CHAPTER 4

Competition Among Schools: Traditional Public and Private Schools

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Contents

Abstract

This chapter considers research on the effects of competition between private and public schools. It focuses on three questions: (1) Do children experience higher achievement gains in private school? (2) If so, is this because private schools are more productive? (3) Does competition from private schools raise public school productivity and/or otherwise affect those "left behind"? The chapter shows that unless each of these questions is answered, one cannot form a full assessment on the desirability of private school entry. Voucher experiments suggest that question 1 can be answered in the affirmative for some subgroups and in some contexts. Such work cannot typically isolate channels, however, and hence does not address question 2. Question 3 has been primarily studied by papers on large-scale voucher programs. These suggest that private school entry results in nonrandom sorting of students, but are less clear on the effects. The bottom line is that despite demand for clear, simple conclusions on the effects of competition from private schools, research does not yet provide these.

1. INTRODUCTION

There is a long-standing perception among economists (eg, Smith, 1776; Friedman, 1955) that competition can improve school markets' performance. Traditionally, competition has been envisioned to take the form of entry by private schools into markets previously dominated by public schools. The conjecture is that such entry can enhance the accumulation of skill, or at least reduce the cost of its production.

It is not surprising that this seems plausible to many observers. After all, in some settings the counterfactual to a larger private sector is a public sector in which it at least casually appears difficult to terminate transparently underperforming teachers, reduce rampant absenteeism, or introduce meaningful curricular experimentation.

This chapter provides an overview of what economic research has revealed — and what knowledge gaps remain — on the effects of competition between "traditional" public and private schools. The exact meaning of this label will depend on the setting, but it leaves aside, for example, somewhat more recent institutional forms, such as charter schools in the USA.[1]

The chapter organizes the issues by focusing on three sets of questions:
1. Do children experience higher achievement gains in private than in public schools?
2. If private schools raise achievement more, is this because these schools are more productive? (We shall understand a given school to be more productive than another if it produces more skill given the same resources.)[2]
3. Does competition from private schools raise public school productivity? Does the existence of private schools otherwise affect those "left behind" in public schools?

The chapter first sets out a simple framework to show that unless *each* of these questions can be answered, one cannot form a full assessment on the desirability of greater competition from private schools. The discussion emphasizes three further points. First, economic theory suggests that none of these questions has an answer that is clear a priori; the effect of competition on school market performance is therefore an empirical question. Second, question 1 can be credibly answered if one has exogenous variation in private school attendance as provided, for example, by many voucher experiments. Third, such experiments do not typically provide answers to questions 2 and 3; analyses of large-scale voucher reforms are better suited to tackling these, but face significant methodological challenges related to identification and nonrandom sorting of students.

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[1] The evidence on charter schools is covered elsewhere in this volume.
[2] Hoxby (2002b) provides a useful introduction to the importance of school productivity in the analysis of competition between schools. Our definition is different than the one she uses, but in the same spirit.

The chapter then considers the existing evidence. Rather than attempt to provide an exhaustive survey, it focuses on the research and settings that potentially provide the most credible answer to these questions, but which also help frame future avenues for research.[3] Specifically, on question 1 the discussion centers mainly on evidence from the USA, Colombia, and India. On questions 2 and 3, it focuses on evidence from Chile and Sweden.

To preview the findings, the literature has made significant strides in answering question 1. Randomized experiments show that in some settings students acquire greater skill if they attend private rather than public school, although a perhaps surprisingly large proportion of estimates suggest an impact that is statistically indistinguishable from zero.

This type of work has made less progress providing answers to question 2. It does provide some evidence consistent with private schools producing skills at lower financial cost (most instances of this result originate in low-income countries and at the primary level; there is little evidence on whether it generalizes beyond that). However, effectively tackling question 2 requires disentangling all the possible *channels* through which a private school effect operates. This is typically not feasible in the contexts of the randomized work. This has important implications, since the case for expanding the private sector is much stronger if one can make the case that higher achievement in private schools reflects higher productivity. Otherwise it could reflect potentially zero-sum mechanisms related to peer effects, for example.

Addressing question 3 ideally requires exogenous cross-market variation in the extent of private enrollment. Not surprisingly experiments to date have not achieved this. As an alternative, some research has focused on large-scale voucher reforms that have induced substantial expansions in some countries' private sectors, with variation across markets. Such analyses have the advantage of potentially revealing the general equilibrium effects of competition from private schools; but they face challenges related to identification, and to the fact that private entry is frequently associated with nonrandom sorting of students across sectors. A mixed assessment of the effects of competition also emerges from this work. Specifically, while growth in the private sector seems to have resulted in sorting and stratification, its impact on learning appears more mixed. Finally, given the difficulty in isolating effects on the public sector (which answering question 3 requires) some of this work looks at aggregate effects. Here again mixed results emerge. To cite the most aggregate evidence, some countries with large voucher-induced private school growth have seen their relative performance in international tests improve significantly; others have seen it decline. In addition, here again it is difficult to isolate specific channels.

The bottom line is that despite demand for clear, simple conclusions on the effects of competition from private schools, research does not yet provide these. For now,

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[3] For reviews of related literature, see Ladd (2002), Neal (2002), McEwan (2004), Gill et al. (2007), Levin (2008), Barrow and Rouse (2009), and Epple et al. (2015).

the evidence seems more mixed than observers have usually expected. Further, the existing research underlines that educational markets are complex (MacLeod and Urquiola, 2013).

The gaps in knowledge around questions 1–3 suggest pathways for future research. Aside from these, an important area for future work concerns how competition-related policies themselves may be better designed. Indeed, the heterogeneity in results itself suggests that the impact of competition from private schools may depend on how and in what context it is introduced, as has been found, for instance, in the case of school decentralization (Galiani and Schargrodsky, 2002; Hanushek et al., 2013).

The remainder of the chapter proceeds as follows. Section 2 presents a simple framework to organize ideas. Section 3 reviews the evidence, and Section 4 concludes.

2. A SIMPLE FRAMEWORK

Studying the effects of introducing private schools into a public school market raises many analytical issues. This section presents a basic framework to organize some of these. The objective is not to derive estimating equations, but rather to provide — by making simplifying assumptions and abstracting from multiple issues — a closed form illustration of the different mechanisms that questions 1–3 (Section 1) raise. One theme will be that in realistic settings in which relevant variables are unobserved and functional forms are more complicated, it is difficult to get a sense of the direction and/or the magnitude of each mechanism.

Consider a market in which students differ only according to their ability, A. Suppose that the skill of a student i who attends school s is given by

$$T_{is} = \alpha_s + f(A_i) + g(\bar{A}_s). \tag{1}$$

Note that there is a peer effect: a student's outcome is a function not just of her own ability, A_i, but also of the average ability of students at her school, $\bar{A}_s$. This peer effect should be thought of broadly. It could reflect a direct externality in the sense that students learn from more able classmates; alternately, it might reflect that the parents of more able children more effectively discipline school administrators. α_s is the school's productivity (ie, its contribution to skill that is independent of peer ability). As stated, we shall understand a given school to be more productive than another if it produces more skill given the same resources. Thus, by our definition productivity would also be independent of resources other than peer effects, such as spending per pupil, that are not in Eq. (1) — again the point will be that even assuming away such other issues clear answers to questions 1–3 are hard to obtain.

In using Eq. (1) to assess competition between private and public schools, an important step is to specify the functional form of peer effects and the distribution of students across the two sectors. This reflects that in reality, it is likely to be the case that the children who use private schools are not a random sample of the population.

Drawing on Hsieh and Urquiola (2003) and to simplify matters, suppose that initially all students attend a single public school. Further assume that both own ability and peer quality have a linear effect on learning outcomes. Thus skill is given by

$$T_i^0 = \alpha_{\text{pub}} + \beta A_i + \gamma \bar{A}, \tag{2}$$

where α_{pub} is the public school productivity, β indicates the impact of own ability, and γ that of peer ability. $\bar{A}$ is the average ability over all students. The superscript on T indicates this is the initial situation.

To simplify further, suppose that $A_i = i$ (ie, students are indexed by ability) and that i is uniformly distributed in the interval [0,1]. With this, the initial mean achievement is

$$\bar{T}_{i \in [0,1]}^0 = \alpha_{\text{pub}} + \frac{\beta}{2} + \frac{\gamma}{2}, \tag{3}$$

where $\bar{T}_{i \in [0,1]}^0$ denotes the mean skill in the public school when it contains all children, $i \in [0,1]$.

Now suppose that a private school with productivity α_{priv} enters the market. Assume that a simple form of "cream skimming" takes place: all students $i \in [\frac{1}{2}, 1]$ enroll in the private school, while those $i \in [0, \frac{1}{2})$ remain in the public sector. This is a stark assumption; while it simplifies matters, any form of nonrandom sorting will render relevant several issues discussed below.

The average achievement in the private school is

$$\bar{T}_{i \in [\frac{1}{2}, 1]}^{\text{priv}} = \alpha_{\text{priv}} + \frac{3\beta}{4} + \frac{3\gamma}{4}, \tag{4}$$

and the average in the public school is now

$$\bar{T}_{i \in [0, \frac{1}{2})}^{\text{pub}} = \alpha_{\text{pub}}^* + \frac{\beta}{4} + \frac{\gamma}{4}, \tag{5}$$

where the asterisk on α_{pub}^* indicates that the public sector productivity may change in the presence of the private school — that is the public school productivity may no longer be that which prevailed in the initial situation.

The remainder of this section uses the above expressions to discuss challenges in answering questions 1–3.

2.1 Question 1: Do Children Experience Higher Achievement Gains in Private Than in Public Schools?

In our example the children $i \in [\frac{1}{2}, 1]$ switch to private school. For these individuals the change in achievement — the effect of going to the private rather than the public school — is

$$\bar{T}_{i \in [\frac{1}{2}, 1]}^{\text{priv}} - \bar{T}_{i \in [\frac{1}{2}, 1]}^0 = (\alpha_{\text{priv}} - \alpha_{\text{pub}}) + \frac{\gamma}{4}, \tag{6}$$

where $\overline{T}^0_{i\in[\frac{1}{2},1]}$ denotes the mean score they would have had in the original public school. There are two sources of gains for these children: a productivity effect, if $\alpha_{priv} > \alpha_{pub}$, and a peer group effect given by the last right-hand side term. The latter arises because they are now isolated from the lowest ability children.

Suppose one were able to carry out an experiment in which two children of identical ability $i \in [0, \frac{1}{2})$ in the public school were eligible to transfer to the private school. If only one was randomly selected to do so, a comparison of their outcomes would approximate (Eq. 6).[4] In other words, such an experiment would provide a reduced form estimate of the causal impact of switching from a public to a private school.

If this impact is positive, then the first part of question 1 can be answered in the affirmative (ie, children experience greater achievement in private school). Below we shall discuss papers that essentially implement such an analysis.

2.2 Question 2: If Private Schools Raise Achievement More, Is This Because These Schools Are More Productive?

As stated, a randomized experiment can under some assumptions provide a credible approximation to Eq. (6). Note, however, that in general such an estimate does not identify the source of this difference. Specifically, with strong assumptions on aspects such as functional form, Eq. (6) decomposes the gain into a productivity effect ($\alpha_{priv} - \alpha_{pub}$), and a peer group effect, $\frac{\gamma}{4}$. Such a decomposition is hard to achieve empirically. For example, sorting may happen along unobservable characteristics that are therefore impossible to control for. Further, peer effects, to the extent they exist, are unlikely to follow a simple linear-in-means specification like Eq. (2). The literature on peer effects is complex and far from delivering a consensus on a functional form. In fact, the findings in Carrell et al. (2013) suggest that at least in some settings a stable functional form may not exist.[5]

In short, the existing research — even when it can credibly estimate the reduced form impact of attending a private school — does not isolate whether at least part of this effect is due to higher private school productivity.

As Hsieh and Urquiola (2003) point out this has two important consequences. First, to the extent that the gain measured in Eq. (6) is at least partially due to a peer effect, then this gain will not be independent of the size of the private sector or of the sorting its growth induces. For instance, the advantage conferred by transferring to a private school may dissipate as the private sector grows and incorporates weaker children. To illustrate, in our setup the measured private advantage would have been higher if only the children $i \in [\frac{4}{5}, 1]$ had transferred to private school, as the private peer quality would be higher in that case.

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[4] This abstracts from impacts on the public sector productivity, to which we return in discussing question 3.
[5] See Epple and Romano (2011) and Sacerdote (2011) for further reviews on peer effects.

Second, a positive estimate of Eq. (6) does not necessarily imply that average achievement would grow if the private sector is expanded. Given the assumptions we have made here, if a positive difference $\overline{T}^{\text{priv}}_{i \in [\frac{1}{2}, 1]} - \overline{T}^{0}_{i \in [\frac{1}{2}, 1]}$ is only due to a peer effect, then private expansion will be zero sum. More generally, the aggregate effect of private expansion could be positive or negative.

The bottom line is that a credible positive reduced form estimate of Eq. (6) — such as some randomized studies deliver — is sufficient to answer question 1; it is not sufficient to answer question 2, and hence must be treated with caution in assessing the desirability of further private expansion. This does not even address the complications raised by question 3, to which we now turn.

2.3 Question 3: Does Competition From Private Schools Raise Public School Productivity? Does the Existence of Private Schools Otherwise Affect Those "Left Behind" in Public Schools?

As the previous section suggests, the case for expanding the private sector is much stronger if private schools have higher productivity. In addition, welfare effects could arise from private expansion if it induces the public sector to change its own productivity, or if it affects public school children through other channels.

This is difficult to analyze experimentally — it would be hard to implement a scheme that, for example, significantly manipulated private school entry across randomly selected markets. Instead, the literature has focused on large-scale, nationwide voucher reforms that have induced large changes in private enrollment in some markets.

The above setup can be used to think about how private school entry affects public performance in such settings. A first pass measure of this is provided by simply comparing the mean public sector achievement before and after private entry:

$$\overline{T}^{\text{pub}}_{i \in [0, \frac{1}{2})} - \overline{T}^{0}_{i \in [0, 1]} = (\alpha^{*}_{\text{pub}} - \alpha_{\text{pub}}) - \frac{\beta}{4} - \frac{\gamma}{4}. \tag{7}$$

Expression (7) is a first pass partially because it compares different sets of students ($i \in [0, \frac{1}{2})$ and $i \in [0,1]$), but it is useful because this difference is typically readily observed. This expression shows that to answer question 3 one would ideally want to decompose this change into three effects. First, there is the public sector's productivity change $\alpha^{*}_{\text{pub}} - \alpha_{\text{pub}}$. Second, there is a composition effect, $\frac{\beta}{4}$ — the public sector does worse simply because it has lost the most able children. Third, there is a peer effect given by $\frac{\gamma}{4}$. This is the consequence of public school children having lost the ability to interact with higher ability children, and is the mirror image of the peer-related gain for private school students in Eq. (6).

Empirically achieving a decomposition like that in Eq. (7) is difficult. Again, relevant variables might be unobserved, and functional forms may be more complicated than we have assumed here.

A clear prior on the direction of at least some of the components in Eq. (7) could provide analytical leverage to guess the direction of the others. However, theoretical work does not provide a clear prediction on the direction of any of these effects.

First, consider the productivity difference $\alpha^*_{\text{pub}} - \alpha_{\text{pub}}$. One might expect it to be positive, but McMillan (2005) shows that it could be negative, and it is useful to consider the main aspects of his argument. His model features two types of households, low and high income; the latter are willing to pay more for school quality than the former. Schools can influence their productivity by exerting effort. Competition ensures that the private schools provide an efficient level of effort. That is, some private schools serve high-income students and charge high tuition for high effort; others serve low-income households and charge low tuition for low effort. The public school sector can also choose between the two effort levels. If it exerts high effort it attracts both types of students — the high income prefer it in this case to tuition-charging private schools. In contrast, if it exerts low effort, it only gets the low-income students. Now suppose the public sector is initially exerting high effort, and a voucher is introduced. This lowers the cost of private schooling and may lead the private sector to grow as it enrolls more high-income households. If this happens, public schools choose to lower their effort to the level required to retain only low-income students. Hence competition may lower public school productivity.

Second, one might think that the sorting terms in Eq. (7) would be easy to sign, but note that here we have assumed straightforward cream skimming. As discussed below, many voucher programs that expand private schooling are targeted at lower-income children. In addition while in many countries some private schools serve the elite, in many other (particularly developing) countries the private sector also includes low-cost schools that serve low-income households.

The bottom line is that in general it will be hard to determine the sign on $\alpha^*_{\text{pub}} - \alpha_{\text{pub}}$, that is, to establish if competition from private schools forces public schools to become more productive. This matters because productivity gains are in a real sense the whole point of introducing competition.

Furthermore, it is possible for private entry to hurt those "left behind" in public schools even if one assumes that the effects on productivity are positive. The intuition is illustrated by the last term on the right-hand side of Eq. (7): *if* there are peer effects, and *if* private school entry leads to cream skimming, then the overall impact on the public sector could be negative even if there is a positive productivity effect.

It is again useful to discuss briefly some theoretical examples of this, and we cover three. First, Manski (1992) presents a theoretical and computational framework in which students are heterogeneous along their household income and motivation. Manski uses this to assess whether vouchers equalize educational outcomes. His setup features an externality in that students benefit from motivated peers at school. He shows that as the voucher level rises, motivated students tend to transfer to private schools, especially

in poor communities. This renders the effects of vouchers ambiguous, much as in our simple framework. Manski's own conclusion is illustrative: "The immediate lesson is that qualitative analysis cannot determine the merits of alternative school finance policies. Qualitatively plausible arguments can be made both for public school finance and for voucher systems. Hence, informed assessment…requires quantitative analysis…The educational effects of systemic *choice* on low income young people appear to be neither uniformly positive nor negative."

Second, Epple and Romano (1998) consider a broadly similar setup in that students vary according to their income and ability. A key innovation is that schools charge tuition that can be tailored to students according to these two traits. This generates "diagonal" stratification on the income–ability plane. Intuitively, because school quality increases with peer ability, private schools set lower tuition for high-ability students; in addition, higher-income households with low-ability children pay a tuition premium to enable them to attend high-quality schools. The model still features the result that average peer quality in the public schools declines as private entrants "cream skim" higher-income/ higher-ability students from the public schools. Epple and Romano (2008) extend this work by using estimates from Hoxby (2000) in a computational exercise. They assume private schools are more productive than public schools. In this case, as in Eq. (7), the overall effect is still ambiguous.

Third, the ambiguity need not arise from true human capital externalities as in Manski (1992) and Epple and Romano (1998). MacLeod and Urquiola (2009) show that private growth may hurt those left in public schools through informational channels. Specifically, they assume that an individual attends school and accumulates skill as a function of her innate ability, her effort, and her school's value added. The key assumption is that individual innate ability is more readily observed by schools than by employers. In this sense schools perform two services: they supply productivity, but they also provide information. For example, an employer might use the fact that a graduate is from a certain school to make inferences regarding her ability. This is a version of signaling (Spence, 1973) but one that originates in *which* school children attend, as opposed to *whether* they attend. This again produces a setting in which private school growth can hurt those left behind, since their low ability is revealed to employers by their failure to gain admission to a selective private school.[6]

In closing, note that one theme in the discussion surrounding questions 2 and 3 is that it is difficult to decompose the different mechanisms potentially unleashed by competition from private schools. One alternative — particularly when looking at the large-scale reforms used to analyze question 3 — is to simply analyze a single, market-level net effect.

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[6] MacLeod and Urquiola (2009) refer to K–12 schools. MacLeod and Urquiola (2015) extend similar informational mechanisms to the college setting, and MacLeod et al. (2015) present empirical evidence that such mechanisms are operative.

If panel data for multiple local school markets are available then one can take differences over time removing market-specific fixed characteristics. In addition, looking at aggregate outcomes nets out composition effects like the second term in Eq. (7). We shall review such evidence below. Nevertheless, considering aggregate effects is still not ideal because it confounds the impact of productivity improvements and the net effect of peer group composition.

Below we review studies on large-scale voucher reforms that essentially implement such an analysis. It should be noted that two challenges remain. First, it may be difficult to account for factors that give rise to endogenous differential voucher growth across markets. Second, market-specific factors other than the voucher may impact a market over the time period under analysis.

3. THE EVIDENCE

This section reviews the evidence on the three questions considered above.[7] Dealing with these questions sequentially naturally imposes an ordering in terms of the types of evidence considered. Specifically, question 1 has been most credibly analyzed in the context of small-scale experiments involving the distribution of a limited number of targeted vouchers. In contrast, the papers that address questions 2 and 3 generally consider large-scale voucher reforms — namely, situations in which vouchers are distributed to anyone wishing to use them, without income or geographic restrictions.

3.1 Question 1: Do Children Experience Higher Achievement Gains in Private Than in Public Schools?

The most credible evidence on question 1 comes from small-scale voucher programs, as these often (eg, when they involve explicit or implicit randomization) provide a way to control for selection into private school. This makes it more likely that performance differences in public-private comparisons reflect the net effect of causal mechanisms. A large number of small-scale voucher programs potentially offer such evidence. For instance, Epple et al. (2015) count 66 voucher programs in the USA; 9 funded by tax revenues, 7 via tax credits, and 50 by private foundations.

This section begins by reviewing some of the evidence that has emerged from these programs; it then turns to studies set in Colombia and India.

3.1.1 United States

The largest voucher program in the USA is that in Milwaukee, Wisconsin. It began in 1990 with the distribution of about 800 vouchers giving students the opportunity to

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[7] In some cases the review of the US evidence draws on Epple et al. (2015); that on Colombia on MacLeod and Urquiola (2013) and that on India on Urquiola (2015).

attend private school. Eligibility for the vouchers was restricted to students with house-hold incomes below 175% of the federal poverty level. The beneficiaries were selected via lottery, such that the performance of the lottery losers provides a natural and credible counterfactual for that of the winners. Barrow and Rouse (2009) point out that the data collected as part of the program made available an additional comparison group: a random sample of low-income students from the Milwaukee Public Schools.

Using the latter comparison group Witte et al. (1995) find that the program had no statistically significant impact on test scores. Using the former comparison group, Greene et al. (1996) find statistically significant positive impacts in both math and reading. Using both comparison groups, Rouse (1998) finds significant impacts in math but not in read-ing. Recent research on Milwaukee faces greater difficulty exploiting randomization, as growth in the number of vouchers offered reduced the need for lotteries.[8]

Another relevant voucher program is that enacted by the US Congress for Washington, D.C. in 2003. The design is broadly similar to Milwaukee's in that it also features the use of lotteries. Using data on 2000 eligible applicants, Wolfe et al. (2010) and Wolfe et al. (2013) find that winning a voucher had no significant effects on test scores, but a relatively large impact on graduation: the intent to treat effect is about 12% points relative to a base graduation rate of 70%.

The Milwaukee and Washington, D.C. programs are publicly funded. Additional evidence comes from typically smaller, privately funded targeted programs. For example, Peterson et al. (2003) analyze programs with lottery-based voucher distribution in New York, Washington, D.C., and Dayton.[9] Averaging over all three programs they find a positive effect on math and reading scores for African Americans, although none for a group consisting of all other ethnic groups. When analyzed separately, the positive effect for African Americans persists only in New York (see also Howell et al., 2002; Mayer et al., 2002). However, Krueger and Zhu (2004) revisit the New York data and show that this last finding is sensitive to how ethnicity is coded, as well as to how one handles students with missing baseline scores.

Chingos and Peterson (2012) use more recent data to look at college enrollment as an outcome in the New York program. Although they find no effect on aggregate, they again find a statistically significant and substantial positive impact on African Americans.

To summarize, voucher experiments in the USA in principle allow one to identify the causal impact of switching a small number of children from public to private schools. The results often point to an absence of an achievement gain on average. There does appear to be a positive impact on black students, although this appears to be more robust when

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[8] Witte et al. (2012) use a matched sample and report statistically significant effects in reading but none in math.

[9] These are the School Choice Foundation program in New York City, the Washington Scholarship Fund program in Washington, D.C., and the Parents Advancing Choice in Education program in Dayton, OH.

graduation rather than test scores are the outcome. In short, the US-based evidence on question 1 — which as emphasized above must be complemented with evidence on questions 2–3 to get a full sense of the effect of competition — does not provide robust evidence that an expansion of the private sector would significantly raise achievement. This overall conclusion is qualitatively similar to that in two recent reviews of the US evidence by Barrow and Rouse (2009) and Neal (2009).[10] To the extent that our conclusion is slightly more positive regarding the effects on subgroups, it reflects recent evidence that points in that direction.

3.1.2 Colombia

Additional evidence comes from Colombia, where from 1992 to 1997, a few larger municipalities operated a secondary school voucher program.[11] The stated goal of this program was to increase enrollment in grades 6–11, using private-sector participation to ease public-sector capacity constraints that mostly affected low-income households. As a result, the vouchers were targeted at entering sixth-grade students who were: (i) residing in low-income neighborhoods, (ii) attending public school, and (iii) accepted to begin the next academic year at a participating private school. When there was excess demand, the vouchers were generally allocated via lotteries.

Angrist et al. (2002) find that 3 years after the allocation, lottery winners were 10% points more likely to finish the eighth grade, and scored 0.2 standard deviations higher on achievement tests. In addition, they were less likely to work while in school, or marry/cohabit as teenagers. Using a similar design, Angrist et al. (2006) find that positive effects persist in longer term outcomes: voucher winners were 15–20% more likely to complete secondary school, and, correcting for differences in test taking between lottery winners and losers, the program increased college admissions test scores by two-tenths of a standard deviation.

In terms of identifying an effect like Eq. (6), the Colombian voucher experiment raises three important caveats. First, the vouchers were renewable contingent on grade completion, and thus the program included an incentive component — voucher winners faced a stronger reward for doing well at school. Therefore, the superior test performance of lottery winners might be due to external incentives rather than to the type of school attended per se.

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[10] For example, Neal (2009) states that: "Measured solely by achievement and attainment effects, existing evidence does not support the view that private schools are generally superior to public schools in all settings." Barrow and Rouse (2009) conclude that "[t]he best research to date finds relatively small achievement gains for students offered education vouchers, most of which are not statistically different from zero."

[11] For further background on the Colombian voucher system, and for comparisons to others, see King et al. (1997), Angrist et al. (2002), and Epple et al. (2015).

Second, both lottery winners and losers tended to enroll in private schools, particularly in larger cities. Focusing on Bogota and Cali, Angrist et al. (2002) point out that while about 94% of lottery winners attended private school in the first year, so did 88% of lottery losers. This is not surprising to the extent that: (i) a high private enrollment rate in secondary was symptomatic of the very supply bottlenecks that the program was implemented to address, and (ii) applicants were required to be accepted at a private school. The latter likely increased the probability that households with preferences for private schooling applied, and that even lottery losers might have found private options they liked and were therefore willing to pay for independently. Since the reduced-form estimates in these papers are based upon a comparison of lottery winners and losers, they may in some cases measure a "private with incentives versus private without incentives" effect, rather than the effect of private versus public schooling per se.

Finally, the institutional setup implies that many voucher winners who, again, would have attended private school even if they did not win the lottery, used the vouchers to "upgrade" to more expensive private schools. Angrist et al. (2002) observe that the maximum tuition the voucher covered was roughly equivalent to the cost of a low-to-mid-price private school, and that it was common for voucher recipients to supplement this amount. Thus, part of the effect of winning a lottery could reflect access to greater resources, as opposed to a public/private differential.

To summarize, these studies support the hypothesis that the Colombian voucher program enhanced student performance, but they may not directly isolate the benefit of attending a private school as in Eq. (6).

3.1.3 India

India provides another interesting example of a privately funded voucher experiment. As Muralidharan and Sundararaman (2015) point out, in 2008 a foundation began distributing vouchers in 180 villages in Andra Pradesh. The vouchers were sufficient to cover about the 90th percentile of the private school fees in these markets. Baseline tests were conducted at all private and public schools in these villages. All the test takers were then allowed to apply for a limited number of vouchers.

Randomization took place in two steps. First, 90 villages were randomly selected to receive vouchers, and 90 remained in a control group. Second, within the 90 treatment villages, about 2000 of 3000 applicant households were randomly selected to receive vouchers (about 1200 of these actually made use of them). As we shall discuss below, this double randomization allows this paper to move beyond question 1.

Muralidharan and Sundararaman (2015) find that after 4 years of treatment, lottery winners did not have higher test scores than losers in Telugu (the local language), math, English, science, and social studies; in contrast, they did perform significantly better in Hindi. Using other survey evidence, Muralidharan and Sundararaman (2015) point out that these results are consistent with the allocation of instruction time at private

schools. Namely, relative to public schools, private schools seem to devote time to Hindi at the expense of other subjects. This is in turn consistent with parents valuing learning on this subject (perhaps due to labor market returns) above others that are regularly tested.

In addition, Muralidharan and Sundararaman (2015) collected data on school costs. They find that private schools in the villages considered have expenditures that are only one-third of those in public schools. Thus, they emphasize that even if private schools produced no greater gains in Hindi — as in the other five tested subjects — they would still be providing substantial cost savings.

It is worth mentioning that there is evidence of lower private school costs in other settings; particularly in low-income countries. For example, Andrabi et al. (2008) describe that the private sector in Pakistan includes for-profit schools that charge very low fees — in their survey rural private schools charge about $18 a year. Andrabi et al. (2008) provide further information on where these savings originate. Consistent with the majority of educational expenditure in developing countries going to salaries, they find that they reflect that many private schools hire young, single, untrained local women as teachers. They pay them much less than the trained teachers (who are more likely to be men) more common in public schools.

There is nothing a priori wrong with this — these teachers are essentially producing comparable learning for much a lower cost and clearly find the employment opportunity worthwhile. At the same time, this may limit the relevance of such savings to other countries, or even to other educational levels within the same countries. Specifically, Andrabi et al. (2008) highlight that most private expansion in Pakistan has taken place at the primary level. Secondary education might require private schools to hire trained and more specialized teachers, which could drive up their costs up significantly.

To summarize, the literature on whether private schools provide learning gains in excess of those observed among public schools has produced mixed results. There is evidence of greater private gains for some subgroups and outcomes in some settings. But the estimated effects do not seem to be of a regularity or magnitude such that transferring students into private schools would by itself substantially and reliably raise achievement. This finding is consistent with a broader literature on the effects that attending a higher-achieving school or class has on academic performance, even when these transfers occur within a given (public or private) sector. Here again some papers find little or no effect (eg, Cullen et al., 2005, 2006; Clark, 2010; Duflo et al., 2011; Abdulkadiroglu et al., 2014) and some find positive effects (eg, Jackson, 2010; Pop-Eleches and Urquiola, 2013), but no uniform pattern emerges. Beyond this there is evidence of heterogeneous effects in that in some cases some groups (eg, low socioeconomic status individuals) seem to derive higher gains from private enrollment. This is also consistent with some papers in the literature on attending more selective schools or universities (eg, Dale and Krueger, 2002, 2014; Card and Giuliano, 2014).

3.2 Question 2: If Private Schools Raise Achievement More, Is This Because These Schools Are More Productive?

The previous section illustrates that some studies have used experiments to credibly answer question 1. Specifically, there is some evidence that in some cases and for some outcomes, private schools can raise achievement more than public schools, and at lower cost. Question 2 asks whether this reflects higher productivity on the part of private schools. As emphasized in Section 2, this is quite relevant to what these findings imply about the impact of competition more broadly. The more a private advantage is due to a productivity differential, the less likely it is to reflect potentially zero sum (or even negative) mechanisms related to sorting.

In general, the experiments that credibly answer question 1 do not provide a clear answer to question 2. This can be illustrated relative to several of the studies reviewed above. For example, in the study on India, Muralidharan and Sundararaman (2015) find that private schools produced higher learning in Hindi. The result is consistent with private schools being more focused on or better at teaching Hindi. But it is also consistent with a peer effect. For instance, suppose that parents who value Hindi are more likely to choose private schools in the absence of vouchers, and are also more likely to speak Hindi at home or expose their children to television programming in Hindi. This would be analogous to the setting in many countries where parents who value instruction in English (Hindi plays a similar "lingua franca" role in India) are more likely to use private schools. As a result lottery winners may be more exposed to and interact with children who know Hindi. In such a situation the greater achievement of lottery winners could be due to a peer effect, as in Eq. (6). A similar issue emerges in any setting in which some factor is in fixed supply (at least in the medium term); for example, parents who especially value Hindi or well-trained teachers.

One experimental paper that makes an interesting attempt to get around this issue is Bettinger et al. (2010), which also covers the Colombian case reviewed above. This paper attempts to find a setting in which voucher lottery winners did not enjoy a better peer group than losers. If one can thus sign the peer effect in Eq. (6) then one can argue that the effect of switching children to the private sector is mainly working through school productivity. Specifically, Bettinger et al. (2010) focus on applicants who requested vocational schools. As in many countries, these tend to be less selective. On average, therefore, the lottery winners that transferred to these school experience worse peer groups than those they would have encountered in public schools. Yet they still experienced higher achievement. A caveat is that this may not be the case along unobservables.

To summarize, although it is challenging to implement, research that illuminates the channels through which private enrollment affects skill would be of distinct value.

3.3 Question 3: Does Competition From Private Schools Raise Public School Productivity? Does the Existence of Private Schools Otherwise Affect Those "Left Behind" in Public Schools?

As the discussion surrounding Eq. (6) illustrated, question 1 can in principle be addressed in any setting in which private schools exist. All that is needed is exogenous variation in who enrolls in the private as opposed to the public sector. Of course identifying such variation is much easier said than done, but that is essentially what, by using lotteries, the papers reviewed in Section 3.1 achieve.

In contrast, answering question 3 requires settings in which there is variation in the size of the private sector across markets. Such variation can be found in the cross section in many countries. For instance, Epple et al. (2015) cite several countries that have implicitly or explicitly implemented large-scale voucher programs, where large-scale refers to programs in which vouchers are distributed nationwide to any child who wishes to use them. To the extent that households (and schools) react differentially to the introduction of vouchers across jurisdictions, cross-market variation in private enrollment emerges. Such variation can emerge even in the absence of voucher programs provided that private school entry is allowed. For instance, at any given point there is variation in the private enrollment rate across metropolitan areas in the USA or villages in India.

The expectation suggested by Friedman (1962) is that, all else equal, achievement will be higher where the private sector has a greater reach. The concern immediately arises that the "all else equal" clause is violated because such variation is endogenous: market characteristics correlated with higher private enrollment may be the real drivers of market performance. The resulting bias could go either way. For example, it may be that private enrollment is higher in areas that have a higher concentration of motivated or high-income parents. This might cause one to overestimate the beneficial impact of private participation. On the other hand, it could be that private enrollment is higher where public teacher unions most adversely affect performance, with the opposite effect.

As hard as it is to design and run an experiment awarding vouchers, it would be much harder to implement an experiment that induced cross-market variation in private enrollment (and prevented migration, for instance). In part because of this, another part of the literature has focused on large-scale voucher programs and their associated changes in the private share. This yields two analytical advantages. First, it allows analyses to include "market fixed effects," essentially comparing the performance of the same areas before and after significant expansion in private enrollment. This holds constant factors that are potentially fixed over time, such as the prevalence of motivated parents or persistent dysfunction in the public sector. Nevertheless, this is not equivalent to experimental variation and in this sense studies that tackle question 3 are often at a disadvantage relative to randomized experiments focused on question 1.

Second, and quite aside from identification concerns, large reforms may be better suited to revealing the general equilibrium effects of competition. For instance, it may be that the truly beneficial or deleterious effects of private school participation do not really reveal themselves until a market has a chance to experience substantial entry and exit of private schools. In this sense the studies that look at large-scale reforms may have an analytical advantage over small-scale voucher experiments.

A final note before proceeding to the evidence is that even if one were to assume that the fixed effects strategy delivers identification, Eq. (7) suggests that it will be difficult to separately identify effects on the productivity of the public sector from those affecting children through mechanisms like peer effects. As discussed in Section 2 one alternative is to look at the aggregate effect (including both private and public schools), and some of the papers reviewed below attempt that.

The remainder of this section focuses on two reforms that introduced "unrestricted" voucher schemes — those of Chile and Sweden. Although we shall not discuss them here because work on these cases has less directly addressed question 3, we note that Denmark, Holland, and New Zealand have also implemented large-scale school funding schemes that effectively function as voucher systems.[12] A final section makes brief reference to evidence from Canada, India, and the USA. These three countries have not implemented large-scale reforms and so they are not as suited to considering question 3, although there is nonetheless relevant research.

3.3.1 Chile

In 1981, Chile introduced a voucher system that led to perhaps the largest policy-induced expansion in private schooling in history.[13] Prior to this three types of schools were in operation: (i) public schools were managed by the National Ministry of Education and accounted for about 80% of enrollments, (ii) unsubsidized private schools catered to upper-income households and accounted for about 6% of enrollments, and (iii) subsidized private schools did not charge tuition, received limited lump-sum subsidies, were often Catholic, and accounted for roughly 14% of enrollments. The reform transferred public schools to municipalities, simultaneously awarding them a per-student

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[12] See Epple et al. (2015) for further description of these cases. For further reference on Denmark, see Justesen (2002); on Holland: Patrinos (2002) and Levin (2004); on New Zealand: Adams (2009), Ladd and Fiske (2001), and Lubienski et al. (2013).

[13] This section draws on Hsieh and Urquiola (2006), Urquiola and Verhoogen (2009), and McEwan et al. (2008). Here we use Chile as a setting to analyze question 3. It has also been used to analyze questions 1 (and to a lesser extent 2), but the literature has had more trouble arriving at identification as clear as that in the randomized studies reviewed above. For further background on the Chilean voucher system, its evolution, and its comparison to others, see Gauri (1998), McEwan and Carnoy (2000), Bellei (2007), Mizala and Urquiola (2013), and Neilson (2013).

subsidy sufficient to cover their costs. In addition, subsidized (or "voucher") private schools began to receive exactly the same per-student subsidy as municipal schools. Unsubsidized private schools continue to operate largely as before.

While all schools must participate in annual standardized exams, private schools generally operate under fewer restrictions. They can be religious and/or for-profit. They are allowed to implement admissions policies, albeit with increasing restrictions in recent years, at least in principle. After 1994 private voucher schools were allowed to charge tuition add-ons. Public schools are more constrained in many of these dimensions. They are not allowed to turn away students unless oversubscribed, and cannot charge tuition at the primary level; they essentially do not at the secondary level either.

These changes resulted in substantial private school entry. By 2009, about 57% of all students attended private schools, with voucher schools alone accounting for about 50%. Recent years have seen further reforms. In 1997, schools charging tuition add-ons were forced to provide exemptions on these for a percentage of low-income students. Recent legislation aims to eliminate the add-ons in the coming years. In 2008 the voucher was increased for low-income students, albeit only for schools agreeing to conditions including limitations on the selection of students. The recent reforms also include ending the ability of private voucher schools to operate for-profit, and further prohibitions on the selection of students. As often, the implementation details surrounding these reforms will be important.

Recall that several of the difficulties raised in answering question 3 arise due to sorting. Thus a crucial question is whether Chile's reform led to stratification, as would be predicted, for example, by theoretical models such as Epple and Romano (1998). Hsieh and Urquiola (2006) suggest that this indeed happened. In general terms, there was a "middle class" exodus from public schools consistent with cream skimming. There is also evidence that dynamics leading to stratification have continued both between and within sectors, and that at this point Chile displays one of the highest levels of school stratification by socioeconomic status in the OECD (see, for instance, Mizala and Urquiola, 2013; Valenzuela et al., 2013).[14]

Thus the analytical issues induced by sorting and highlighted in Section 2 are relevant in Chile. For example, Hsieh and Urquiola (2003) point out that addressing question 3 with a regression in the spirit of Eq. (7) suggests that private competition worsens public performance. Specifically, public schools have lower average test scores in areas with more private enrollment. While this could reflect an adverse effect of private schooling on public sector productivity, it could also be driven exclusively by sorting and peer effects.

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[14] For other examples of school market liberalization leading to stratification, see Mbiti and Lucas (2009) on Kenya. For related evidence in the USA, see Urquiola (2005).

As stated, one way of netting out the effects of sorting is to focus on whether areas in which the private sector grew more displayed relative aggregate improvement. In this spirit, Hsieh and Urquiola (2006) apply a difference-in-differences approach to municipal-level data for 1982–96, suggesting that while areas with greater private growth display clear signs of greater stratification, they display no relative advantage in terms the evolution of achievement on standardized tests and years of schooling. As stated above the key caveat — despite the use of some candidate instrumental variables (eg, population density) — is that private entry into school markets is endogenous. For instance, if outcomes had been declining in areas where the private sector grew more, these effects would underestimate the salutary effects of competition.[15]

Bravo et al. (2010) consider the labor market rather than test score effects of the growth of private enrollment. The idea they exploit is that individuals who were more "exposed" to the 1981 reform — measured by the number of years they were still in school after the reform — should fare better. As in Hsieh and Urquiola (2006), this could reflect a host of mechanisms, and the idea is to capture an aggregate effect. They use a structural approach to analyze individuals' dynamic school and labor market choices. That said, the identification challenges cited above are still present, as are the need for several simplifying assumptions.[16] The authors use 2002 and 2004 survey data (with retrospective questions) to estimate the model. They then use simulations and compare the outcomes of individuals who were exposed to the post-1981 regime for their whole school career to those who were never exposed. A key finding is that individuals' average lifetime earnings are not affected by the reform, although this reflects different impacts at different educational levels: attending primary school after the reform raised earnings, but attending secondary school tended to reduce them. The latter result in turn reflects that while educational attainment rose with the reform, this postponed individuals' entry into the labor force and lowered the return to secondary education. Despite the lack of an overall effect on earnings, the authors find that the reform resulted in generalized and significant gains in average discounted lifetime utility. These arise from the utility of time spent attending school and not working.

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[15] One alternative to considering the performance of certain markets over time is to use cross-sectional variation. In other work, Auguste and Valenzuela (2006) use a 2000 round of standardized tests to implement a cross-sectional variant of this approach, using distance to a nearby city as an instrument for the private share. They also find evidence of cream skimming, but in contrast to Hsieh and Urquiola (2006) significant positive effects on achievement. Finally, Gallego (2006) implements a similar cross-section specification on 2002 testing data using the density of priests per diocese as an instrument for the prevalence of voucher schools, and also finds substantial effects of the competition proxy on average student achievement. Yet again questions surround the exogeneity of this variation.

[16] For example, Epple et al. (2015) note that for tractability the model assumes individuals are of "just" three types. This limits the extent to which sorting effects can be analyzed.

Another way of getting at whether greater private participation has improved aggregate performance is by looking at Chile's performance in international tests. If the effects of a growing private share were substantial, then one would expect Chile's performance in international (or national) tests to have improved over time, and/or for the country to be an outlier in performance relative to GDP per capita. Hsieh and Urquiola (2006) point out that over the first two decades after the reform, Chile's relative performance worsened. Hanushek et al. (2012) point out, however, that this trend then reversed. Specifically after dropping from 1999 to 2003, Chile's eighth-grade math and science scores increased substantially from 2003 to 2011. They estimate that Chile had the second-highest growth rate among 49 countries they studied. On the other hand, recent news reports indicate that this progress significantly decelerated or stagnated, depending on the subject, by the 2013 round.

In looking at such long-term trends, however, identification issues begin to loom even larger. For instance, while the decade that featured the most improvement saw further expansion in the private school share, it also featured: substantial increases in GDP per capita and educational expenditures, expansions in pre-school enrollments, reforms to rules governing university admissions, and reforms to the voucher system itself. Thus, it is very difficult to causally assign periods of improvement (or for analogous reasons, lack of improvement) to the growth in the private sector.

Chile is one setting where the literature has turned to considering whether the design of competition-related policies matters. For example, Hsieh and Urquiola (2006) briefly point out that private schools may not have been competing on productivity but rather on peer composition. In theoretical work, MacLeod and Urquiola (2009) and MacLeod and Urquiola (2015) formalize this notion. The essential idea again goes back to Friedman, who suggested that competition will improve outcomes as firms endeavor to develop reputations for quality. MacLeod and Urquiola thus ask: What is a school's reputation? Suppose it is given by the average skill of its graduates — good schools are those whose graduates have skills that are useful in the labor market or perhaps in higher education. This implies that schools' reputations depend not just on their value added, but also on the quality of students they admit. The implication they work out is that vouchers systems will work better if they restrict private schools' ability to select students. In essence, they suggest that voucher systems that borrow elements used for charter schools in the USA — which must select students via lotteries, for example — will be more likely to ensure that competition from private schools leads to greater value added.

In broadly related empirical work, Neilson (2013) asks how the design of voucher payments may affect the incentive that schools have to engage in supply side responses (eg, by raising their productivity). This paper considers a recent reform of the Chilean voucher system: the introduction of targeted vouchers in 2008. This reform in most cases eliminated tuition charges for poor students, and increased the payments to schools when they took on such children. Neilson's point is that this may have significant effects, as in

some cases the Chilean market actually features — despite a universal voucher system — little competition between schools in poor neighborhoods. This reflects that low-income households can be very sensitive to distance and price. Using a structural model he calculates that the introduction of targeted vouchers allowed the effective prices of private schools for lower-income households to drop. This prompted schools to compete for these customers by raising productivity, since the targeted voucher made more expensive schools attractive to poor students. Although subject to several assumptions, the results suggest that in the case of Chile the introduction of targeted vouchers raised school value added and reduced the performance gap between poor and nonpoor children significantly.

3.3.2 Sweden

Prior to the early 1990s, almost all Swedish children attended municipal schools.[17] The national government funded these and hired teachers as well. A 1991 reform introduced three changes. First, the government awarded lump sum funding to municipalities. Second, "open enrollment" plans were instituted at the municipal level, such that in principle students could attend any school in their jurisdiction. Third, the government mandated that municipalities fund independent schools with a per-student payment equivalent to the resources they would have spent themselves.

As in Chile, independent schools may be religious and/or operated for-profit. They are not allowed to charge tuition add-ons, and must be open to all students regardless of their municipality of origin, ethnicity, or religion. At the compulsory level, admissions priority depends on proximity to the school, wait list (first-come, first-served), and sibling presence. Ability-based admissions are allowed at the secondary level.

Like Chile's, Sweden's voucher reform can be used to analyze question 3. Sandstrom and Bergstrom (2005) focus on whether individuals in public schools perform better if they live in municipalities that have a larger share of independent schools. As discussed this is difficult to ascertain as results could be driven by sorting. In addition the paper uses cross-sectional variation, but it nonetheless provides a useful introduction to the outcomes and issues. Specifically Sandstrom and Bergstrom (2005) report that independent and public schools indeed enroll different types of students. For example, independent school students are more likely to be immigrants and/or to have parents with higher income and education (see also Bjorklund et al. (2005) on this issue).

Sandstrom and Bergstrom (2005) implement a Heckman correction and, to address the endogeneity of private entry, use variables approximating whether local authorities are "hostile" to independent schools. Specifically, they proxy for this attitude using measures of the extent to which municipalities contract out responsibilities to the private

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[17] For further reference on the setup of Sweden's voucher system, see Bohlmark and Lindahl (2007, 2008) in addition to several papers cited in this section.

sector. The assumption is that this attitude will only affect educational outcomes through the channel that municipalities with less hostility will be less likely to block private school entry. The key finding is that the presence of greater independent school competition results in better public performance in a GPA-type measure, as well as in standardized mathematics exams and in an indicator for whether students passed all three exams necessary for high school admission (see also Ahlin (2003) for related results).

Given the concerns generated by sorting another possibility is to look at aggregate effects. Bohlmark and Lindahl (2008) ask if outcomes improved by more in municipalities that experienced more extensive private entry. This analysis has advantages — and raises analogous caveats — to the work on Chile presented by Hsieh and Urquiola (2006). In implementing this approach Bohlmark and Lindahl (2008) focus on three types of outcomes measured at different points of students' careers: (i) GPA after the first year and at the end of high school, (ii) a dummy for having completed at least 1 year of higher education within 6 years of leaving compulsory schooling, and (iii) years of schooling 8 years after leaving compulsory school.

After showing that there are at most slight differences in preexisting trends in municipalities' performance along these dimensions, Bohlmark and Lindahl (2008) find: (i) a small positive effect of vouchers on average ninth-grade GPA, (ii) little evidence that the positive ninth-grade effect persists to the end of high school, and (iii) no evidence of effects on university attendance and years of schooling.

Bohlmark and Lindahl (2012) extend this analysis using data for a longer time span — all cohorts finishing ninth grade from 1988 to 2009. They look at average performance according to the growth of the independent sector in 284 municipalities. This leads to significantly more positive conclusions. Specifically, outcome measures include combined test scores in language and math at the end of ninth grade, combined grades in these subjects at the end of ninth grade, the fraction of students completing at least one semester of university education, and average years of schooling at age 24. All of these measures are found to increase with the share of independent-school students.

As noted above, mobility of students across districts argues for aggregation of districts to define the "market" as the set of districts among which students may choose. When Bohlmark and Lindahl conduct the same analyses after aggregating to the local labor market level, the findings with respect to test scores and grade gains prove to be robust in magnitude and significance, but the effects on college attendance and years of schooling do not.

Bohlmark and Lindahl (2012) make two additional points. First, they attribute the more positive results in the longer-term study to the fact that the independent school sector did not grow immediately, with growth picking up only around 2004. Second, they contrast their findings for Sweden to Hsieh and Urquiola's (2006) conclusions on Chile. They observe that their more favorable findings are consistent with the predictions of the reputational model of MacLeod and Urquiola (2009). Specifically, the idea

is that competition will be most effective when it is not associated with significant sorting, as in this case parents' preferences for schools will be more likely to be driven by school productivity as opposed to school peer composition.

Some recent work suggests some caution with respect to the test score–related results in Bohlmark and Lindahl (2012) (the ones that are most robust to aggregation). Specifically, while the content of these tests is nationally standardized, they are graded locally at each school. A concern is that independent schools might be grading more leniently. This was recently analyzed in a regrading exercise described by Tyrefors Hinnerich and Vlachos (2013). Independent graders reexamined tests from different schools. The authors point out that independent schools were more likely to have their grades lowered after a second examination. It is possible that the independent schools — perhaps under greater pressure to please parents and to compete — engaged in more grade inflation.[18]

Finally, as in Chile, one can look at the evolution of Sweden's performance in international test scores (with analogous threats to identification). Sweden has seen significantly deteriorating performance in the years since vouchers were implemented. Perhaps this is not surprising; as in Chile, there is consensus in Sweden that the voucher program has not been a panacea and is in need of reform.

3.4 Evidence From Small-Scale Programs Related to Question 3: Canada, India, and the USA

Evidence on question 3 can also originate from smaller-scale programs. For instance, Chan and McMillan (2009) consider the effects on public school performance of a private school tax credit in Ontario. This is analogous to the tax-credit funded voucher programs in the USA analyzed by Epple et al. (2015). This credit was implemented on short notice in 2001, and became available to families in January 2002. The plan provided a credit that was scheduled to grow in increments over 5 years, although it was canceled before the end of this period. Using the 2002–03 private school enrollment share in a public school attendance zone as their measure of private school competition, Chan and McMillan find that a one standard deviation increase in competition is associated with a statistically significant 0.1 standard deviation increase in the percentage of public school students achieving the provincial performance standard for grade 3.

Turning to India, the experimental design in Andra Pradesh was unique in that randomization involved not only students but also towns/markets. This allows Muralidharan and Sundararaman (2015) to go beyond the usual comparison (lottery winners vs lottery losers) and address potential externalities on children who remain in public school, thus addressing question 3. For example, by comparing nonapplicants in towns that did not receive vouchers to nonapplicants in towns that did, they obtain an estimate of the effects

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[18] This brief synopsis is based on correspondence with the authors, as the article cited is in Swedish. In addition, see the reporting in Fisman (2014).

on children "left behind" in the public sector. The authors find little if any evidence of such effects.

The USA has also not implemented a large-scale national voucher scheme like Chile or Sweden. This is in part due to its very decentralized school system. This decentralization introduces competition, however, because it provides choice between public school districts rather than between private and public schools. Such competition lies outside the scope of this chapter, but it bears mentioning that here again the literature has produced mixed results rather than a distinct sense that greater competition raises achievement (eg, Hoxby, 2000; Rothstein, 2007). Similarly, there are different claims as to whether Tiebout choice leads to stratification (eg, Clotfelter, 1998; Hoxby, 2000; Urquiola, 2005). Finally, there is also literature on the effects of private voucher-induced competition on public school performance in the USA (eg, Hoxby, 2002a; Chakrabarti, 2013; Figlio and Hart, 2006, 2014) with the caveats surrounding the discussion around Eq. (7) and also raised in presenting the evidence on Chile and Sweden.

To summarize, question 3 is most squarely addressed by the literature on large-scale voucher reforms. This work offers relevant analytical advantages, not least the fact that it allows one to observe situations in which the entry of private schools may display its full effect. This very characteristic, however, also introduces complications, such as the difficulty of isolating causal relationships. Keeping that in mind, the conclusions that emerge from this research also offer a mixed assessment of the impact of competition from private schools. First, there is relatively strong indication that private school entry can lead to stratification by socioeconomic status and other characteristics — although this would seem to depend on the institutional design in question. Second, the effects of competition on achievement appear more mixed.

4. CONCLUSION

This chapter has illustrated that assessing the impact of competition from private schools requires addressing a host of questions. If each of the three questions raised in this chapter cannot be clearly answered, then a full understanding of the consequences of competition is not attainable.

In terms of question 1, the evidence from randomized experiments suggests that transferring children from public to private schools may indeed increase their achievement. Yet the evidence is surprisingly mixed given the usual expectation. In particular, multiple experiments suggest little or no improvement for multiple outcomes. The most positive effects, at least in the USA, emerge for subgroups of generally lower socioeconomic status students. Work in Colombia finds systematically positive results, subject to some interpretation-related issues. All of these results are based on experimental designs, and so conform to a high standard in terms of identification.

In terms of question 2, the experimental evidence does not reveal if a private advantage originates in a particular channel (eg, higher private school productivity) although there is suggestive evidence that in low-income countries and at low-educational levels, the private sector can deliver cost savings. The difficulty in isolating channels has important implications. For instance, the case for transferring children to the private sector is significantly weakened if at least part of the private advantage is not due to productivity differences.

In addition, question 3 raises the need to assess the effects of private entry on the students that remain in the public sector. This is generally not possible in the context of randomized experiments, and so researchers have considered large-scale voucher programs that provide a chance to study how given educational markets change when there is substantial private entry. This research has produced mixed evidence. On the one hand, the findings suggest that greater private participation can cause more sorting/stratification. On the other hand, the evidence on achievement effects is mixed. A challenge in this area is credibly establishing causality. For instance, the effects of large-scale reforms take a long time to observe — making it hard to disentangle the effects of private entry, for example, from those of other reforms or events.

The gaps in knowledge around questions 1–3 suggest pathways for future research. In addition, the variety of impacts observed suggests that the effect of competition from private schools may be endogenous to how it is designed. For example, it may be that to successfully enhance the effects of competition from private schools it is not enough to introduce vouchers that are generally in the spirit of Friedman (1962), but that careful attention must be put on the design of these vouchers and/or the rules that govern private entry. Further the effects of competition-related policies may depend on context (Hanushek et al., 2013). Exploring this may be a productive area for future research.

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