et al*. (2006)	A	X	X	
Auguste and Valenzuela (2006)	A	X		
Bellei (2005)	A	X	X	
Bravo *et al*. (1999)	A	X	X	
*Bravo *et al*. (2010)	A	X		
Carnoy and McEwan (1998)	A	X	X	
Coloma (1999)	A	X		X
Contreras (2001)	A	X	X	
Contreras (2002)	A	X	X	
Contreras and Santos (2009)	A	X	X	
Contreras *et al*. (2010)	A	X	X	
CMS social (2009)	C	X		
Elacqua (2006)	A	X	X	X

Study	**Voucher Programme**	**Reason for exclusion**		
		No baseline equating of groups	**Compares outcomes for different types of voucher schools, rather than impact of voucher programme**	**Examines behaviour of demanders and suppliers of education, rather than impact of voucher programme**
Elacqua (2009a)	A	X		
Elacqua (2009b)	A	X		X
Elacqua (2011)	A	X	X	
Elacqua and Fabrega (2004)	A	X		X
Elacqua *et al*. (2011)	A	X	X	
Elacqua *et al*. (2006)	A	X		X
Erisen (2008)	A	X		X
Gallego (2002)	A	X	X	
*Gallego (2004)	A	X	X	
Gallego and Hernando (2009)	A	X		X
Garces (2009)	A	X	X	
*Henriquez *et al*. (2010)	A	X	X	
* Hsieh and Urquiola (2006)	A	X		
King *et al*. (1998)	B			X
Lara *et al*. (2010)	A	X	X	
McEwan (2001)	A	X	X	

Study	**Voucher Programme**	**Reason for exclusion**		
		No baseline equating of groups	**Compares outcomes for different types of voucher schools, rather than impact of voucher programme**	**Examines behaviour of demanders and suppliers of education, rather than impact of voucher programme**
McEwan (2002)	A	X	X	
McEwan and Carnoy (2000)	A	X	X	
Mizala and Romaguera (1998)	A	X	X	
Mizala and Romaguera (2000)	A	X	X	
Mizala and Torche (2012)	A	X	X	
Mizala and Uriquola (2007)	A	X		X
Mizala *et al*. (1998)	A	X	X	
Mizala *et al*. (2004)	A	X	X	
Patrinos and Sakellariou (2008)	A	X		
Rounds Parry (1996)	A	X		X
Rounds Parry (1997)	A	X	X	
Sapelli and Torche (2002)	A	X		X
Sapelli and Vial (2002)	A	X	X	
Sapelli and Vial (2003)	A	X	X	

Study	**Voucher Programme**	**Reason for exclusion**		
		No baseline equating of groups	**Compares outcomes for different types of voucher schools, rather than impact of voucher programme**	**Examines behaviour of demanders and suppliers of education, rather than impact of voucher programme**
Sapelli and Vial (2005)	A	X	X	
Somers *et al*. (2004)	A	X	X	
Tokman Ramos (2002)	A	X	X	
Torche (2005)	A	X	X	
Urquiola and Verhoogen (2007)	A	X		X
Valenzuela (2008)	A	X	X	
Vegas (2002)	A	X	X	

*Underwent final screening and structured abstract completed.

A = Chile nationwide school choice; B = Columbia PACES; C = Delhi voucher project

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