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The Role and Impact of Private Schools in Developing Countries: A Response to the DFID-Commissioned ‘Rigorous Literature Review’

James Tooley and David Longfield

March 2015
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James Tooley and David Longfield
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ABOUT THE AUTHORS

James Tooley is Professor of Education Policy and Director of the E. G. West Centre at Newcastle University. He is the author of The Beautiful Tree (Penguin Books India), a best-seller in India and winner of the 2010 Sir Antony Fisher Memorial Prize. This book built on his ground-breaking research on private education for the poor in India, China and Africa, for which he was awarded Gold Prize in the first International Finance Corporation/Financial Times Private Sector Development Competition. His research has featured in documentaries for the BBC and PBS, where it was profiled alongside the work of Nobel Laureate Muhammad Yunus and Hernando de Soto.

Following on from his research, Tooley has dedicated himself to creating and improving working models of innovative practice in low-cost private education, to help explore its potential to extend access and improve educational opportunities for the poor. Amongst other roles, he is Chief Mentor, National Independent Schools Alliance (India); Patron, Association of Formidable Educational Development
(Nigeria); Chairman, Omega Schools Franchise Ltd (Ghana); and Patron, Independent Private Schools Association (Sierra Leone).

Professor Tooley has held a number of teaching and research posts around the world, including at Oxford and Manchester universities. He was also Director of the Education Unit at the Institute of Economic Affairs. His first job was a mathematics high-school teacher in Zimbabwe. His latest book is From Village School to Global Brand, a case study of a chain of schools originating in Lebanon in 1866, which now runs charter schools in America, public-private partnerships in Iraq and elsewhere in the Middle East and a range of private schools.

David Longfield is a researcher in the E.G. West Centre, Newcastle University, where his research focuses on education in post-conflict countries including South Sudan, Liberia and Sierra Leone. He studied for his first degree at Cambridge University followed a few years later by a PGCE at Newcastle University. He taught mathematics for fourteen years in South India, where he also held various senior management roles. Returning to the UK in 2005 he studied an M.Ed. in International Development and Education at Newcastle University.

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show from his research that many poor parents could and did afford them, and the outcomes they achieved often exceeded those of neighbouring public schools.

The problem Tooley faced for much of his career was that there was a hegemonic idea that dominated (and to a large extent still dominates) the major global institutions in the development field and most of the academic work that was commissioned. This idea was that ‘public’ was better and more equitable than ‘private’; the solution to the developing world’s education problems was therefore to ensure universal access to government schools by raising the funds to make this provision possible. The hegemonic idea is so powerful in its influence that it still effects the entire narrative: for example, while it is widely known that Nobel Prize-winner Malala Yousafzai’s father was a head teacher, hardly anyone mentions that he was head of a low-cost private school.

There was one small problem with this dominant narrative: while the global establishment shared it, parents across the developing world didn’t. They wanted schools where teachers turned up and taught lessons and where children learnt. All too often they saw that government schools did not offer that.

So, for the past ten years or more, millions of parents across the developing world voted with their feet. The poor management of many...
public systems, which resulted in mass teacher absenteeism and very poor outcomes combined with remorseless urbanisation, fuelled this demand to the point where in many large cities, such as Delhi, Karachi and Accra, the majority of low-income families now choose low-cost private schools for their children. The hegemonic idea began to unravel from the bottom up.

So now the argument turned to access and quality. Are the poor, especially the very poor, choosing low-cost private schools? And how does the performance of these schools compare to that of government schools? The UK Department for International Development, which has become much more evidence-based and less ideological on this subject in the past five years, commissioned the Rigorous Review of the evidence with an eye to answering these and other questions of vital importance to public policy. The Review concludes that, on the whole, low-cost private schools do outperform government schools and that many poor parents are using them.

Tooley (who knows the evidence in this field better than anyone because he ploughed his lonely furrow for so long) believes the authors of the Rigorous Review have underestimated the positive impact of low-cost private schools and in places have misinterpreted the evidence.

In this paper, he critiques the Review in time-honoured, rigorous academic fashion. Because he has been involved in changing the facts on the ground and has engaged repeatedly in what has often been a heated debate, it is easy to forget the single most important aspect of Tooley’s career: namely, that he is an outstanding academic who marshals evidence, wields ideas and writes superbly.

This paper is as timely as it is powerful. It should be of interest to everyone who wants to see the educational opportunities of children in the developing world transformed. This is the key to prosperity, social justice and successful diverse societies. Indeed, it is the issue of our time. Tooley’s contribution and that of his co-author David Longfield here will help all those with influence make decisions informed by the evidence.

The future lies not in purely public solutions nor in purely private ones. The road to hell is paved with false dichotomies. Instead, the ideal lies in a judicious combination of both—with government as funder and regulator but not as monopoly provider. As the hegemonic idea of the past twenty years loses its potency, Tooley’s new paper will help us find a better, more productive way forward.

Sir Michael Barber
In April 2014, a report commissioned by the Department for International Development (DFID) was published: *The Role and Impact of Private Schools in Developing Countries* (Day Ashley et al. 2014; henceforth, the ‘Rigorous Review’), aimed at exploring controversies surrounding (low-cost) private schools. The overriding research question of this project was ‘Can private schools improve education for children in developing countries?’ (Rigorous Review, p. 2).

The Rigorous Review selected fifty-nine studies from a much larger sample according to quality and other criteria (pp. 60–5). It explored three ‘thematic fields of analysis’: supply, demand and enabling environment. These were further analysed under eight hypotheses and seventeen assumptions, the propositions against which research evidence was tested. Of the twelve assumptions at the heart of the debate, the Rigorous Review found that in most of these (seven) the evidence was positively in favour of private schools (see Table ES.1). From this, it arrived at, at best, lukewarm conclusions about private schools, suggesting that the evidence was positive regarding their quality and cost-effectiveness but negative or ambiguous concerning equity, affordability and financial sustainability.

This response to that report suggests that these findings may not adequately reflect the studies surveyed, as the Rigorous Review has shortcomings in the following three main areas:

1. reading of evidence;
2. assumptions;
3. evidence missed or duplicated.

**READING OF EVIDENCE**

In several cases, literature that clearly says one thing is presented as saying the opposite or is much more nuanced than the Rigorous Review suggests. Some of the many examples include:

- On quality (learning outcomes), Wadhwa (2009) is said to show neutral evidence about the superior performance of private schools. In fact, in thirty-seven out of forty sets of data, private-school performance is superior to that in government schools.
- On quality (teaching), Ohba (2012) is held up as negative evidence with regard to pupil–teacher ratios (PTRs) in private schools. In fact, it shows that, on average, PTRs in private schools were around half those in government schools.
- On equity (gender), Hartwig (2013) is reported as negative evidence, showing that there is gender inequality against girls in private (secondary) schools in rural Tanzania, whereas, in fact, it shows precisely the opposite — far more girls than boys...
Table ES.1 Findings of Rigorous Review and our revised findings.

<table>
<thead>
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<th>Assumption</th>
<th>According to Rigorous Review</th>
<th>Revised assessment</th>
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<tr>
<td></td>
<td>Overall strength of evidence</td>
<td>Result</td>
</tr>
<tr>
<td>1. Better learning outcomes</td>
<td>Moderate</td>
<td>+</td>
</tr>
<tr>
<td>2. Better teaching</td>
<td>Strong</td>
<td>+</td>
</tr>
<tr>
<td>3. Geographically reach poor</td>
<td>Weak, by definition (neutral findings)</td>
<td>0</td>
</tr>
<tr>
<td>4. Equally accessible to girls</td>
<td>Moderate</td>
<td>−</td>
</tr>
<tr>
<td>4*. Improve education for girls</td>
<td></td>
<td>Moderate</td>
</tr>
<tr>
<td>5. Cost of education delivery lower</td>
<td>Moderate</td>
<td>+</td>
</tr>
<tr>
<td>6. Financially sustainable</td>
<td>Weak, by definition (small number of countries and studies)</td>
<td>−</td>
</tr>
<tr>
<td>7. Poor(est) are able to pay fees</td>
<td>Weak, by definition (neutral findings)</td>
<td>0</td>
</tr>
<tr>
<td>8. As affordable as state schools</td>
<td>Weak, by definition (small number of studies)</td>
<td>−</td>
</tr>
<tr>
<td>8*. Nearly as affordable as state schools</td>
<td></td>
<td>Moderate</td>
</tr>
<tr>
<td>9. Perceived quality underpins choice</td>
<td>Moderate</td>
<td>+</td>
</tr>
<tr>
<td>10. Choice is informed</td>
<td>Moderate</td>
<td>+</td>
</tr>
<tr>
<td>11. Users participate in decisions</td>
<td>Weak, by definition (small number of studies)</td>
<td>+</td>
</tr>
<tr>
<td>12. Responsive to user demands</td>
<td>Weak, by definition (small number of studies)</td>
<td>+</td>
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</table>

Key: + evidence supports assumption; − evidence counters assumption; 0 evidence is ambiguous.
ExEcutivE Summary

...do private schools increase or decrease disadvantage relative to government schools? A more illuminating assumption, worded to explore whether private schools have a beneficial impact on girls’ education, is positively supported by the studies in the dataset.

- On affordability, studies which are reported to show only very small percentages from the poorest quintile accessing private schools (e.g., Härnä and Rose 2012) in fact show much larger proportions, once the literature is properly ‘interrogated’. Studies that show private schools to be unaffordable by the poorest (e.g., Akaguri 2013) appear to show the same is true of government schooling, an additional important dimension missed by the Rigorous Review.

ASSumPTIONS

The framing of several of the assumptions leads to a less favourable view of the role and impact of private schools than would assumptions framed only slightly differently; two in particular seem like ‘straw-man’ assumptions, the wording of which makes it impossible to see private schools in a favourable light:

- On equity (gender), ‘private schools are equally accessed by girls and boys’ (Rigorous Review, p. 24) is highly unlikely to be achieved in contexts where there are cultural and socio-economic barriers within the communities towards girls’ education – and where all school types face the challenges posed by these cultural values. The important question instead is how the gender ratio in private schools compares to that in government schools. Where cultural and socio-economic barriers exist, do private schools increase or decrease disadvantage relative to government schools? A more illuminating assumption, worded to explore whether private schools have a beneficial impact on girls’ education, is positively supported by the studies in the dataset.

- On affordability, the Rigorous Review has put forward a ‘straw-man’ assumption, that private schools are as affordable to users as state schools. No one but an extreme libertarian would suppose that 100 per cent of government subsidies would always be misdirected and so have no impact at all on affordability. A revised assumption, that private schools are nearly as affordable as government schools, shows this positively supported from Ghana and Kenya, although not from India.

Similarly, the interpretation of some assumptions appears designed to lead to a negative appraisal of the role of private schools. For instance, on equity (geographically reaching the poor), the assumption appears to be interpreted to mean that an unspecified but very large percentage of the poor and poorest should already be in private schools. With this interpretation, the Rigorous Review finds only ambiguity and no positive evidence in favour of private schools. A more realistic interpretation, bearing in mind that this is an initiative that has arisen from within poor communities themselves, is that private schools serve at least significant minorities of the poor and poorest. With this alternative interpretation, the evidence becomes positive in favour of private schools.

...are in private secondary schools. Moreover, Pal (2010) is reported as giving neutral evidence for gender parity. In fact, the evidence from five Indian states shows the same percentage of boys and girls in school – in other words, positive evidence.
It is important to note that the assumptions were created by the Rigorous Review authors, based on their initial reading of the evidence and policy debates. We suggest that their approach has led to biased assumptions; we point to better phrased assumptions that genuinely help examine the role played by private schools.

**EVIDENCE MISSED OR DUPLICATED**

In several cases, evidence from the selected literature that could have informed the assumptions is simply missed out; sometimes this evidence would completely reverse the conclusions reached. Evidence is also duplicated: studies using the same datasets are used as distinct pieces of evidence, thus lending greater support to certain conclusions than is warranted. For instance:

- On equity (gender), Härmä (2011) and Härmä and Rose (2012) are held up as two pieces of negative evidence, but these report the same data. Despite the Rigorous Review’s assurance that it would exclude duplication, these are counted as two pieces rather than as one piece of evidence.

- On financial sustainability, the Rigorous Review finds only two studies on longevity of private schools, a useful proxy measure they use for financial sustainability. But there are at least seven other articles in their own sample with relevant data. All contradict the negative result given by the Rigorous Review.

- On affordability, two studies (Ohba 2012 and Heyneman and Stern 2013) that report that private schools appear more affordable than government schools are simply ignored by the Rigorous Review.

The Rigorous Review focused only on studies ‘published’ in the past five years. This arbitrarily, but entirely predictably, excluded research by pioneers in the field. Reputable studies that arrived at conclusions different from those in the Rigorous Review were curiously ignored – even when they had been a key focus in an earlier summary (Mcloughlin 2013) of the same work. ‘Published’ was also broadly interpreted. While some articles in peer-reviewed journals were excluded, other documents, such as a five-page, unpaginated document explicitly reporting provisional (not finalised) evidence (Wadhwa 2009) were accepted. Detailed discussion about research methodology is not entered into; however, it is clear that many of the articles accepted used research methods which do not allow for the kinds of generalisations made in the Rigorous Review.

These criticisms notwithstanding, this response uses only the studies selected by the Rigorous Review. With a revised analysis and modification of two of the assumptions, our response finds that all twelve of the assumptions are positive in favour of private schools, with the most important ten out of these twelve moderately or strongly supported in terms of overall strength of evidence (Table ES.1). Instead of the lukewarm conclusions about private schools, this leads to a much more positive assessment of their current and potential roles in development.

---

1. In our bibliography, this is Härmä 2011a, but for consistency with the Rigorous Review, throughout the text we have referred to this as Härmä 2011.
In April 2014, an important report, *The Role and Impact of Private Schools in Developing Countries*, was published (Day Ashley et al. 2014). This ‘rigorous literature review’ was commissioned by the Department for International Development (DFID) to shed light on controversies surrounding private schools and development, in particular ‘low-fee’ or ‘low-cost’ private schools delivering education to the ‘poorer sections of societies’ (Day Ashley et al. 2014, p. 1; henceforth, Rigorous Review). The overriding research question of this project was ‘Can private schools improve education for children in developing countries?’

It is now widely accepted that low-cost private schools exist in large numbers across developing countries, in both poor urban and rural settings. From tentative initial reports on the sector (e.g., Tooley 2000a, 2000b), there is now a burgeoning literature on low-cost private schools, including several major books (e.g., Srivastava and Walford 2007; Tooley 2009; Dixon 2013; Srivastava 2013; Macpherson et al. 2014). However, the literature reveals a hugely polarised debate about the significance of low-cost private schools, their potential role and their impact.

Why the controversy? Earlier, one of us used the phrase ‘de-facto privatisation’ to describe this low-cost private school movement (Tooley and Dixon 2006); this phrase is now in wide circulation (e.g., Committee on the Elimination of Discrimination Against Women 2014; Rolleston and Adefeso-Olateju 2014; Rigorous Review, p. 38). But this term carries huge significance: privatisation is the assigning of businesses or services to private rather than state control or ownership. It is normally considered a ‘top-down’ approach (governments ‘denationalise’ particular industries, e.g., railways or steel). ‘De-facto’ privatisation, on the other hand, is a ‘bottom-up’ or ‘grass-roots’ privatisation, where the people themselves, not the state – indeed, their actions are often against the wishes of the state – are engaged in reassigning education to private rather than state control and ownership. There is a lot at stake if the people themselves appear to be rejecting sixty-five years of development consensus that emerged from the Universal Declaration of Human Rights in 1948.

The DFID-commissioned report finds much that is positive about the contribution of non-elite private schools, most importantly in terms of quality and cost-effectiveness. First, the Rigorous Review finds well-supported evidence that children attending private schools have better learning outcomes than those in government schools, even after controlling...
for background variables, and that the commitment of teachers is much higher in private than in government schools.\(^1\) The Rigorous Review also finds well-supported evidence that the cost of educational delivery is lower for private than government schools; in combination with the evidence on quality, this points to greater cost-effectiveness for private over government schools.

However, in other critical areas, such as equity, affordability and financial sustainability, the Rigorous Review finds against private schools’ contribution. Concerning equity, the Rigorous Review is unable to report that private schools ‘geographically reach the poor’ and finds that ‘girls are less likely than boys to be enrolled in private schools’ (p. 2). Moreover, the Rigorous Review finds ‘no positive evidence’ that ‘the poor are able to pay private school fees’ (p. 2), while private schooling is found to be more expensive than government education. Finally, it suggests that low-cost private schools are not financially sustainable but instead ‘may be vulnerable to closing down after short periods of time’ (p. 3).

This response to the Rigorous Review questions some of these findings. It suggests the Rigorous Review has serious shortcomings in the following three main areas:

1. reading of evidence;
2. assumptions;
3. evidence omitted or duplicated.

**READING OF EVIDENCE**

In several cases, literature that clearly says one thing is presented as saying the opposite or is much more nuanced than the Rigorous Review suggests. Reading of literature in many cases appears perfunctory, rather than ‘rigorous’ – for instance, citing an article’s conclusions without checking that these are well supported by the evidence given.

**ASSUMPTIONS**

The framing of several of the assumptions leads to a less favourable view of the role and impact of private schools than would assumptions framed only slightly differently. Two in particular seem like ‘straw-man’ assumptions, the wording of which makes it impossible to view private schools in a favourable light, while the interpretation of a third again appears designed to underestimate the contribution of private schools.

**EVIDENCE OMITTED OR DUPLICATED**

In several cases, evidence from the selected literature that could have informed the assumptions is simply missed out; sometimes this evidence would completely reverse the conclusions reached. Evidence is also duplicated: studies using the same datasets are used as distinct pieces of evidence, thus lending greater support to certain conclusions than is warranted.

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1. The term ‘well-supported’ is sometimes used as shorthand for the Rigorous Review’s ‘strong’ and ‘moderate’ overall strength of evidence. Because of the way the criteria were set out, it was very difficult for any hypothesis to be given ‘strong’ overall support. At least three of the four criteria categories would need to be individually rated ‘strong’; typically, this would mean at least ten published studies conducted in more than five countries, with 75 per cent of the studies’ findings pointing in the same direction.
These three areas will be discussed in relation to the Rigorous Review’s hypotheses and assumptions in Chapters 5–12 of this paper. Identifying problems in the analysis requires going into some detail, hence this forms the main body of this response.

In this response, we will not (apart from a few warning remarks) challenge the quality of the fifty-nine studies chosen for inclusion in the Rigorous Review, including the research methods used. To do so would bring up technical issues concerning sampling, validity, reliability and generalisability, which are beyond the scope of this paper: we do not wish to detract from the focus on low-cost private schools. This means that if the study has been selected by the Rigorous Review, then we will not exclude it based on methodological considerations, whatever our reservations about research methods. However, one non-technical issue addressed concerns ‘cogency’ (Rigorous Review, Appendix 6). One of the criteria used for assessing articles for the Rigorous Review was: ‘Are the conclusions clearly based on the study’s results?’ We suggest that there are a number of times when this basic question does not appear to have been asked, or at least not correctly answered.

We will not address the Rigorous Review’s selection of fifty-nine studies for examination, except to make brief comments about one method of exclusion and some of the exclusions themselves, both of which raise important questions (Chapter 4). In addition, we will briefly discuss the Rigorous Review’s Theory of Change (Chapter 3), suggesting it includes assumptions which do not seem to be at the core of the debate. Before embarking on this, Chapter 2 discusses definitions used in the Rigorous Review and this response. Our concluding remarks can be found in Chapter 13.

2. On a few occasions, however, we have suggested excluding studies when they do not appear relevant to the assumption being examined.
2. DEFINITIONS

A minor area of disagreement with the Rigorous Review concerns definitions. Private schools are familiar enough. They are defined by the Rigorous Review (p. 4) as satisfying three criteria. Private schools are:

1. dependent on user fees to cover all or part of their operational and development costs;
2. managed largely independently of the state;
3. owned or founded independently of the state.

Given these three criteria, the Rigorous Review then distinguishes between ‘private schools’ and ‘other non-state schools, such as schools run by charities, NGOs or religious organisations’ (p. 5), which will be the focus of a second rigorous review and then a subsequent synthesis review (p. 5). That is, the ‘private schools’ supposedly under review appear to be those run by individuals, partnerships or companies, rather than by these non-profit organisations. This is very odd, for three reasons. First, each of the latter type of school typically meets all of the three criteria above: schools run by charities, NGOs or religious organisations typically charge fees that cover ‘all or part’ of their costs, are managed largely independently of the state and are owned or founded independently of the state.

Second, in several of the Rigorous Review’s studies it is made clear that the latter type of school is in fact included. For instance, from Kenya, Ohba (2012) makes clear that the private schools in its sample are run by community groups, charities and churches as well as proprietors. In fact, there is just one proprietor-run school (Ohba 2012: 771, Table 1; see also p. 767). Also from Kenya, Tooley et al. (2008) similarly point to non-profit and for-profit private schools in their sample; parallel observations appear in Hartwig (2013) and Phillips and Stambach (2008) from Tanzania, and Srivastava (2008b: 453) from India.

Third, concerning India, which provides a large number of the studies used in the Rigorous Review, none of the schools could legally be called ‘private’ in the restricted sense of proprietor-owned schools, because, by law, all private schools have to be run by non-profit bodies (usually societies or trusts). This was pointed out by, among others in the Rigorous Review’s sources, Srivastava (2008b), which notes the 1993 Supreme Court ruling that ‘schools should not be run for profit’ (p. 453).

If the Rigorous Review wishes to distinguish between these different types of schools, it is suggested that all be referred to as ‘private schools’ – they satisfy the three-pronged definition – but that a further distinction is made between ‘non-profit’ private schools, run by churches, mosques, charities and NGOs, and
‘for-profit’ private schools, run by proprietors, partnerships or companies (see Tooley and Longfield 2013a, 2013b). For India, a third category would be substituted for the second: ‘de-facto’ for-profit private schools, ostensibly run by a non-profit trust or society but in effect run by an individual or partnership. (Whether a school fitted into this category could really only be ascertained on inspection.) Notwithstanding this, it is clear that the Rigorous Review does in fact look at both types of private school, non-profit as well as for-profit; it is therefore not clear how the subject of the second rigorous review will differ from the first.

On the subject of definition, we should note that the controversial debate is about schools which the Rigorous Review calls ‘low-fee private schools’ (LFPs). In a footnote, the Rigorous Review observes that these schools are also referred to as ‘low-cost private schools’ but that this usage is ‘contentious’ because ‘some commentators consider that the poor demand education at a low price to them, not a low cost of delivery’ (p. 4, footnote 1). This suggests that LFPs could simply mean schools run at high cost, but subsidised, rather than schools run at a low cost. Rather than clarifying the position, this actually detracts from the object of investigation. Lewin (2007), for instance, specifies that the ‘low-fee’ private schools his teams are researching are unsubsidised private schools. Yes, the poor can ‘demand education at a low price to them’; however, without subsidies, i.e. in the situation under investigation in developing countries, this has to mean that education is provided at a ‘low cost of delivery’ – that is the only way fees can be low.

For this reason, we suggest that the preferred term should be low-cost private schools, precisely because this delineates those schools that have a low cost of delivery (all the costs of inputs are low, reflected in low fees charged to parents) rather than high-cost schools which could provide low fees if they are subsidised.1

Finally, how low is ‘low-cost’ or ‘low-fee’? This is not defined in the Rigorous Review. The authors note that they were ‘not always able to talk about “low-fee” private schools … with certainty’ (Rigorous Review, p. 5) because there was not always detailed information on fees charged; nevertheless, the focus was on ‘private school delivery of education to poorer sections of societies’ (p. 1) and on ‘non-elite private schools’ (p. 5). Mostly these are likely to be low-fee private schools; this caveat is assumed in the discussion that follows. How low is low, however, will remain undefined for this response.2

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1. It is conceded, however; that there is no good name for the sector. ‘Microfinance’ benefited from having a snappy name; it would do a good service if someone could suggest a suitable one here.

2. Elsewhere, we have suggested a more exact definition (see Tooley and Longfield 2013a, 2013b; Longfield and Tooley 2013), which we hope will gain traction.
The Rigorous Review seeks to answer the research question ‘Can private schools improve education for children in developing countries?’ (p. 1). More specifically, given the context of the work, we take it to mean, ‘Can private schools, especially low-cost private schools, improve education for (poor) children in developing countries?’

Out of all the research in this area, the team finally selected fifty-nine studies ‘published’ in the past five years that pass muster in terms of their quality and other criteria. These studies were then explored in the context of three ‘thematic fields of analysis’: ‘Supply’, ‘Demand’ and ‘Enabling Environment’ (p. 6). Under each of these fields, the team created eight ‘hypotheses’.

1 Private schools are better quality than state schools.
2 Private schools provide education to disadvantaged children.
3 Private schools are cost-effective and financially sustainable.
4 Private schools are affordable to the poor and the poorest.
5 Demand for private schools is driven by informed choice and a concern for quality.
6 Private schools are accountable to users.
7 State collaboration, financing and regulation improves private school quality, sustainability and equity.
8 Private schools have positive effects on the overall education system.

Under each hypothesis were constructed ‘assumptions’, seventeen in total, the propositions against which the research evidence was tested, to arrive at conclusions concerning the role of private schools in improving education for poor children in developing countries.

It is very important to emphasise the origin of these hypotheses and assumptions. They were created by the authors based on their initial ‘rapid appraisal of policy debates and research findings undertaken in the inception phase of this review’ (p. 6). Getting these assumptions right is clearly crucial to arriving at a correct interpretation of the research being reviewed.

It is our view that some of the assumptions have been expressed in ways that will inevitably show private schools in an unfairly unfavourable light. For instance, it is likely, a priori, that Assumption 4, ‘Private schools are equally accessible by boys and girls’ (p. 24), will be negatively supported, given that much of the research was conducted in countries where it is known that there is not equal access of boys and girls to schools in general and hence probably not in private schools either. We suggest that this much will have been obvious.
Getting these assumptions right is clearly crucial to arriving at a correct interpretation of the research being reviewed.

from the authors’ ‘rapid appraisal of policy debates and research findings’ (p. 6). The way the assumption has been expressed will determine the research finding. Below we suggest an alternative way of wording this assumption that would allow evidence for and against private schools to be genuinely weighed in the balance.

Similarly, no reading of the literature or understanding of policy debates, we contend, could lead anyone to Assumption 8: ‘Private schools are as affordable to users as state schools’ (p. 29). No one, a priori, would think this was true — state subsidies are highly unlikely to be wasted all of the time in all places — so, again, the way the assumption has been phrased seems destined to lead to a negative conclusion about private schools. We have suggested an alternative wording for Assumption 8 that genuinely allows us to explore the affordability of private compared to public education.

Returning to the three ‘fields of analysis’, ‘supply’ concerns areas such as quality, equity, access and cost-effectiveness (incorporating financial sustainability), while ‘demand’ focuses on affordability, choice and accountability. The ‘enabling environment’, however, is different. It focuses on issues to do with governments: on collaboration, financing and regulation and also on whether private education ‘complements’ public education and leads to improved quality in both types of school.

The Rigorous Review appears to give each of these fields equal prominence and weighting. However, in an earlier paper based on the same evidence (Mcloughlin 2013), ‘supply’ and ‘demand’ are separated from ‘enabling environment’: the first two are included in the chapter ‘Evidence on the Role and Impact of Low-Cost Private Schools’ (closely related of course to the title of the Rigorous Review), while the third is in the separate ‘Support to Low-Cost Private Schools: Challenges, Approaches and Lessons’.

We think this separation of these areas (supply and demand from enabling environment) is the better approach, fitting in more clearly with the Rigorous Review’s aims. The team is trying to answer the research question ‘Can [low-cost] private schools improve education for [poor] children in developing countries?’ (p. 6). They are searching for evidence, therefore, on the role and impact of private schools, so it is the first two areas, ‘supply’ and ‘demand’, that are most significant. Only once we have determined that private schools can improve (and indeed are improving) education does it make sense to look at the ‘enabling environment’, to see how governments can, have and perhaps should interact with the private sector.

This is additionally important because, in the Rigorous Review’s conclusions, it is stated that ‘the majority of assumptions [nine out of seventeen] at the heart of this debate are in fact weakly evidenced’ (p. 50, emphasis added). It is our contention that, of the seventeen assumptions, only twelve could be considered to be at the ‘heart’ of the debate about low-cost private schools. The rest are mainly about
government capacity, something peripheral to the debate. This alters the arithmetic of what is well supported, even on the Rigorous Review’s reading of the evidence.

Partly for these reasons, and partly due to our own resource constraints, we have focused on the assumptions that relate most directly to the overriding research question, ‘What is the role and impact of private schools in improving education for the poor?’ The main body of what follows therefore addresses the first twelve assumptions; for the sake of completeness, brief notes are made about the discussions of state capacity and markets (Assumptions 13–17).
4. EVIDENCE INCLUDED OR OMITTED

In this response, we will not be commenting on how the literature was selected as evidence for the Rigorous Review, except for making these four observations.

First, the Rigorous Review was to look at ‘the latest quality published evidence’ (p. 4) on private schools in developing countries. On the one hand, their definition of ‘published’ appears all-embracing: their final selection includes, for instance, a five-page, unpaginated document explicitly based on provisional rather than confirmed data (Wadhwa 2009). On the other hand, however, their final selection excludes some studies on low-cost private schools published in reputable academic journals which have already gone through the rigours of peer review (and which were published in the correct time period and focused on countries of interest to DFID). In total, less than half of the sources (twenty-seven out of fifty-nine, 46 per cent) were published in academic journals, while a further eleven were published in academic books, which may also have had some form of (usually less rigorous) peer review. That leaves twenty-one out of the fifty-nine sources unlikely to have been peer-reviewed at all.

Second, the team narrowed down the focus from the past ten years to the past five years for date of publication, which they said gave a ‘natural’ cut-off date (p. 10), as 79 per cent of the studies in their initial bibliography were published after the beginning of 2008. Using other selection criteria on this 79 per cent of studies, including a focus on ‘DFID priority countries’, ‘more exacting’ quality criteria (p. 11) and the avoidance of repetition (p. 10), they end up with fifty-nine acceptable studies.

While we understand the relevance of the second sift, as the review is for DFID and it is vital to test hypotheses using high-quality, non-repetitive research studies only, it appears strange, when the research interest in low-cost private schools is relatively new, to have a cut-off date at all. Assuming that these other selection criteria would have reduced the pre-2008 studies in a similar proportion as the post-2008 material, this suggests the cut-off date removed only around sixteen studies which otherwise passed the quality and relevance criteria. Given this well-resourced study, it is not clear why it could not also have included this small number of additional studies. It does have the unfortunate – but we suggest entirely predictable – outcome that much of the research from pioneers in this field has been excluded.

It is important to stress that the five-year cut-off was for date of publication not date on which the research was conducted. This much of the research from pioneers in this field has been excluded."
means that some included studies are based on data collected back in the 1990s, whereas some studies excluded because of the publication-date restriction are in fact based on much more recent data.

Third, while we do not wish to name certain authors excluded from the Rigorous Review, even when they have published in peer-reviewed sources during the allowed period, perhaps for obvious reasons, there is work from other excluded authors which perhaps we can name. We mention only two notable examples – there are, of course, others. While Härmä’s work in India (which draws rather negative conclusions about private schools) is very well represented (as we shall see, three articles are used as major sources of evidence, albeit all using the same dataset), there is only one jointly authored article focused on her work in Nigeria, and this is based on a small-scale study. It seems odd that her magisterial large-scale quantitative surveys from Lagos and elsewhere are not included (see, e.g., Härmä 2011b, 2011c, 2013). These studies are far more positive about the role of private schools than Härmä’s work in India; had they also been included, then much of the evidence, including that on equality for girls in private schools, might have been represented in a much more affirmative way.

Another strange omission is Dixon’s book on low-cost private schools (2013), which includes research from several parts of rural and urban India relevant to many of the propositions explored in the Rigorous Review. This omission is particularly curious because it featured heavily in an earlier summary of the same Rigorous Review evidence, published by its second author (Mcloughlin 2013). Dixon’s book is featured on pp. 4, 6, 8, 9, 11, 12, 14, 15, 17, 18, 20 and 21 of Mcloughlin (2013). What transpired between Mcloughlin (2013) and Day Ashley et al. (2014) that led to its exclusion? Again, including this work would have altered the balance of evidence and challenged the Rigorous Review’s findings, as indeed is made clear in the review’s earlier version – see Mcloughlin (2013), for example, p. 9 on gender equality in private schools, p. 14 on affordability of private schools and p. 16 on longevity of private schools.

Finally, one way in which the Rigorous Review team attempted to capture as much literature as possible was to consult ‘a selection of experts working in this area’ (p. 9). Disappointingly for us, one omission from their list of sources given in Appendix 5 is Newcastle University’s E.G. West Centre. This is especially curious because researchers there are described, among others by The Economist (2012), as pioneers of research in the field of low-cost private education. Had this Centre been consulted, more research might well have been brought to light that would have led to the Rigorous Review drawing conclusions different from those currently presented.

We now turn to examine the evidence adduced by the Rigorous Review and to explore
its conclusions about the role of private schools in improving ‘education for all’. Chapters 5 to 10 take us through what are considered to be the major hypotheses concerning supply and demand, while Chapters 11 and 12 make brief comments about the remaining hypotheses.
The first hypothesis of the Rigorous Review is that ‘Private schools are better quality than state schools’ (p. 14). This leads to two ‘testable assumptions’ which are well supported by the literature.

ASSUMPTION 1: PRIVATE-SCHOOL PUPILS ACHIEVE BETTER LEARNING OUTCOMES THAN PUPILS IN STATE SCHOOLS

Initial finding
The Rigorous Review cites twenty-one studies, as follows: positive (fourteen), neutral (six), negative (one). The headline finding is that ‘Pupils attending private school tend to achieve better learning outcomes than pupils in state schools’ (p. 15).

Strong findings in favour of private schools
This is a very important and robust finding: that private schools are better quality than government schools. For instance, excellent studies from India include Desai et al. (2008) and French and Kingdon (2010), which show ‘positive private school achievement advantage based on standardised test scores’ even after controlling for observable and unobservable household factors (Rigorous Review, p. 15). Other rigorous studies find similar effects from other settings, including Africa (Rigorous Review, p. 16).

However, because the Rigorous Review finds six neutral and one negative study, this means that there is only a ‘medium level of consensus’ about this finding – a strong level of consensus would require greater than 75 per cent of the evidence (i.e. sixteen articles rather than the fourteen found) to support the thesis (p. 12). But when the ‘neutral’ and ‘negative’ findings are examined, it is clear that some of these have been misplaced – enough indeed to move this finding to be strongly supported. We will look at the supposedly ‘negative’ evidence first, followed by questioning two studies in the ‘neutral’ category.

Negative evidence
As far as ‘counter-evidence’ is concerned, the Rigorous Review notes ‘Another way of approaching the private sector advantage is by analysing rates of transition from primary to secondary schools’ (p. 17). This is an odd suggestion, true only if the transition between primary and secondary school was due solely to the quality of schooling received at primary level, rather than other factors such as poverty, motivation for schooling or government policy. This seems unlikely. One study is cited here: Ohba, researching in Kenya, ‘finds that government primary school leavers were more likely to enter government secondary schools than private school leavers’ (2012: 17). This is given as counter-evidence.

1. ‘State’ schools are used by the Rigorous Review to mean ‘government’ or ‘public’ schools in the international sense.
to the assumption that private schools are superior in terms of quality.

Now, Ohba (2012) is a small-scale study, with twelve opportunistically chosen private schools and two government schools, so it is odd that the Rigorous Review assumed it was possible to generalise from the results. Further, Ohba admits that ‘data obtained from the two government schools were not as reliable as those obtained from the private schools’ (2012: 770). It turns out that the private schools’ owners, with admirable concern for their charges, knew of ‘the whereabouts of each primary school leaver’ (p. 769), whereas the government head teachers thought that ‘once pupils had graduated, they were no longer the school’s responsibility and there was thus no obligation to track their progress’ (p. 770). In the end, the government head teachers had to ‘guess the whereabouts of each school leaver’ (p. 770, emphasis added). So the government evidence is likely to overestimate retention to secondary school, particularly as the government head teachers ‘assumed that those who had performed well must have gone on to and stayed on at secondary school’ (pp. 773–5).

(Ohba specifically states on pp. 773 and 775 that the government figures in particular are likely to be an overestimate.)

Now, as noted, the Rigorous Review reports that Ohba ‘finds that government primary school leavers were more likely to enter government secondary schools than private school leavers’ (2012: 17). Figures from Ohba’s Table 2 (p. 774) confirm this: 34.9 per cent of private but 43.4 per cent of government school-leavers go on to some form of government secondary school. However, it turns out that the reason appears to be because of government policy favouring government school-leavers:

\textit{In general, government primary school-leavers seem to have had better access to a government secondary school than their private counterparts. This might have been due to the quota system and the government preoccupation with attempting to ensure that pupils graduating from government primary schools should benefit from government secondary education.}

(Ohba 2012: 775, emphasis added)

Ohba goes on to give the reason why government has taken this course of action: ‘This is because private schools out-perform the public schools in the KCPE [Kenya Certificate of Primary Education] examination by a considerable margin … Thus, there is an attempt by the government to provide pupils graduating from government primary schools with vacancies for the top national secondary schools’ (2012: 775, emphasis added).

Indeed, a private schools’ organisation (the Kenya Private Schools Association) is reported as requesting the Ministry of Education to review this policy because ‘denying children access to public resources because of the primary schools they attended is … discriminatory’ (Ohba 2012: 775).

The Rigorous Review does refer to the quota system (p. 18) but appears to conclude that the slightly lower proportion of private-school pupils progressing to government schools is an indication that private schools are doing
poorly, when the quota system is actually in place because children in private schools are performing well!

Finally, we can note that the selective reporting by the Rigorous Review of progress to government secondary schools alone is misleading; it turns out that a slightly higher proportion of private than government students progresses to private secondary schools. Indeed, when all destinations are considered, 77.9 per cent of private primary-school-leavers are known to continue in some form of education, compared to an almost identical 79 per cent of government primary-school-leavers. Ohba (2012) is clearly positive, not counter-evidence.2

Neutral evidence
There are six pieces of neutral evidence given. Of these, two cannot possibly be considered as neutral evidence.

First, Wadhwa (2009) is reported as finding that ‘upon controlling for covariates, differentials in reading outcomes between government-private schools disappeared in some states, widened in others and reversed in a few’ (Rigorous Review, p. 17, emphasis added).

Now, Wadhwa (2009) is a rather thin document, explicitly based on provisional (not finalised) evidence from ASER (Annual Status of Education Report) 2009 research. It is hard to see how this satisfied the quality review procedures for the Rigorous Review. Regarding the India-wide study, after controlling for other factors, for reading in the local language, Wadhwa found that ‘the learning differential between government and private schools falls drastically from 8.6 percentage points to 2.9 percentage points – from 20% to a measly 5%.3 This means that 2/3rd [sic] of the learning differential between government and private schools can be attributed to factors other than the type of school’ (Wadhwa 2009). That is, the private schools actually account for a 2.9 percentage-point differential in performance compared to the government schools. This is a private school advantage. Similarly, in English, the raw scores show a 17.7 percentage-point advantage (or 67 per cent), which is reduced to a 10.8 percentage points (or 41 per cent) advantage when other factors are controlled. Again, this is unquestionably a considerable private school advantage.

Perhaps the evidence is ‘neutral’ because of variation between states? For ‘reading in own language’, after controlling for background variables, private schools have the advantage in seventeen out of twenty states, and in a majority of these (ten out of seventeen), the difference is greater than 8 percentage points. In ‘reading in English’, after controlling for background variables, private schools have the advantage in all twenty states, including a massive 31.65 percentage points in Punjab, 22.86 percentage points in Karnataka and 22.26 percentage points in West Bengal. Overall, there are forty sets of data, and in thirty-seven

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2. Assuming, of course, that the study should be included at all, given its methodology. We will not repeat this caveat for other studies in what follows.
3. The 5 per cent seems to be incorrect: a fall from 8.6 to 2.9 percentage points goes from 20 per cent to 6.7 per cent (not to 5 per cent) which gives the one-third and two-thirds split between school effect and other factor effect that is cited.
of them the indications are that the private schools perform better!

Finally, the Rigorous Review commented that, after controlling for background variables, the differences between government and private schools were ‘reversed in a few’ states (p. 17, emphasis added). In fact, they were reversed in only two states, and in only one was the reversal in favour of government schools. (In one, the controlled difference changes from a private school advantage of 2.1 percentage points to a government school advantage of 3.39, while in the other it changes from government school advantage of 1.39 percentage points to a private school advantage of 0.75.)

If this study is to be included (as it has been by the Rigorous Review), then it is clearly not ‘neutral’ but should be reclassified as further ‘positive’ evidence in favour of private schools.

Second, Johnson and Bowles (2010) ‘using middle and secondary exam data from rural Madhya Pradesh conclude that private school students did not perform any differently from their government counterparts’ (Rigorous Review, p. 17). In this case study, the researchers simply report raw pass-rate data from government exams, so there is no sophisticated analysis as in other studies. Nevertheless, curiously for a supposedly ‘neutral’ study, this evidence in fact points to private outperforming government schools. They report that ‘private schools in our study were clearly outperforming the government schools primarily in terms of offering instruction outside of the government curriculum and preparing children for the major board exams’ (Johnson and Bowles 2010: 499).

This study is clearly positive not neutral.

Revised finding
In the Rigorous Review, there are now twenty-one studies, as follows: positive (seventeen), neutral (four), negative (zero). This finding now has a ‘strong’ level of consistency (as more than 75 per cent of the studies are positive, the Rigorous Review’s criteria for this level).

Caveat
Whenever the Rigorous Review finds something favourable in terms of private schools, it tends to give a caveat. Here it is noted that, while private schools have superior performance over government schools, the ‘overall learning levels of children in rural areas in many countries remain worryingly low, whether at private or public schools’ (p. 18).

But surely it would be odd to expect an initiative that has emerged entirely from poor communities, without a long history or the benefits of any research and development (R&D) expenditure, to be already offering an education of international standards? Indeed, it is worth emphasising the quite extraordinary findings of the Rigorous Review. It is remarkable that a grass-roots initiative is found to be delivering education of a higher quality than that provided by government and extensively funded by international agencies. Instead of the rather negative caveat, perhaps the more obvious conclusion to draw from this finding is that if these private schools could benefit from some of the resource input that government schools have enjoyed, then even higher standards, including international standards, could be within reach.
ASSUMPTION 2: TEACHING IS BETTER IN PRIVATE SCHOOLS THAN IN STATE SCHOOLS

Initial finding
In the Rigorous Review, there are fourteen pieces of evidence as follows: positive (twelve), neutral (one), negative (one).

The review states that private-school teaching is better ‘in terms of more teacher presence and teaching activity, and teaching approaches that are more likely to lead to improved outcomes’ (p. 19). This time, the overall finding is strongly positive in favour of private schools: there is no inconsistency in the findings. Indeed, the evidence is even more positive for private schools than the review suggests. For instance, looking at Hartwig’s evidence from fifty-six villages in Tanzania (2013), the Rigorous Review claims that ‘private secondary schools on average have a PTR of 33:1 and government ones had a PTR of 48:1’ (p. 20). In fact, these figures have been misread: they are the students-per-classroom figures given by Hartwig; the PTRs are even more favourable to private schools, with private at 23.5:1 and government at 61:1.

Negative evidence
Ohba (2012) is once again held up as counter-evidence, based on using PTRs as a proxy for teaching quality. ‘Ohba’s (2012) study in the slum area or Kibera refutes the assumption [that teaching is better in private schools than in state schools], and finds that, in this instance, PTRs in private schools were often higher than in government schools’ (Rigorous Review, p. 21, emphasis added).

As noted earlier, Ohba (2012) gives evidence from fourteen opportunistically selected schools, twelve private and two government. Indeed, two of the private schools do have higher PTRs than one of the government schools; ‘often higher’ is an odd way of putting this (see Ohba 2012: 771, Table 1). It is normal to use averages when dealing with data of this kind. On average (mean), in the twelve private schools, the PTR is 28:1, while in the government schools it is 51:1. (In fact, of the fourteen schools in the study, the ten schools with the lowest PTRs are all private.) So this is again positive evidence for private schools, not the opposite as curiously claimed by the Rigorous Review.

Revised finding
In the Rigorous Review there are now fourteen pieces of evidence as follows: positive (thirteen), neutral (one), negative (zero).

Caveat
Again, the Rigorous Review comes up with a caveat here, an ‘unintended consequence’ (p. 21). This is that low-cost private schools ‘keep costs low by exploiting labour markets for less qualified and less experienced teachers working on significantly lower salaries’ (p. 21, emphasis added). A more balanced approach might note, in addition, that these grass-roots initiatives, emerging from within
poor communities themselves, are significant employers of local people, including women, who otherwise might not find work, and that this is a further positive benefit of the rise of low-cost private schools.

**SUMMARY: PRIVATE SCHOOLS ARE BETTER QUALITY THAN GOVERNMENT SCHOOLS**

The evidence given in the Rigorous Review is well supported: private schools are of higher quality, in terms of educational outcomes and teacher commitment, than government schools. It does not mean to say that they already satisfy international standards, or that improvements do not need to be made. Although low-cost private schools have emerged without any of the resources of government or international agencies behind them, they are already achieving better results than government schools. This alone is a remarkable and powerful finding.
6. EQUITY

The second hypothesis of the Rigorous Review is that ‘Private schools provide education to disadvantaged children’ (p. 22). The review focuses on ‘two particular disadvantaged social groups’: the economically disadvantaged and girls (p. 22). This leads to two testable assumptions.

ASSUMPTION 3: PRIVATE SCHOOLS GEOGRAPHICALLY REACH THE POOR

Initial finding
In the Rigorous Review, eight studies are included, with the following assessment: positive (three), neutral (four), negative (one).

The findings being inconsistent, by definition, the ‘overall strength of evidence’ is ‘weak’ (see Rigorous Review, p. 12). The headline finding is as follows: ‘The evidence is ambiguous about whether private schools geographically reach the poor. While private schools continue to cluster mainly in urban areas, they are increasingly prevalent in rural areas. However, most research cautions against assuming that this means they are increasingly accessible to the poor’ (p. 22). In short, the Rigorous Review finds it cannot conclude that private schools geographically reach the poor.

What is meant by ‘geographically reaching the poor’?
One reason for the ‘ambiguous’ results seems to be the difficulty there is in interpreting the assumption. Compare and contrast these three studies, the first given as positive, the second negative and the third neutral:

1. (Positive) ‘Kremer and Muralidharan (2008) find that 28 percent of rural India has access to a private school … the presence of private schooling in India is actually greatest in the economically poorest states’ (Rigorous Review, p. 23).

2. (Negative) ‘Pal’s (2010) study in rural areas of five Indian states suggests private schools are mainly located in better-off villages that generally have better infrastructure, thereby limiting the extent to which they can claim to reach the true disadvantaged’ (Rigorous Review, p. 23).

3. (Neutral) Andrabi et al. (2008) ‘document the significant extent to which the private school phenomenon has reached rural regions of Pakistan … the presence of a private school was correlated with certain village characteristics, including not only infrastructure

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1. They found that 28 per cent of the villages in their sample had a private school. It is also worth noting the extraordinary range, with some of the poorest states in India having large majorities of villages with private schools: Rajasthan (52 per cent), Bihar (54 per cent), Uttar Pradesh (57 per cent), Punjab (65 per cent) and Haryana (68 per cent). Note that the Rigorous Review has Kremer and Muralidharan 2008, but this is an error and should be 2009.
If we put this discussion into the context of the overriding research question, ‘Can (low-cost) private schools improve education for (poor) children in developing countries?’ then one of the major reasons for the ‘geographic’ question is to ascertain how widespread the reach of this phenomenon is, in order to ascertain its generalisability as a solution to education for the poor.

So, if low-cost private schools are already present in rural as well as poor urban areas, then this is the kind of evidence needed to show the geographic reach of private schools. Given the evidence that private schools on average perform better than government schools, this would show their capacity ‘to improve education for poor children’. to assess this dynamic initiative by its present geographical reach when it is still developing, with more schools starting each year in both rural and urban areas, and expecting it to have already come to ‘geographically reach [all or the vast majority of] the poor’ may be expecting too much. What should count as positive evidence in this case would be if private schools were already present in some of the poorest areas, available and accessed by some of the poorest people. this would indicate the potential for this type of school to function in other similarly poor locations among other similarly poor people.

We suggest that each of the studies above, and indeed each study reviewed under this section, is actually giving evidence to support the assumption. There is very little to challenge the assumption, or to suggest a geographic boundary somehow limiting the reach of these private schools.
Revised finding
Given this discussion, we suggest the following revised assessment for the evidence given: positive (eight), neutral (zero), negative (zero). The research clearly shows that private schools are geographically reaching the poor. The overall strength of evidence moves from weak (by definition, because mostly neutral studies) to well supported (strong or moderate), given that all the studies now point in one direction.

Missing dimension
On the positive evidence given, one finding is also notable—and missing in the assumption as it is phrased. This is that private schools seem better able than government schools to serve the poor, for example, by narrowing educational gaps between more and less disadvantaged groups. Pal and Kingdon (2010: 19) observe that the marginal private school effect for SC/ST [Scheduled Caste and Scheduled Tribe – i.e. some of the most disadvantaged groups in India] children are significantly higher than the general population and also it holds for both 10–14 and 15–19 year olds; true effects are likely to be even larger when we address the likely underestimation bias arising from both unobserved heterogeneity and simultaneity. In other words, there is suggestion that there are some large literacy gains to be had from private school growth even among SC/ST children, especially among 10–14 year olds.
On a parallel theme, Heyneman and Stern (2013) point to the role that low-cost private schools play in Jamaica and South Africa by targeting the most disadvantaged in society, those ‘forgotten’ or ‘left behind’ by an education system that has near-full enrolment.

This dimension is worth spelling out: private schools not only geographically reach the poor but their presence appears to be beneficial to the most disadvantaged groups.

ASSUMPTION 4: PRIVATE SCHOOLS ARE EQUALLY ACCESSED BY BOYS AND GIRLS

Initial finding
In the Rigorous Review, twelve studies are included, with the following assessment: positive (two), neutral (three), negative (seven). Their headline finding is: ‘Most of the evidence reviewed indicates that girls are less likely to access private schools than boys’ (p. 24). That is, private schools are not equally accessed by boys and girls. The overall strength of evidence for this is ‘moderate’.

What does this assumption mean?
Once again, we need to remind ourselves that the research question driving the Rigorous Review is ‘Can private schools improve education for children in developing countries?’ When the overriding question is on improvement and the relevant comparison is between private schools and the alternative (government) provision, it appears strange to set the assumption in absolute terms (‘equally accessed’) and to consider only private schools.

The starting point for any comparison is surely important. First, in certain contexts and cultures, there are entrenched cultural and socio-economic barriers to girls’ education. In these situations, there are often more girls than boys out of school. The important question to ask in these contexts is surely the impact of private schools. It would perhaps be unrealistic to expect private schools to already have solved all the problems within these communities, including entrenched attitudes against girls. We need to ask whether private schools are improving the situation for girls or exacerbating it. Are private schools playing a role in educating girls, giving them access and not discriminating against them?

Second, in other contexts, there is full (or nearly full) enrolment, and the assumption is relevant if it is felt that one of the school types is providing ‘better’ education; it is important that girls do not miss out on that ‘better’ education. The earlier assumptions in the Rigorous Review have made clear that private education is likely to be providing this higher-quality education.

So, each study has a background against which the assumption has to be viewed. Some of the studies included as evidence make comparisons in boys’ and girls’ enrolment between private and government schools, while others only compare the enrolment rates of boys and of girls in private schools. We would contend...
that the former studies are much more relevant for the Rigorous Review, while the latter are only helpful in the context of full (or nearly full) enrolment.

Moreover, if our concern is with improving education for girls, there appear to be other dimensions missing from the analysis if the only question asked tests gender parity in private schools. For example, evidence on private schools narrowing gender achievement gaps relative to government schools’ contribution would also seem very relevant to this assumption.

With these considerations in mind, we suggest an alternative wording for this assumption, as follows:

**Assumption 4**: Private schools improve education for girls in developing countries

In what follows, we will go through the evidence that the Rigorous Review adduces with their Assumption 4 as well as the revised Assumption 4 in mind. We’ll go through the positive evidence first, followed by neutral and negative in order:

**Positive evidence**

The Rigorous Review reports two sources, Srivistava (2008a) and Andrabi et al. (2008), which show private schools are equal or better for girls in terms of enrolment. The first ‘finds an equal likelihood of sending girls and boys’ to low-cost private schools in Lucknow (Rigorous Review, p. 25); the second shows that ‘the presence of private schools is strongly associated with female enrolment in rural Pakistan: the share of female enrolment in private schools is 3–5 percentage points higher than in government schools’ (p. 25).

Srivastava (2008a) certainly reports this finding from one of her earlier articles. She also warns that her sample was not representative ‘and not intended to be generalised’ (2008a: 194); nevertheless, as the study has been included in the Rigorous Review, we take it as positive evidence for Assumption 4 as well as Assumption 4*. 

Second, Andrabi et al. (2008) do report this higher share of enrolment in private than government schools from three datasets (p. 340 and footnote 10). Unfortunately, these datasets still show a smaller percentage of girls than boys in private school (e.g., 42 per cent of girls, 58 per cent boys), as well as in government schools (e.g., 37 per cent girls, 63 per cent boys). They report that ‘In settlements without private schools, females are 16 percentage points less likely to be enrolled compared to boys. When there are private schools in the settlement, the enrollment by all children increases, but female enrollment increases more so that the overall gender gap decreases to about 8 percentage points’ (Andrabi et al. 2008: 341-2).

As there is not gender parity (equal access) in private schools, this should not be included as positive evidence for the Rigorous Review’s Assumption 4. It is clearly negative evidence. However, as Andrabi et al. (2008) show private schools improving the situation for girls, this is, on the other hand, positive evidence for our revised Assumption 4*. 

**Assumption 4**: Private schools improve education for girls in developing countries

In what follows, we will go through the evidence that the Rigorous Review adduces with their Assumption 4 as well as the revised Assumption 4 in mind. We’ll go through the positive evidence first, followed by neutral and negative in order:
Neutral evidence

There are three studies which the Rigorous Review regards as ‘neutral’. One appears to have been misread and is incorrectly classified: Pal (2010) uses data from the 1999 Probe Report study in five Indian states. The article reports:

> a significantly larger proportion of boys (60 per cent as opposed to 40 per cent of girls) are ever-enrolled in our sample while a larger proportion of ever-enrolled girls (19.6 per cent as opposed to 15.6 per cent of boys) go to private schools. If however we consider the proportion of total boys and girls going to private schools, the proportion is very similar (around 11 per cent for both boys and girls).

(Pal 2010: 790, footnote 17, emphases added)

In other words, as reported, private schools are more favourable to girls, although more boys are enrolled (overall in school); taking into account this differential enrolment, there is equal enrolment of boys and girls in private schools. This is positive support for the Rigorous Review’s assumption, ‘Private schools are equally accessed by boys and girls’. It is also, of course, positive for Assumption 4*.

The other two studies given as neutral are the following: Johnson and Bowles’ (2010) study from four schools in Madhya Pradesh shows that before private schools were created, ‘girls were already severely limited in accessing secondary schooling’ (Rigorous Review, p. 25). However, private secondary schools were opened, and these ‘marginally reduced the gap in girls’ access’ (p. 24). This shows a positive impact for private schools in reducing the gender gap. Johnson and Bowles portray their findings in this way: private education, they say, ‘had created new opportunities for girls and children of dalit (formerly untouchable) families’ (2010: 487).

Fennell (2012) reports from Pakistan on qualitative evidence collected from interviews with twenty-five parents and twenty-five school-attending youth, plus interviews with teachers and head teachers. It notes that ‘there is evidence that girls’ enrolment has risen as a consequence of a growing number of private schools’ (Fennell 2012: 270). Again this shows a positive trajectory for private schools in increasing opportunities for girls.

Both studies point out the ongoing gender disparity, and, as such, they should be included rather as negative evidence for the tightly worded Assumption 4. However, they both indicate that private schools are improving access to education for girls: they are both positive in terms of our more illuminating Assumption 4*.

2. It would have been helpful if the Rigorous Review could have pointed this out – data from pre-1999 is likely to yield different results from later data, given the increasing wealth of India and the likelihood that this would lead to even greater numbers of girls in private school.
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Negative evidence

There are seven studies given as negative evidence. The Rigorous Review notes that ‘Härmä (2011) and Härmä and Rose (2012) … document girls being significantly less likely to attend LFPs in remote rural Uttar Pradesh’ (p. 24). They do show this. But they report the same data and the same study – Rose added no new data to Härmä. This is made clear in Härmä and Rose (2012), where it is explicitly stated that the discussion there, including on gender inequality, summarises the research ‘described in detail in Härmä (2011)’ (2012: 251). Despite the Rigorous Review’s claim that it avoided repetition (where publications repeated similar findings, only ‘the most empirically focused or higher quality publication was included in the review’, p. 10) this does not seem to have been consistently followed. This is one piece of negative evidence, both for Assumption 4 and Assumption 4*.

Next, the Rigorous Review groups together three of the studies in this way: ‘Further evidence elsewhere of inequality of access for girls to private schools is provided by Aslam (2009) in Pakistan, Hartwig (2013) in rural Tanzania, and Nishimura and Yamano (2013) in rural Kenya’ (p. 24).

One of these studies is clearly incorrectly categorised. Hartwig’s study (2013) from fifty-six villages compares government and private schools at the secondary level only (where there are twenty government or government-aided and six private secondary schools, so a very small sample). In the conclusion, Hartwig does comment that ‘Our secondary school enrolment figures for mixed gender schools suggest that girls still do not have equal access to boys when it comes to school attendance’ (2013: 494, emphasis added). This seems to have been the comment picked up by the Rigorous Review. However, the emphasised phrase is important. Looking at Hartwig’s Table 2, it is true that for the government secondary schools, which are all mixed gender, there are more boys than girls (4,369 boys and 3,482 girls, so 44 per cent girls). However, this is not true in the private schools. Even in the (two) mixed schools, there are more girls than boys (284 girls and 254 boys). Moreover, in the private sector, there are also three single-sex girls’ schools and one single-sex boys’ school. In these schools, there are 1,100 girls and 150 boys. In total, in the private schools, there are 1,384 girls and 404 boys, or 77 per cent girls. So, whereas the public secondary schools in this study cater for more boys than girls, the private secondary schools cater for more girls than boys.

Finally, adding up totals in public and private gives 50.5 per cent girls (4,773 boys, 4,866 girls). In other words, at secondary level, the only place we have any information on private schools, private is much more favourable to girls than government, leading to rough equality overall. In rural Tanzania, one could conclude that parents clearly prefer to send their girls to single-sex private schools, which is why there is not gender parity in the mixed (public) schools.

Curiously, the Rigorous Review goes on to cite this article as fleshing out reasons for gender disparity in private schools:
Aslam then goes on to control for ‘observed and unobserved household characteristics’ (2009: 334) and finds that, once these are taken into account, boys are now more likely to attend private schools than girls (2009: 337–8).

What does this study bring to Assumption 4? The study finds that there are more out-of-school girls than boys, due presumably to socio-economic and cultural factors (many of which are beyond the control of any school). However, the first quote shows that the proportion of girls in private schools is greater than the proportion of girls in government schools. Clearly, this is negative evidence with regards to exact gender parity (Assumption 4). However, for Assumption 4*, as the percentage of girls is higher in private than government schools, this part is positive evidence.

The study then seeks to control for other background and family characteristics, including family fixed effects, and comes to the conclusion that a boy is more likely to attend private school than an equivalent girl (i.e. one with the same household, background and family characteristics). This shows the complexity of the interactions of wealth, family education, etc. (background variables), gender and schooling choice: ‘This suggests that a select sample of girls is enrolled in school … enrolled girls are significantly more likely than boys to be from more affluent and possibly more enlightened homes’ (Aslam 2009: 337). In other words, the greatest discrimination against girls is likely to be in the choice of whether to send them to school at all, not the choice between private or government school.

A number of explanations are offered for the smaller proportion of girls than boys enrolling in private schools … Hartwig’s (2013) case study explains gender disparity through household-level and socio-cultural factors, including … a tendency to invest more in the education of sons, inadequate access to latrines and water at schools (which may prohibit girls’ attendance during menstruation), and concerns about the safety of the environment for girls, who were often perceived by parents to be particularly vulnerable to sexual assault.

(Rigorous Review, p. 24)

This may seem rather excessive coverage for an article that does not actually find the relevant (i.e. in private schools) gender disparity at all. Hartwig (2013) is clearly positive, not negative evidence for both Assumption 4 and Assumption 4*.

Second, Aslam (2009) has to be considered carefully. We include two quotes to illustrate this. Aslam finds that

conditional on enrolment, girls are not any less likely than boys to be enrolled in private schools. Indeed, except in the 20–24 years age group, girls are significantly more likely to be enrolled in fee-charging private schools as compared with boys … in terms of girls’ enrolment, private schools in Pakistan cater as much for girls as for boys.

(Aslam 2009: 333, emphases added)
the greatest discrimination against girls is likely to be in the choice of whether to send them to school at all, not the choice between private or government school.

If we include this in our discussion, then this would be negative for Assumption 4*, so, overall, we can conclude that Aslam (2009) is neutral.3

Finally, Nishimura and Yamano (2013) do find that ‘girls have a 3.6 per cent point lower probability of attending private schools than boys do. The result suggests that gender inequality persists in the access to quality education’ (p. 273). Looking at their Table 5 (p. 272), we see that this result is only significant at the 10 per cent level. Many high-quality studies choose to disregard such probabilities, so some might have considered this evidence to be insufficient to show gender disparity.4 Descriptive statistics are not given, so we cannot compare the proportion of girls and boys in school. Nevertheless, if this study is to be included, we agree that the study is likely to suggest that access is not equal. That is, this appears to be negative evidence for Assumption 4 and possibly for Assumption 4*.

There are two remaining sources of evidence given as ‘negative’. First, Maitra et al. (2011) show that in India there is ‘significant inter-state variation in the degree of female disadvantage with respect to private school enrolment, with large northern states having significantly higher female disadvantage rates when compared to southern ones’ (Rigorous Review, p. 24). This may be a slightly misleading way of presenting what the study actually reports. Maitra et al. (2011: 17) state:

There is indeed a great deal of variation across the different provinces ... The GIRL dummy is not statistically significant for Gujarat and Maharashtra in western India and for the southern states of Kerala and Tamil Nadu. For Gujarat, Kerala and Tamil Nadu, there is no evidence that girls are less likely to be enrolled in private schools relative to boys.

In fact, looking in detail at Maitra et al. (2011), it is clear that in five out of the fourteen states researched, neither the ‘GIRL’ variable, nor any of the other combined gender-related variable measures they examine, are significant. There is no evidence (even at the 10 per cent level) of gender inequality in private schools in states as diverse as Orissa, Jammu and Kashmir, Tamil Nadu, Gujarat and Kerala (Maitra et al. 2011: 36, Table 9). That is, there is no evidence

3. This is the result we include in the revised finding below. However, it may be that we shouldn’t accept the second part of Aslam’s analysis, if we keep strictly to the Rigorous Review’s specification that gender itself is a specific category for disadvantage: ‘Two particular disadvantaged social groups are ... the economically disadvantaged, and girls’ (p. 22, emphasis added). The assumption then is that girls of whatever socio-economic background are disadvantaged. Aslam’s study finds positively that girls who are in school are more likely to be in private school. It only gives a negative finding (so neutral overall) if you bring in socio-economic disadvantage. It is not clear that we need to bring this in, given that girls are considered a separate category of disadvantage.

4. Moreover, this evidence is also about the probability of girls going to school once other pupil and household characteristics are controlled for and so falls under the same caveat as footnote 3 concerning Aslam (2009).
Finally, Pal and Kingdon (2010) is given as negative evidence; they ‘find evidence of gender differentials in access to private schooling’ (p. 24). That is not really what Pal and Kingdon’s paper is about. It is true that the paper does mention the hypothesis that ‘given the importance of son preference especially in some Asian countries, private school growth could widen the gender gap between boys and girls if this induces resource constrained parents to send only their boys to private schools, thus encouraging discrimination against girls’ (Pal and Kingdon 2010: 1). But they also mention (p. 4) the alternative assumption that private schools may mitigate gender differences … if private schools fulfil differentiated demand (e.g., provide local schools so that girls do not have to travel far or provide separate toilets for girls and boys), availability of private schools will increase girls’ access to schooling and learning and thus reduce the gender gap in literacy.

These are assumptions of the paper, not things that are explored. So, in terms of providing evidence for or against the Rigorous Review’s Assumption 4, it is hard to see that this paper offers anything concrete.

However, if we think of the new Assumption 4*, then this does provide useful evidence – positively in favour of private schools. For the paper provides evidence that the greater the number of private schools, the better the impact on closing gender differentials: ‘higher private school share is associated with significantly higher literacy for all age groups while it is associated with significantly lower of female disadvantage in five of the fourteen states analysed.

In other words, while it might be true that in some (they suggest) northern states private schools do not have gender parity, this is not true for other states, including those in the west and south. The study even gives reasons why this might be the case, pointing among other things to lower economic development in the north (pp. 17–18), suggesting that even there this might change as India’s development continues. So this study should not be considered as just presenting counter-evidence to gender parity in private schools. In some states there is counter-evidence, but in other states, including, perhaps, those in southern and western India (where they suggest socio-cultural barriers are lower), private schools are equally accessed by girls as boys.

With different findings from different parts of India, it is an interesting study, but not one that readily fits into the Rigorous Review’s categories of positive, negative or neutral. If we need to come down one way or the other, with some states showing negative and others positive evidence about girls’ access to private schools, we must draw the conclusion overall that the evidence is neutral for Assumption 4 and also for Assumption 4*.
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gender gap in literacy ... among 10–14 year old children’ (Pal and Kingdon 2010: 14). (The age group ten- to fourteen-year-olds, the report says, are the ones ‘who naturally benefitted more from the recent trend of private school growth around the country’, p. 14, see also p. 6.) The study then points out that these are probably underestimates of the size of the private-school effect on the ‘gender gap in literacy’ (p. 14) when other factors are included.

Moreover, this study benefits from disaggregating data about India as a whole to look at different regions. Here it again finds something rather striking. In South India, there is an even more pronounced narrowing of the gender gap: ‘while private school share remains insignificant to determine both literacy and gender gap among 10–19 year olds in the northern districts, both these effects are significant in the southern districts’ (Pal and Kingdon 2010: 17).

So, it is suggested that this study be removed from the sample for Assumption 4 (not because of its methodology but because it is not relevant) but retained as positive evidence for Assumption 4*.

Revised finding
As the discussion above has been quite complex, we have summarised the results in Table 6.1. Assumption 4 (exploring absolute equality for boys and girls in private schools) remains negative. However, for the more meaningful Assumption 4* (exploring the impact of private schools on improving equity), the evidence is positive towards private schools and well supported.

Revised finding: positive (seven), neutral (two), negative (two). The majority of studies show that private schools are improving the situation for girls, even in areas where educational provision overall is inequitable towards girls.

SUMMARY: PRIVATE SCHOOLS MEET THE DEMANDS OF EQUITY

Research evidence shows that low-cost private schools geographically reach the poor. There is no suggestion of a geographical limit beyond which [low-cost private schools] have not or cannot pass.

In South India, there is an even more pronounced narrowing of the gender gap.
Table 6.1 Matrix of research evidence for Assumptions 4 and 4*.

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7. COST-EFFECTIVENESS AND FINANCIAL SUSTAINABILITY

The third hypothesis is that ‘Private schools are cost-effective and financially sustainable’ (Rigorous Review, p. 25). This leads to two testable assumptions.

ASSUMPTION 5: THE COST OF EDUCATION DELIVERY IS LOWER IN PRIVATE SCHOOLS THAN IN STATE SCHOOLS

Initial finding
The Rigorous Review gives the following findings: positive (seven), neutral (zero), negative (zero). The headline finding is that the cost of education delivery is lower in private schools than in state schools often due to the lower salaries of private school teachers’ (p. 25).

Cost of delivery is lower and greater cost-effectiveness in private schools
The Rigorous Review notes that the reason for private schools’ greater cost-effectiveness is the lower teacher salaries compared with government schools. This could either be seen as providing ‘employment where it would otherwise not exist’ or possibly as ‘exploitative’, an area that could warrant ‘further investigation’ (p. 26).

There is very little rigorous evidence in this area, as Day Ashley et al. concur, comparing the total costs of government schools, including costs of the central, regional and local ministry of education offices, with those of private schools (which typically will include all costs in school fees alone). This is an area that could benefit from further research.

Revised finding
We concur with the Rigorous Review’s findings: positive (seven), neutral (zero), negative (zero).

ASSUMPTION 6: PRIVATE SCHOOLS ARE FINANCIALLY SUSTAINABLE

Initial finding
The Rigorous Review says that there are only two studies available here, as follows: positive (zero), neutral (zero), negative (two). The headline finding is that low-cost private schools are not financially sustainable, they ‘may be vulnerable to closing down after short periods of time’ (Rigorous Review, p. 27). The overall strength of evidence is ‘weak’, however, by definition, because of the small number of studies (p. 12).

Counter-intuitive finding
This finding is rather counter-intuitive, given the numbers of low-cost private schools that are found in the market (see below). How does the Rigorous Review arrive at it? First, it notes that the ‘issue of the financially [sic] sustainability of private schools is not directly addressed in the literature reviewed’ (p. 27); instead, they look at an ‘indirect and imprecise’ measure, the ‘length of operation of private
Cost-effectiveness and financial sustainability

Although this does not seem to have been satisfactorily discussed, we shall accept the Rigorous Review’s verdict that this is negative evidence. In passing, we also ask how seriously we should take the comment. It is explicitly a detail obtained after the fieldwork period for the reported study and therefore is either from a new study or is based on anecdotal information. If the former, it would be good to see details of the methodology and analysis (e.g., whether the researcher went back to visit every school site, and, if she found one missing, did she check that the school had not simply moved site, or merged with another school, etc.). If the latter, its validity has to be called into question. However, in the spirit of not rejecting studies based on methodology considerations, we will not pursue this line of enquiry further.

Second, ‘A similar finding is also reported by Tooley et al. (2008), whose interviews in Kenya highlighted that parents felt private schools could close down at any time because they existed merely on the “whim of an individual”’ (Rigorous Review, p. 27, emphasis added). In fact, the interviews were mostly with parents who had moved their children from private to public and then back again to private school, at a particularly fraught time for private schools given the introduction of free primary education in government schools. The fact that parents were willing to do so shows, among other things, their belief in the stability, not fragility, of the private schools chosen. Indeed, it is not clear where the quote about the ‘whim of an individual’ comes from, as it is not from Tooley et al. (2008), although the phrase ‘could close down at any time’ is in fact a direct quote from Härmä (2009: 163).

Negative evidence

First, ‘In their study in rural India, Härmä and Rose (2012) observe that LFPs in their dataset were operating for short periods of time, with as many as a quarter of the sample closing down within 18 months of the end of the study period’ (Rigorous Review, p. 27, emphasis added).

Härmä and Rose (2012) report on Härmä’s work – Rose added nothing to the fieldwork. If we go back to one of the original articles on which Härmä and Rose (2012) is based, we see that this so-called ‘finding’ is reported in passing, as a footnote to the main study: ‘Within 12 months of the completion of the fieldwork for this study, four of the LFPs visited had closed down’ (Härmä 2009: 164, footnote 6). That is, a quarter of the sixteen reported private schools had closed within twelve months. Here we have two oddities: (1) Härmä (2009) is also included in the Rigorous Review’s evidence, so it is strange that the original paper was not reported in this context rather than the derivative paper; (2) Why is it not noted that in the original paper it is a quarter of the schools closing within twelve months, while in the derivative paper it is a quarter closing within 18 months? Which is it? If both are true, then this suggests a period when a number of schools closed (although still a minority) followed by a period of total stability for six months. It is worth knowing what was going on in that initial year before drawing conclusions in a review of this importance.
In fact, rather than ascribing made-up quotes to their work, the Rigorous Review could have used Tooley et al. (2008) for evidence of the proxy measure itself (length of operation of private schools). The paper explicitly explored the objection to private schools that they are ‘mushrooming’, suggesting that all ‘such schools are recently established’ (p. 454). In fact, the mean age of schools in their study was seven years in 2003. A study that positively supports the Rigorous Review’s assumption of school longevity is used instead as counter-evidence.

**Missing evidence**

Moreover, there are at least seven other studies in the Rigorous Review’s sample that also provide evidence on school longevity, yet are ignored:

1. Researching the growth of low-cost private schools in rural Kenya, Nishimura and Yamano (2013) come up with a more or less identical figure to Tooley et al. (2008): private schools in their sample were on average 7.2 years old (p. 269, Table 1).
2. Andrabi et al. (2008: 335) show that in the context of extremely rapid expansion of private schooling, the median age of a private school in 2000 was four years, even though over one-fifth of the schools had opened in the past two years.
3. Härmä and Adefisayo (2013: 133) point to evidence that one quarter of the private schools in Lagos, again in a time of rapid expansion, were thirteen years old or more.
4. Srivastava, in her study of ten randomly selected recognised private unaided schools, finds the average age of her schools is around eleven years (using her Table 1, 2008b: 455). This study is also from Uttar Pradesh, so it is remarkable that the Rigorous Review highlighted the passing remark of Härmä and Rose (2012) about schools closing while missing this other work from the same state that explicitly tabulates evidence about school longevity.
5. Schirmer (2010: 49) is also positive: even during a time of rapid acceleration of growth of private schools, ‘75 per cent of the schools had been around for ten years or more. Most or all of these schools are therefore well-established, and have been growing ‘taller and fatter’ as their reputations have spread and demands for their services have increased.’
6. Ohba gives the foundation year of the twelve private schools in the Kenya study (2012: 771, Table 1). These are very old schools in general (the modal date was 1982). On average, the age of the schools was just under twenty years (assuming the data was collected in 2011).
7. Kremer and Muralidharan (2009: 91) report that more than 50 per cent of the rural private schools in their sample were more than five years old, again in what they suggest is the ‘rapid expansion of private schooling’.

In other words, studies that are within the Rigorous Review’s dataset appear to offer evidence to contradict its counter-intuitive ‘negative’ finding. Using the proxy of length of operation of private schools, there is strong evidence that private schools have remarkable longevity, particularly given the huge expansion of the sector in recent years: this implies that they are likely to be financially sustainable.
studies that are within the Rigorous Review’s dataset appear to offer evidence to contradict its counter-intuitive ‘negative’ finding.

Revised finding

There are now (at least) nine studies available, as follows: positive (eight), neutral (zero), negative (one). The overall strength of evidence moves now to be well supported (rather than weak), given the increased number of studies.

Additional circumstantial evidence

Even stronger circumstantial evidence can surely be found by reflecting on the huge numbers of private schools present in the market. It is often said that there are between 300,000 to 400,000 low-cost private schools in India (see, e.g., Garg 2011); Härnä and Adefisayo report over 12,000 private schools in Lagos State alone (2013: 133), with around three-quarters (the unapproved schools) likely to be low-cost. Nishimura and Yamano (2013) report a dramatic growth to somewhere near 8,000 private schools (reading from their graph on p. 268) by 2007, while Aslam reports over 24,000 private schools in Punjab alone, even as far back as 2001 (2009: 333). So many entrepreneurs, especially from poor communities, are not likely to be in this market unless they have good reason to believe that schools are financially sustainable. These extraordinary numbers should also count as additional proxy evidence for this assumption.

SUMMARY: PRIVATE SCHOOLS ARE MORE COST-EFFECTIVE THAN GOVERNMENT SCHOOLS AND ARE FINANCIALLY SUSTAINABLE

Private schools, the evidence shows, have lower cost of education delivery than government schools; in combination with their higher quality levels, this would suggest greater cost-effectiveness. Using the proxy measure of length of operation of private schools, private schools are very clearly financially sustainable. Even stronger circumstantial evidence comes from the vast number of private schools: so many educational entrepreneurs would not be entering these markets if they did not believe the schools to be financially sustainable.
8. AFFORDABILITY

The fourth hypothesis of the Rigorous Review is ‘Private schools are affordable to the poor and the poorest’ (p. 27). Two testable assumptions follow.

**ASSUMPTION 7: THE POOR AND POOREST ARE ABLE TO PAY PRIVATE SCHOOL FEES**

**Initial finding**
The Rigorous Review reports thirteen studies falling under this category, as follows: positive (zero), neutral (eight), negative (five). In the text, however, the team actually summarise six studies as negative, as Akaguri (2013) is included as both neutral and negative evidence.

The headline finding is that ‘The evidence on whether the poor are able to pay private school fees is ambiguous. Most is neutral, some is negative but there is no positive evidence’ (Rigorous Review, p. 27). The overall strength of evidence here is weak, by definition, because the majority of studies are ‘neutral’ in outcome.

**What does this assumption mean and how is it to be tested?**
Affordability is a hugely important area, one closely linked to discussions of equity. As the issues need to be discussed in some depth, it may be worth outlining at the outset what we believe are some core propositions:

- Some of the poorest families are sending their children to private schools.
- Some of the poorest who would like to send their children to private schools are not, for financial reasons.
- Moving up income or wealth quintiles brings increasing proportions using private schools, presumably in part for financial reasons.
- Published ways of looking at affordability of private education by investigating average school fees and average household income/assets, etc., may not be the most helpful ways of looking at affordability of private education for the poorest.

We suggest that the Rigorous Review is in agreement with the first three propositions, while the fourth is a challenge to some of the methods used. (The evidence for these suggestions is found below.)

**What conclusions follow?** It partly depends on what you are trying to test with the assumption, in the context of the overriding research question, ‘Can [low-cost] private schools improve education for [poor] children in developing countries?’

Are we assuming that (a) low-cost private schools have to do all the heavy lifting by themselves, and even that they have done so already? Or that (b) low-cost private schools
need to show the way, and others (e.g., philanthropists, business enterprises, international agencies, governments, etc.) can come alongside them, to ensure that all of the poor and poorest can afford them?

If we go along with (a), then we would want to find private schools serving some large proportion of the poor and the poorest already for the evidence to be positive for Assumption 7. However, if we go along with (b), then we would only want to find that private-school fee levels are affordable for some of the poor and poorest now.

To explain further, if private schools were found not to be affordable by any of the poor or poorest now, then clearly what is provided is too expensive (or not a priority for expenditure by the poor and poorest), so they could be ruled out as a viable solution for them. In which case, private schools could not improve education for the poorest children in developing countries.

However, if some proportion of the poor and poorest is able to afford private schools now, then this means something quite remarkable: some private schools have managed to find ways of bringing their costs low enough to serve the poorest. Having shown the way, others (such as readers of the Rigorous Review) can come alongside and help ensure that even greater proportions of the poor and poorest are able to afford private schools. This could be through a variety of methods: focusing on demand (e.g., targeted vouchers, unconditional or conditional cash transfers, research on how poorest families are managing to use private schools, etc.) or supply (e.g., exploring efficient business practices in private schools to enable them to lower their costs even further, or to enable parents to pay for costs over the whole year rather than up front). In other words, finding the evidence positive for this assumption would certainly not be the end of the discussion – there is still plenty to be done.

Which is the most sensible approach? We suggest it is (b). Public schooling has had huge resources ploughed into it from governments and international agencies over decades; low-cost private schooling, by contrast, is a grassroots initiative that depends on resources marshalled from within poor communities themselves. On the one hand, to find that private schools have been able to create opportunities at a low enough price to be affordable by (some of) the poor and poorest already is quite remarkable. But, conversely, to expect this initiative of the poor to have been able to solve the problems of all of the poor (e.g., universal access) without any outside assistance might be expecting too much.

In what follows, our preferred interpretation of Assumption 7 is along the lines of (b). Finding that some proportions of the poorest or most disadvantaged are able to afford private schools should, we believe, be taken as positive evidence. However, in what follows we will be a little flexible in our interpretation, for reasons given.

We now turn to the approaches used in the Rigorous Review, to see how they fit into this outline discussion. The Rigorous Review appears to investigate the evidence in the following three main ways.
First, there are studies that report the proportion of children from low-income families attending private schools, where positive evidence appears to be that ‘Those in the poorest quintile are willing and able to pay for private schools’ (Rigorous Review, p. 66). However, while no proportion of those in the poorest quintile is given as to what counts as positive, neutral or negative, the suggestion from the Rigorous Review’s discussion is that 10 per cent of the poorest quintile accessing private schools is neutral evidence.

Given our discussion above, our preferred approach would be to disagree: 10 per cent of the poorest quintile accessing private schools suggests that some of the private schools are affordable to the poorest and should therefore be taken as remarkable and positive evidence for the affordable reach of low-cost private schools. However, in the interests of finding at least some common ground with the Rigorous Review in this area, we will go along with this assumption for the four pieces of evidence they use that make these kinds of quantitative statements. We will in addition add — arbitrarily but hopefully within the spirit of the Rigorous Review’s discussion — that a figure of 20 per cent of the poorest quintile accessing private schools should then be taken as positive evidence for the assumption, while less than 10 per cent would be negative.

Second, indications of unaffordability have been taken from qualitative research that found parents expressing a preference of private school for their children but instead enrolling them in a government school. These could suggest that, for these families, private schools are unaffordable. The Rigorous Review appears to take any ‘mismatch between school preference and actual enrolment’ (p. 28) as negative evidence. We shall question this below, as it seems to be inconsistent with the other ways of looking at the evidence and with our preferred approach.

The third approach to affordability investigates studies that compare the average fees of private schools with average household income quintiles. These studies, all given as negative evidence, have average private-school fees (for two children) ranging between 25.6 and 30 per cent of an average household income in the poorest quintile, and in another study 29.8 per cent of average household income in the poorest quintile for total schooling costs of one child (Rigorous Review, p. 29). No other indications are given as to what might count as positive or neutral evidence. We will offer detailed counter-arguments to show why we do not believe this approach to affordability is very helpful.

There is also a fourth possibility, apparently not considered by the Rigorous Review with regard to affordability. Sending a child to government school, though nominally ‘fee-free’, can still be expensive for poor families, given the costs of uniform, travel and school-based levies. Where evidence shows that both government and private schools are unaffordable to the poorest, then we do not believe that this should be taken as negative evidence for private schools; instead, we take such evidence as neutral.

Finally, four studies reviewed do not neatly fit into the categories above (Baird 2009; Phillips and Stambach 2008; Tooley et al. 2008, 2011).
For these two studies, assuming ‘first generation learners’ are to be classified as among the most disadvantaged in the sample, then the first article should be moved to positive evidence in our interpretation of Assumption 7, while the second, if we take the evidence at face value, would remain neutral (at least for Pakistan). However, perhaps taking the evidence ‘at face value’ is something that a rigorous literature review should not be doing. Let us examine this in the context of the next article given as neutral evidence.

Härmä and Rose (2012) find ‘that only 10 percent of children from the poorest quintile were accessing private schools in their study area in India (compared to 70 percent of the richest quintile)’ (Rigorous Review, p. 28). This figure can indeed be read off from the graph in Härmä and Rose (2012: 251). But this does not need to be taken at face value as the authors explicitly point to ‘the model … described in detail in Härmä 2011’ (p. 251). In that model, we find that Härmä collected household data and used these to create income and wealth indices for her study villages. Crucially, as she rightly points out, this means that her quintiles of wealth and income reflect the relative positions of families in her sampled villages: ‘quintile five represents the rich-est families in the sample’ (Härmä 2011: 354, emphasis added); they are ‘relatively well-off’ (p. 354, emphasis added). So the study finds that 10 per cent of the poorest quintile of a “typical” rural UP [Uttar Pradesh] village’ attends low-cost private schools.

To put Härmä’s work into context, we need to know what the income or wealth quintiles are for India, not just for her remote villages.

Kremer and Muralidharan (2009) is reported as finding that ‘while private schools mainly cater to the better-off in rural areas, many children within them come from the more disadvantaged backgrounds’ (Rigorous Review, p. 28). Indeed, the survey found, for example, that ‘20 per cent of the private school students are first generation learners, which while lower than the 30 per cent in public schools, is still quite significant. Thus … many of their students come from disadvantaged backgrounds’ (Kremer and Muralidharan 2009: 14).

Heyneman and Stern (2013) ‘cite private school enrolment rates of between 10–11 percent of students from the two lowest economic quintiles in Jamaica, and 10 percent of the poorest households in Pakistan’ (Rigorous Review, p. 28). The study did indeed report these figures, but there is no discussion about them nor any explanation of where they were obtained. The purpose of the article was, as the title, ‘Low Cost Private Schools for the Poor: What Public Policy Is Appropriate?’, suggests, to explore appropriate public policy not to report details of data.

All are included by the Rigorous Review as neutral evidence; they are discussed at the end of this section.

Neutral evidence: studies reporting proportion of poor children attending private schools

Two of the studies are cited in the text as giving precise figures which we can interpret in the light of the above discussion, while a further study also gives precise figures (not cited in the text) which we can use here.
Usefully, an all-India study of household income (Vanneman and Dubey 2010) was conducted using data collected in 2005, around the same time as Härmä’s fieldwork. It presents deciles of household income against which we can approximately fit Härmä’s quintiles. Their findings are shown in the first three columns of Table 8.1. The last two columns fit Härmä’s income quintiles for her thirteen villages in Uttar Pradesh into these all-India categories. What we see is dramatic: all but one of her quintiles fit into the poor or very poor categories! Her bottom two quintiles are ‘very poor’, by Indian standards, while her third and fourth relatively wealthy quintiles are in fact still ‘poor’ by Indian standards. Only her very richest quintile is ‘rich’ by Indian standards, fitting into the eighth decile of household income.

This is what might be expected, given Härmä’s description of her study villages: they are in ‘a remote, rural area’, where the vast majority of villagers are ‘farmers or landless day labourers’ (2011: 351); ‘only the most well-off’ own ‘even carts’ (Härmä 2009: 152). In another of her articles using the same data, we are also told that ‘48 per cent percent of sampled families own no land at all’ (Härmä 2010: 8). The infrastructure is terrible, ‘the roads are mostly rutted dirt tracks with deep holes’, electricity supply ‘is rare and extremely erratic’. There are no clinics. ‘Indeed when viewed from outside, most village households may appear poor’ (Härmä 2010: 38, emphasis added).

But this puts her findings in a completely different light from that suggested by the Rigorous Review. Reading again from the graphs in Härmä and Rose (2012) (on ‘asset index quintiles’) or from Härmä (2009: 161–2) (three graphs showing quintiles of equivalised income, asset index scores and standard of living index scores), it is evident that a much larger proportion of the poorest groups (in each category) are in fact already using private schools. For instance, for ‘asset index scores’, 10 per cent of the lowest quintile but 30 per cent of the second lowest are using low-cost private schools.

Table 8.1 Deciles and quintiles of wealth, India.

<table>
<thead>
<tr>
<th>By decile (All India) (Vanneman and Dubey 2010)</th>
<th>Descriptor</th>
<th>Mean household income (All India)</th>
<th>Mean household income (13 villages Uttar Pradesh)</th>
<th>By quintile (Härmä)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Decile 1</td>
<td>Very poor</td>
<td>2,854</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decile 2</td>
<td>Poor</td>
<td>10,701</td>
<td>7,049</td>
<td>Quintile 1</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>9,802</td>
<td>Quintile 2</td>
</tr>
<tr>
<td>Decile 3</td>
<td>Average</td>
<td>15,197</td>
<td>13,404</td>
<td>Quintile 3</td>
</tr>
<tr>
<td>Decile 4</td>
<td></td>
<td>19,709</td>
<td>18,797</td>
<td>Quintile 4</td>
</tr>
<tr>
<td>Decile 5</td>
<td>Rich</td>
<td>24,791</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decile 6</td>
<td></td>
<td>31,914</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decile 7</td>
<td>Very rich</td>
<td>41,966</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decile 8</td>
<td></td>
<td>56,871</td>
<td>52,490</td>
<td>Quintile 5</td>
</tr>
<tr>
<td>Decile 9</td>
<td></td>
<td>83,175</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decile 10</td>
<td></td>
<td>192,384</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Both are likely to be classified as the poorest families. Moreover, 47 per cent of the third, and 56 per cent of the fourth quintile are also using low-cost private schools. These are the poor in India. Similarly, for income, just over 10 per cent (lowest quintile) and around 20 per cent (second lowest quintile) are using private schools. Again, these should be classified as the poorest families. Meanwhile, more than 50 per cent in both the third and fourth quintiles are using private schools – these should be classified as the poor.

Exploring Härmä and Rose (2012) in detail leads to the conclusion that there are much higher percentages of the poorest (and poor) using low-cost private schools than the 10 per cent given in the Rigorous Review. Given our discussion above, this evidence is positive, not neutral.

Negative evidence

There are five pieces of negative evidence given, the first three of which seem to be related to the ‘mismatch’ approach to affordability, while the remaining relate to the concern that ‘parents’ ability to pay is unsustainable, or increases household poverty’ (p. 29).

First, the ‘mismatch’ studies. ‘In Singh and Sarkar’s (2012) study in Andhra Pradesh, parents with children in government schools expressed helplessness in not being able to afford to send their children to private schools, which continued to serve the large majority of economically marginalised children’ (Rigorous Review, p. 28). A footnote is given to flesh this out: ‘Over 70 percent of the students enrolled in government schools belonged to households from the bottom two quartiles’ (p. 28, footnote 14).

It is worth mentioning in passing the almost taken-for-granted notion in many of the studies that government schools are so bad that some parents express ‘helplessness’ if they have to send their children to them. This highlights parents’ preferences for private schools, something discussed further below. But why is this negative evidence? The study itself says, ‘It is interesting to note that choice of private schools is not limited only to more affluent families’ (Singh and Sarkar 2012: 11). Their table shows that, for eight-year-olds in 2009, 44.1 per cent of the total children enrolled in school were in private school, including 31.3 per cent of rural, 29.3 per cent of SCs, 21.7 per cent of STs and 44.2 per cent of ‘other backward Castes’. These are likely to be considered some of the most ‘disadvantaged’ groups in India. On this evidence, it is difficult to see why this is included as negative. Even though it may be true that some parents who prefer private schools are unable to afford them, substantial minorities of the most disadvantaged are able to afford them. Given our discussion above, as each of these categories is above the 20 per cent cut-off mark, it is suggested this evidence is taken as positive for this assumption.

Schirmer (2010) and Fennell (2012) are also cited as negative evidence concerning the affordability of private schools. Schirmer, it is said, ‘concludes similarly’ to Singh and Sarkar

1. Assuming that the asset index quintiles are somewhat similar to the equivalised income quintiles discussed.
2. The Rigorous Review has Fennell 2013, but this is an error.
quality government alternatives, only 41 percent of the children in the sample were actually attending private schools’ (Rigorous Review, p. 28).

Now, while these Härmä articles report the same study from rural Uttar Pradesh, they do appear to report different aspects of the data, so it may be legitimate to include these as separate studies here (unlike in the gender assumption above, where it was simply the same evidence being reported for both studies, hence we took the two studies as one). However, as both refer to the same dataset, we can obviously relate their findings to each other, as we do below.

We will try to classify this study using the criteria we accepted above. The key for our classification is not that 41 percent of the children in the sample attend private schools but what proportion of the poorest or most disadvantaged groups attend private school. Usefully, Härmä (2009) does give more detail, in addition to the wealth and income quintiles already examined. She is explicitly using membership of SC or minority religion (predominantly Islam) as one measure of deprivation (p. 158). Her figures show that for these most deprived groups, 23 percent of SCs and 31 percent of Muslims are using low-cost private schools (p. 160). In other words, far from being negative, this shows Härmä’s dataset giving positive evidence for the affordability of private schools, even by some (but certainly not all) in the poorest and most disadvantaged groups.

What of the use of Härmä’s dataset as negative evidence concerning the affordability of school fees by the poorest households?
‘Härmä’s (2009) research in India … finds that the percentage of the average household income required to access an LFP for an average-sized family in the poorest quintile is 30 percent for unrecognised and 25.6 percent for recognised LFPs, compared to 3.9 percent for government schools’ (Rigorous Review, p. 29).

We shall have more to say about the comparisons with government schools when we consider Assumption 8 below, as Härmä (2009) is also cited as negative evidence for that assumption, using these same figures. But now we can remind ourselves again that all but one of Härmä’s quintiles are ‘poor’ and ‘poorest’ by Indian standards, so the figures given by the Rigorous Review (for the poorest quintile only in Härmä’s sample) will significantly overestimate the percentage of average costs required by the poorest families in India.

Moreover, Härmä also finds that ‘parents and headteachers reported a “three for the price of two” policy on the monthly tuition fee across all LFPs’ (2009: 163), which suggests further price reductions for some of the poorest families. Although not included as evidence for this assumption by the rigorous review, Srivastava (2008b) brings in additional useful insights here. She notes:

Effectively, the school-set tuition fees acted as guide prices and represented the maximum amount that a case study school could charge. Many parents employed the ‘fee-bargaining strategy’ and negotiated a lower amount …, thus not paying the full fees. Furthermore, fee concessions for families with multiple children enrolled or those that could not afford the set fee were internally instituted by owners.³

(Srivastava 2008b: 454, emphases added)

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(Srivastava 2008b: 454, emphases added)

3. Parents had this power because of the ‘schools’ interests in retaining clients’ (Srivastava 2008b: 454).
All of this shows the flexibility of private-school costs to poor parents – and makes us realise that the headline fees given by private schools should be taken as ‘upper bounds’ rather than as the fees paid by all parents. So, in Härmä’s study, these poorest parents, as well as choosing the most affordable schools, may also not pay full fees in those schools. Further research is clearly necessary to reconcile the figure of 10 per cent of the poorest of the poorest attending private schools with the figure of 30 per cent of family income needed for an average family in the lowest quintile to send their children to an average private school.

We can summarise this discussion as follows. For this evidence, the question that we need to answer is not ‘Can a poor or poorest family afford the average costs that are incurred in sending a child to a private school?’ but rather ‘Are there private schools that are financially accessible to the poor and the poorest?’ That 10 per cent of the poorest (of the poorest) are sending their children to a private school appears to be the clearest answer to the question. In other words, we suggest that this evidence from Härmä (2009) should not be used to challenge the finding we have already deduced from Härmä (2011) as above, using the same dataset, but should again be seen as positive evidence.

Akaguri (2013) is also used as negative evidence. ‘This finds that enrolment of just one child in an LFP by a household in the poorest quintile would require about a third (29.8 per cent) of its income’ (Rigorous Review, p. 29). Four points can be made about this. First, the discussion about average fees and average family income above of course applies here too, especially as Akaguri points out that ‘spending on private education rises substantially with income’ (2013: 143).

Second, we note that Akaguri is using ‘criteria suggested by Lewin (2007)’, that ‘no more than 10 per cent’ of a poor household’s income should be ‘expended on one child’s education’ (2013: 154–5). Given this, then clearly the 29.8 per cent figure quoted by the Rigorous Review is indeed unaffordable by those in Akaguri’s poorest quintile. However, his figures clearly demonstrate that public education is also beyond the financial means of the poorest. Akaguri suggests that the cost of sending a child to public school is roughly 53 per cent of the cost of sending a child to private school (see below). This would mean that to send one child to government school would take up 15.7 per cent of the mean household income of the poorest families, which is also unaffordable by Akaguri’s criterion. Indeed, Akaguri himself writes, ‘Even some fee-free public provision may not be sustainable among the poorest’ (2013: 159).

From this perspective, we suggest that Akaguri’s work should be taken as neutral, not negative evidence.

4. We hesitated about critiquing this article, given the author’s tragic and untimely death in 2012. As those who made the final edits note the ‘spirit’ of Akaguri’s work and his ‘concerns that low-fee privately financed schools would never meet the needs of the poorest’ (Akaguri 2013: 159), and that one of us did engage in fruitful discussions with him, we felt it more respectful to his spirit and concerns to engage with his ideas again rather than let his article pass. We hope readers concur.
Third, we must note that Akaguri was researching in a very poor area, where ‘[a]bout 60 per cent of its inhabitants live on less than one dollar a day’ (2013: 145). Exactly parallel arguments to those made above about Härmä’s work seem applicable here. In his Table 1, the author quotes the national annual quintile values for 2005 (Ghana Statistical Service 2008), while his Table 6 gives the annual household income from his study. When the national values are adjusted for inflation (only), we see (Table 8.2 below) that all but the highest quintile in the study are well below the mean of the lowest national quintile. So, all but those in Akaguri’s highest quintile would be poorest or poor in Ghanaian terms. Clearly those in his higher quintiles (still poorest or poor by Ghanaian standards) require considerably lower percentages of their income to be spent on private education than the figures given in the Rigorous Review.

For instance, using the figures given by Akaguri, those in the third quintile would require 12 per cent of their household income to send a child to private school, while those in the fourth (still very poor by Ghanaian standards) would require only 7 per cent. That is, using Akaguri’s criteria, private schooling is indeed affordable to at least some proportion of those in the lowest income quintile in Ghana. From this perspective, it would seem that this evidence should be included as positive evidence for the affordability of private schools.

However, fourth, there appear to be problems with Akaguri’s calculations. The school fees he uses (from his Table 4) are actually given as per term but they are used in his calculations as if they were annual fees. If instead we substitute the corrected annual fees, then schooling is pretty much unaffordable to everyone, apart from those in the richest quintile, even sending

### Table 8.2 Mean household income by quintile, Ghana.

<table>
<thead>
<tr>
<th>Quintile</th>
<th>National Mean</th>
<th>Akaguri (data collected in 2008)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Quintile 1</td>
<td>728 (Ghana, 2005)</td>
<td>208.02 (Quintile 1)</td>
</tr>
<tr>
<td>Quintile 2</td>
<td>1,020</td>
<td>355.24 (Quintile 2)</td>
</tr>
<tr>
<td>Quintile 3</td>
<td>1,098</td>
<td>518.74 (Quintile 3)</td>
</tr>
<tr>
<td>Quintile 4</td>
<td>1,263</td>
<td>875.17 (Quintile 4)</td>
</tr>
<tr>
<td>Quintile 5</td>
<td>1,544</td>
<td>1,909.04 (Quintile 5)</td>
</tr>
</tbody>
</table>
to government school. Our Table 8.3 shows, using Akaguri’s preferred 10 per cent figure spent on one child’s education, only those in the very highest income quintile can afford either government or private schooling for their children. For everyone else, government schooling is as unaffordable as private schooling. This would again make Akaguri’s evidence neutral.

Perhaps it might be thought that the household incomes given in Akaguri’s Table 6 should be per term too rather than per annum. But then the families appear too rich by Ghanaian standards. In any case, if this is where the mistake lies, then the discussion above under our first point is valid – which shows that while private education may be unaffordable by the poorest, so is government schooling – so, again, a neutral finding.

Overall, Akaguri’s evidence is either neutral or positive for the assumption, certainly not negative. Finally, Akaguri (2013) is also used by the Rigorous Review for an additional point: ‘Based on interviews with a small sub-sample of LFP drop-outs, the study finds that over half stayed away because of fee arrears, and a significant share had been suspended or punished for non-payment of fees’ (Rigorous Review, p. 29). Moreover, ‘Akaguri (2013) adds another cautionary note, finding that while children from the lowest quintiles did enrol in LFPs in rural Ghana, they were also the most likely to drop out’ (Rigorous Review, p. 28). The number of drop-outs was small – in the largest school there was only one. But only eight interviews were conducted with drop-outs, and these were opportunistically selected, so impossible to generalise from. Moreover, the study did not report that these drop-outs were more likely to be from the lowest quintiles.

**Other neutral evidence**

Finally, we turn to the four other studies that do not neatly fit into the categories discussed above.

Baird (2009) clearly indicates that the schools in his study are serving the ‘poorest’, a point acknowledged in the Rigorous Review: his work, a ‘nationally representative analysis of rural and urban India indicates that unrecognised schools do, in some cases, serve the poorest of the poor’ (Rigorous Review, p. 28). In the absence of quantitative evidence, as

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**Table 8.3 Schooling costs per term, per year and by income quintile (data from Akaguri 2013).**

<table>
<thead>
<tr>
<th>Schooling costs (GHS)</th>
<th>Per term</th>
<th>Per annum</th>
</tr>
</thead>
<tbody>
<tr>
<td>Public school</td>
<td>32.7</td>
<td>98.1</td>
</tr>
<tr>
<td>Private school</td>
<td>62.06</td>
<td>186.18</td>
</tr>
<tr>
<td>Income quintiles</td>
<td>Q1</td>
<td>Q2</td>
</tr>
<tr>
<td>Mean income (GHS)</td>
<td>208.02</td>
<td>355.24</td>
</tr>
<tr>
<td>Percentage of family income</td>
<td></td>
<td></td>
</tr>
<tr>
<td>for one child in public school</td>
<td>47%</td>
<td>28%</td>
</tr>
<tr>
<td>for one child in private school</td>
<td>90%</td>
<td>52%</td>
</tr>
</tbody>
</table>
discussed above, we suggest including this as positive evidence for some at least of the poor being able to afford private school fees.

Phillips and Stambach (2008) is said to give neutral evidence for this assumption, apparently raising ‘the issue of the political economy of assisted spaces’ (Rigorous Review, p. 28). The study is interesting in exploring the strategies and techniques that parents use to access schools. Parents undertake complex financial and social negotiations with their families, friends and benefactors as well as with the schools to get their children educated. But this study sheds further light on the problems researchers encounter when they try to understand, in financial terms only, how the poorest manage to access schools that charge fees. This study appears to suggest that there are a multitude of social and relational methods used by parents who are accessing schools (private and government) to enable them to do so. This appears to be positive evidence showing that poor parents have methods by which some manage to afford (private) schools.

Tooley et al. (2008) and Tooley et al. (2011) are used to provide ‘different … explanations for affordability’ (Rigorous Review, p. 28), in particular that ‘not all children enrolled in LFPs were paying fees, including orphans and children from disadvantaged backgrounds that were given fee reductions or allowed to attend for free’ (p. 28).

In fact, Tooley et al. (2008) say that they did not (unfortunately) measure the percentage of children enrolled on these concessionary and free places, although they acknowledged their existence, and pointed to the team’s earlier studies which showed these places making up between 5 and 18 per cent of all places (p. 455). While we acknowledge the potential problems of such a calculation using mean values, the study does, however, explore affordability in that way. Curiously, this was missed by the Rigorous Review. The study showed that in the private schools studied in a slum of Nairobi, the mean fees per child for different classes ranged ‘from 4.7 per cent to 8.1 per cent of [the] “absolute poverty line” income level’ for Kenya (Tooley et al. 2008: 454). This would seem to suggest that these private schools would be affordable even to those on the absolute poverty line, so could be taken as positive evidence.

Tooley et al. (2011) also only pointed to this finding about concessionary and fee places from earlier studies; there is no explicit discussion on affordability given in this paper. We suggest that this study is removed from the evidence for this assumption.

Revised finding

It has been tricky putting evidence into the categories of positive, neutral and negative, given the lack of explicit guidance offered by the Rigorous Review; our suggestions are given in Table 8.4. Overall, we now suggest the following: positive (ten or eleven), neutral (two or one), negative (zero). This now positively supports the assumption that (at least some of) the poor and the poorest are able to pay private school fees; the overall strength of evidence is now ‘strong’.
The role and impact of private schools in developing countries

Assumption 8: Private schools are as affordable to users as state schools

Initial finding
The Rigorous Review has five studies, as follows: positive (zero), neutral (zero), negative (five).

The headline finding is that ‘The small body of evidence consistently indicates’ that low-cost private schools ‘are considerably more expensive than state schools, both in terms of the school fees and of hidden costs such as uniforms and books’ (Rigorous Review, p. 29). The overall strength of the evidence is ‘weak’ by definition because of the small number of studies (Rigorous Review, p. 12) but is negative towards private schools.

Straw-man assumption
Recall that the assumptions were created by the Rigorous Review team based on their initial ‘rapid appraisal of policy debates and research findings’ of their inception phase (Rigorous Review, p. 6). It is our contention that no sensible reading of the policy debates or

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<th>Assumption 7 (original)</th>
<th>Positive</th>
<th>Neutral</th>
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<tbody>
<tr>
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<td>8</td>
<td>6 (in the Rigorous Review text)</td>
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<th>Assumption 7 (revisited)</th>
<th>Positive</th>
<th>Neutral</th>
<th>Negative</th>
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</thead>
<tbody>
<tr>
<td>Number of studies</td>
<td>10 or 11</td>
<td>2 or 1</td>
<td>0</td>
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research findings, however rapid, could come up with such an assumption. Unless one was an extreme libertarian who believed that 100 per cent of government subsidies were always misdirected, all of the time, the target embodied in this assumption seems set unfairly high – surely there is likely to be some cost saving to parents who send a child to a (heavily subsidised) government school rather than to a (no subsidy) private school? In other words, this appears to be unequivocally a straw-man assumption.

As it stands, presumably the criteria for deciding evidence was positive would be exact parity of fees and other costs in private and government schools. Negative evidence would be any deviation from that. It is not clear what neutral evidence would be in this regard.

We think this bar is set far too high and instead suggest that a more qualified assumption such as the following should have emerged from an initial reading of the evidence, at least from those not deliberately seeking to undermine the role of private schools:

**Assumption 8**: Private schools are nearly as affordable to users as government schools

We can easily quantify evidence for this assumption. For instance, if the total costs of sending a child to a government school were, say, 75 per cent or more of sending a child to private school, then this would count as positive; 50–74 per cent would be neutral, and below this would be negative evidence. If even this revised assumption was positively satisfied, it would still be a remarkable finding: sending a child to a low-cost private school, without any subsidies, was nearly as affordable to parents as the heavily subsidised government schools.

Again, as in the discussion earlier, we also suggest that if government schools are also unaffordable for the poorest, then this should not be used as negative evidence concerning the private schools. Instead, we suggest that such evidence should be counted as neutral. Let us review the evidence with both these assumptions in mind. Of the five pieces of evidence, two are major, while the other three are minor.

**Negative evidence: major studies**

There are two studies to be examined under this heading, by Akaguri (2013) and Härmä (2009).

‘The differential in total costs to households between public and private schools varies, but in some cases it is substantial. Among the sample of seven rural Ghanaian schools investigated by Akaguri (2013), the cost differential was approximately 40 percent’ (Rigorous Review, p. 29). Akaguri found that the cost of sending a child to government school was 53 per cent of the cost of sending a child to private school – which, by our classification above, would make this study neutral not negative evidence. However, a detailed examination of his findings reveals an even more optimistic figure for private schools.
Akaguri gives a useful table comparing direct household expenditures per child per term by school type (2013: 151, Table 4). These figures are given in the first four columns of Table 8.5 below. Here we can see that there are large differences between the amounts spent between public and private schools on identical items. Particularly apparent are the differences in costs on transport, extra classes and school meals. Regarding transport, this data was obtained from the schools (e.g., ‘none of the public schools under study reported transport costs’, Akaguri 2013: 149). However, in an earlier summary of the same data, it is noted that some children at public schools did commute: ‘The proximity of households to public schools was a major factor for not incurring transport costs, although the study found that some children were commuting daily to school’ (Akaguri and Akyeampong 2010: 2, emphasis added). So it seems possible that the difference between figures here is because the public-school head teachers did not know how much was spent on transport, whereas the private-school head teachers did.

For ‘extra classes’ and ‘school meals’, although the figures cannot be faulted, they do not provide the most useful way of answering our question, ‘Are low-cost private schools (nearly) as affordable to users as state schools?’ A poor family faced with the choice of private or public school does not have to spend the same amount of money on ‘extra classes’ as existing families who use private schools.

Similarly, unless school meals are compulsory for all attending the private school, a family could opt to spend less on lunch than those families currently using private schools. All this impacts on the potential affordability of private schools.

The right-hand columns have been created in Table 8.5 to explore these issues. These estimate what a poor family could afford if they had a choice of public and private schools within walking or cycling distance (so zero transport costs). How much would this poor family have to spend to get, prima facie, the same schooling in public or private school?

Assuming that a child spends the same on transport, food and extra classes for both public and private, the relative cost of sending a child to government school is 77 per cent of the cost of sending a child to private school, rather than the 53 per cent suggested by Akaguri. By the strict criteria of the original Assumption 8, this is still negative evidence, of course. But it is decidedly positive with regard to the (more realistic) Assumption 8*.

The second major study is from Härmä: ‘Härmä (2009) finds that among a sample of 16 LFP schools in India, the average full cost (including all other fees) of sending a child to a private school was approximately nine times as much as the cost of a government school’ (Rigorous Review, p. 29, emphasis added). In fact, Härmä (2009) shows that sending a child

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5. It is not quite clear where these figures come from. While many work out to be exactly the averages given for the seven schools in his Table 3, others are different. The averages in Table 3, however, are more or less identical to the figures given in the earlier summary, Akaguri and Akyeampong (2010).

6. In this case, Akaguri does suggest that in the four private schools under investigation, extra tuition and lunch were compulsory (2013: 151). This is unusual in the Ghanaian context.
to a recognised private school costs exactly nine times as much as sending a child to a government school (Rs. 1,322 per annum compared to Rs. 148 per annum) and eight times more in an unrecognised private school (at Rs. 1,121 per annum).

However, just as for Akaguri, we can see that like is not being compared with like: the private school figures include uniform, whereas the government figures do not (see Härmä and Rose 2012: 253 and Härmä 2010: Tables 19 and 20). This is quite a substantial part of the private-school budget, a mean of around Rs. 200 per annum, or nearly one-fifth of the total cost for unrecognised schools. Given that we are also told that the government is supposed to provide free uniforms for girls but that these are ‘virtually never delivered’ (Härmä 2009: 157), then the assumption is that uniform is required in government schools too, for boys (who never get it free in any case) as well as girls. So it probably should be added to government costs. Making the numbers fair in this way would increase the government school total spend per annum to around Rs. 350. Now the average private-school multiple is reduced to three times, down from the eight or nine times given earlier. Recall that for a family choosing not an ‘average’ cost but the lowest-cost private school, this multiple is potentially reduced further.

This is still negative evidence as far as Assumptions 8 and 8* are concerned. But it is not quite so bleak a picture as that painted by the Rigorous Review.
Negative evidence: minor studies

Three other studies are given as negative evidence for this assumption. ‘In the case of India, Siddhu (2011) and Sucharita (2013) find similar results in Uttar Pradesh and Andhra Pradesh, respectively’ (Rigorous Review, p. 29). Moreover, ‘In Dimla, Bangladesh, Sommers (2013) finds that government schools charge less for books and uniforms than private tuition-charging schools’ (Rigorous Review, p. 29).

The similar results in the first two cases refer to the ‘nine times more expensive’ finding of Härmä. In the study, also from Uttar Pradesh, Siddhu in fact finds something similar to the three times more expensive that we suggested was more appropriate for Härmä’s data, giving us confidence in the working outlined above: Siddhu (2010: 12, Table 3) shows that the cost of sending a child to upper primary school is Rs. 2,307 (private) and Rs. 664 (government), a factor of 3.47 times, while for lower secondary schooling it is Rs. 3,326 (private) and Rs. 1,680 (government-aided), a factor of 1.98 times.

Second, Sucharita (2013) is an ethnographic study in two schools purposively selected, one private, one government, so it would not be able to tell us anything about private versus public schooling generally. The only mention of anything that could relate to the citation by the Rigorous Review is that poorer parents ‘found it difficult to enrol their children in private schools. Their children were studying in government school for the single reason that private schools were unaffordable and education in government schools was free’ (Sucharita 2013: 383, emphasis added).

This simplistic approach does not get us very far: It is precisely because education in government schools is not free that we are investigating Assumption 8 at all. We suggest that is excluded as not relevant to the discussion for Assumption 8*.

Finally, Sommers (2013) did find that private schools have stricter uniform policies and more expensive uniforms than government-funded schools. But this surely is not the study’s most interesting finding. She writes that, in her focus groups, parents said that ‘many find difficulty in meeting all educational expenses even when tuition is free’ (2013: 30). Parents and teachers at government-funded schools reported that ‘many students missed class for want of basic school supplies, such as pens and notebooks’ (p. 31). This reinforces what we found in Akaguri (2013): for the poorest, government schools are also unaffordable. We suggest that this is neutral not negative evidence.

Missing evidence

There are at least two pieces in the Rigorous Review’s accepted literature that could also shed light on this assumption but which unfortunately are not included.

Ohba gives evidence on fees in Kenya (2012: 771, Table 1). Averages are not given, but by calculating these we see that, for primary grade 8 (the only grade with data enabling comparisons), the average fee for private school is KES 4,773, and for government school it is KES 3,875. That is, on average, private schools cost 1.23 times more than government schools (government-school costs are 81 per cent of

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7. It is Siddhu 2010, not 2011.
All this could bring down the costs of private schools to be closer still to the government option.

Heyneman and Stern (2013: 5), comparing costs given for public and private schooling, note that ‘unregistered private schools [are] the more economical alternative’. That is, sending a child to private school can be less expensive than sending a child to public school. This is positive evidence for both Assumptions 8 and 8*.

Revised finding
Table 8.6 shows the revised findings for Assumption 8 – which, not surprisingly, remains negative – and the more realistic Assumption 8*, which is as follows: positive (three), neutral (one), negative (two). This positively shows that private schools are nearly as affordable as government schools; the overall strength of evidence is now ‘well supported’ (‘moderate’), given that there are now six studies, with 50 per cent supporting the assumption.

SUMMARY: PRIVATE SCHOOLS ARE AFFORDABLE TO THE POOR, SOMETIMES NEARLY AS AFFORDABLE AS GOVERNMENT SCHOOLS

Some private schools are affordable to significant minorities of the poorest and most disadvantaged groups in society; findings show anything from 20 per cent to nearly 40 per cent of these groups accessing private schools. Studies that find private schools unaffordable by the very poorest sometimes suggest that public schools are also unaffordable. In some contexts, the total cost to parents of sending a child to private school is surprisingly competitive...
Low-cost private schools have developed something quite remarkable: they have managed to find some ways of bringing their costs down low enough to serve poor communities. They are not serving all of the poor and poorest, of course, but they demonstrate what is possible. It may be up to others, including readers of the Rigorous Review, to explore ways to help those families who wish to send their children to private schools but who presently cannot afford to do so.

Table 8.6 Matrix of research evidence for Assumptions 8 and 8*.

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<thead>
<tr>
<th>Assumption 8 (original)</th>
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<th>Neutral</th>
<th>Negative</th>
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| Number of studies | 0 | 0 | 5 |

<table>
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<tr>
<th>Assumption 8 (revisited)</th>
<th>Positive</th>
<th>Neutral</th>
<th>Negative</th>
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</table>

| Number of studies | 2 | 1 | 4 |

<table>
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<tr>
<th>Assumption 8*</th>
<th>Positive</th>
<th>Neutral</th>
<th>Negative</th>
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| Number of studies | 3 | 1 | 2 |
Hypothesis 5 is ‘Demand for private schools is driven by informed choice and a concern for quality’ (Rigorous Review, p. 30). Two assumptions follow:

1. Assumption 9: Perceived quality of education is a priority for users when choosing private schools. Eleven studies: positive (eight), neutral (three), negative (zero).
2. Assumption 10: Users make informed choices about the quality of education. Seven studies: positive (six), neutral (zero), negative (one).

These are both positive in favour of private schools, and the overall strength of evidence is ‘moderate’. We will not challenge these findings further but will make brief notes about the Rigorous Review’s general approach.

**SUMMARY: PRIVATE SCHOOLS ARE THE PREFERRED OPTION FOR POOR PARENTS**

The headline findings of these two assumptions are that a majority of studies ... indicate that perceived quality of education is a priority for users when choosing between schools, and that private schools are often perceived to be of higher quality than government ones’ (Rigorous Review, p. 30). Moreover, positive support is found for ‘Users make informed choices about the quality of education’ (Rigorous Review, p. 31), where ‘informed choice implies users have adequate information on the performance of schools to be able to judge them. Informal sources including networks of parents were found to play a significant but often under-recognised role in informing users in their choice of school’ (p. 31).

Again, it might be useful to explore the relevance of these assumptions to the overriding research question, ‘Can [low cost] private schools improve education for [poor] children in developing countries?’

The Rigorous Review gives the hint that ‘Underpinning the idea that private schools drive up quality are the concepts of market competition, choice and accountability’ (p. 50). From this they move straight to the assumptions above about ‘informed choice’ and ‘perceived quality’, which seems to suggest that in a market consumers must be informed. And informed choice, they imply, must be concerned with ‘the perception of quality’ and the concomitant ‘dissatisfaction with government schools’.

This may reflect a rather narrow understanding of markets in education. Is it really the role of outsiders to determine how parents should be making choices about education (and judging what constitutes ‘informed’ choice for them)? Education is a contested area, and parents value different things from it, including character education, the education of values, beliefs, attitudes and dispositions, a disciplined and safe
Instead of these two assumptions, we would have preferred a simple assumption along the lines that ‘demand for private education is driven by the choices of the poor’. This is the key: parents are not compelled to use private schools – they are doing so out of choice, sometimes because of positive advantages (such as English language, more attentive and more friendly teachers, etc.) and sometimes because of negative concerns (dire standards in government schools or government schools too far away). Phrasing the assumption in this way would allow us to bring in what we have noted above as a taken-for-granted assumption: private schools are massively preferred by the poor.

We have made notes about this throughout the above discussions. An additional piece of evidence could be Härmä (2011), who found the ‘vast majority of parents indicating a preference for private schools over poor quality government alternatives’ (Rigorous Review, p. 28, emphasis in original). Härmä reports that there is a ‘near universal preference for private schools’ (2011: 353, emphasis in the original), with ‘94.4 percent of sample parents’ preferring private over government school (p. 353). Indeed, in her large-scale household survey, ‘the majority of families (84 percent) view government schools negatively and LFPs positively (77 percent)’ (p. 353).

1. The one study that is given as negative evidence for Assumption 10 would presumably also be negative here.
Hypothesis 6 is that ‘Private schools are accountable to users’ (Rigorous Review, p. 32). Two assumptions follow from this:

1. Assumption 11: Users actively participate in or influence operational decision-making in private schools. Three studies: positive (three), neutral (zero), negative (zero).

2. Assumption 12: Private schools are responsive to users’ demands and complaints. Five studies: positive (five), neutral (zero), negative (zero).

Again, both are positive in favour of private schools, although weakly supported, by definition, given the small numbers of studies. We will not challenge these findings but will make some brief comments about the hypothesis.

SUMMARY: PRIVATE SCHOOLS ARE ACCOUNTABLE

As for choice, we shall explore the two assumptions together. Here, the way in which the assumptions and discussion are made seems to suggest at least partial misunderstanding that there can be different equally valid forms of accountability. (For a discussion of these in connection with low-cost private education, see Tooley 2009.)

According to the Rigorous Review, ‘Accountability implies that users have the ability to influence how a service is provided and participate in decisions’ (p. 32, emphasis added). The first part seems right; the second part, however, is misplaced. Accountability in the market does not mean that consumers have to (or have to have the ability to) ‘participate in decisions’. When we buy a product or service in the marketplace, there is another way that we can make our pleasure or displeasure known to the provider and so ‘influence how a service is provided’ (Rigorous Review, p. 32). The Rigorous Review does note the distinction between ‘exit’ and ‘voice’ and that dissatisfaction with an education provider could be expressed by ‘voting with their feet’ (exit) or ‘making demands or complaints’ (voice). The team also notes that, ‘In principle, exit is a market strategy that depends on choice, while voice is more likely in a public sector setting’ (Rigorous Review, p. 33). This seems more or less correct. Exit is the prime way in which we exercise displeasure in the private sector; and it is the threat of this that keeps producers on their toes and accountable – trying to predict in advance what will keep us as customers rather than taking our business elsewhere.

Having stated this clearly, the Rigorous Review then notes, ‘The hypothesis that private schools are particularly accountable to users is premised on the assumption that they will respond to complaints and strive to meet parental expectations, in order to avoid the implicit or explicit threat of the withdrawal of fees and reputational damage’ (p. 33, emphasis added).
Again, not necessarily: the virtue of the market is that parents do not have to be making complaints in order to keep schools on their toes. As long as there is competition in the market, then the threat of parents taking their custom elsewhere will help ensure that the private proprietors ‘strive to meet parental expectations’ (Rigorous Review, p. 33) without parents having to use their ‘voice’.

In any case, the literature summarised as evidence seems to support the discussion here. It shows how private schools are accountable, in the market sense (see Tooley 2008, 2009), because parents pay fees and have the right to ‘exit’. For example, evidence from South Africa shows that ‘parents felt payment of fees made private schools more accountable to parents’ (Rigorous Review, p. 34, citing Schirmer 2010, emphasis added). Similarly, from Bangladesh, ‘Sommers (2013) attributes fewer teacher absences and more teaching time to [private schools’] awareness of dependence on tuition fees’ (Rigorous Review, p. 34). Finally, experimental evidence from rural Punjab, Pakistan, is taken as showing that ‘the potential (veiled) threat of parents exercising choice is what matters’, making private schools ‘alert to signals about users’ preferences’ (Rigorous Review, p. 34, citing Andrabi et al. 2008).
The seventh hypothesis is ‘State collaboration, financing and regulation improves private school quality, sustainability and equity’ (Rigorous Review, p. 34).

Under this hypothesis there are (unusually, meaning that this is more strongly weighted for their conclusions) three assumptions:

1. Assumption 13: States have the knowledge, capacity and legitimacy to implement effective policy frameworks for collaboration and regulation of the private-school sector. Eight studies: positive (zero), neutral (zero), negative (eight).

2. Assumption 14: State regulation is effective and improves the quality, equity and sustainability of private-school provision. Eleven studies: positive (three), neutral (two), negative (six).

3. Assumption 15: State subsidies improve the quality, equity and sustainability of private-school provision. Three studies: positive (three), neutral (zero), negative (zero).

The headline findings are that ‘attempts by governments to intervene in the private education sector are constrained by a lack of government capacity, understanding and basic information on the size and nature of the private sector’ (Rigorous Review, p. 35); where ‘state regulation of private schools exists, it is not necessarily effective or may be selectively enforced’ (p. 36). In short, governments are not particularly adept at regulating private schools in the countries examined. However, there are a small number of studies showing that targeted vouchers could improve inputs and outputs (Rigorous Review, p. 39); the evidence comes only from Pakistan. In line with the comments in Chapter 3 above, as this evidence is about the capacity of governments and not about the virtues or otherwise of private schools, we will not explore the issue further.
Hypothesis 8 is that ‘Private schools have positive effects on the overall education system’ (Rigorous Review, p. 40). Two assumptions follow:

1. Assumption 16: Private schools complement government school provision. Four studies: positive (four), neutral (zero), negative (zero).
2. Assumption 17: Market competition enhances quality in state and private school sectors. Three studies: positive (one), neutral (one), negative (one).

Although it might be an interesting question to explore, it is not clear why Assumption 16 is of importance for the underlying research question ‘Can private schools improve education in developing countries?’ The Rigorous Review suggests that there is an ongoing debate about whether ‘the relationship between private and state schools is competitive or complementary’ (p. 40, emphasis added). A complementary relationship would mean that ‘private schools fill the gaps left … by the under-provision of government schools’; the alternative suggests ‘that private schools overlap and compete with government schools, thereby drawing students from the state into the non-state sector’ (Rigorous Review, p. 40). Indeed, this question ‘really goes to the heart of what is driving the apparent growth of private schools’ (p. 40, emphasis added).

It would seem simpler than that. If government schools improve, then it is likely that the real (why did they write ‘apparent’?) growth of private schools would slow, and if government schools do not improve, then the growth is likely to accelerate. In the former scenario, private schools could be seen as ‘complementary’, whereas in the latter they are likely to be seen as ‘competitive’. This is a dynamic situation that depends on how private and government schools are serving the poor. It is, of course, of interest to academics but does not seem to be at the core of questions to ask concerning whether or not private schools can improve education.

Regarding Assumption 17, the Rigorous Review notes that ‘Economic theory suggests the presence of private schools should enhance the performance of all school types within a more competitive educational market. This is underpinned by the idea of choice as a driver of quality’ (p. 41). This is not correct. It all depends on whether there are incentives within the public sector for improvement. If they are present, then the presence of private schools could lead to improvements. If they are not, then the presence of private schools is not likely to have any impact at all. Indeed, the Rigorous Review notes that one of the studies (Pal 2010) shows precisely this – there was no impact on government pass rates, ‘attributed to a lack of real competition between private and government schools, the latter of which
enjoyed secure enough funding to not be incentivised to compete on quality’ (Rigorous Review, p. 42).

How can state systems be incentivised? One way is through per-capita funding, so that it matters to a head teacher of a government school if the school loses students — for then funding to that school will diminish. Similarly, performance-related pay of teachers and head teachers could lead to incentives to improve. However, if there are no incentives like this, then the public schools will not be motivated to improve, whatever the dynamism of the private-school market. In other words, this again is about state capacity to improve rather than about the virtues of the private sector in education, so it is not explored further.

Finally, we can observe what may be a contradiction in how the Rigorous Review views a desirable role to be played by private schools. Assumption 16 views complementarity as positive, while Assumption 17 appears to view competition (at least that which can raise standards) as positive for private schools. But filling the gaps where government provision is lacking (which would be seen as positive for Assumption 16) would not then be able to put pressure on government schools to improve (so could not be positive for Assumption 17).
The role of private schools in meeting the educational needs of the poor is a controversial area. Low-cost private schools have arisen from within poor communities themselves, as a solution to the problem of providing educational opportunities suiting people’s aspirations. This grass-roots initiative dramatically challenges the development status quo, which typically sees government schools as the only way forward for the poor; many of the development experts are, not surprisingly, wary of what the poor are doing for themselves.

In the light of the importance of the debate about the role of private schools and the controversies that surround it, we welcome the involvement of DFID and their desire to get a balanced and accurate summary of the research to date.

The DFID-commissioned report into this area ostensibly ‘set out to rigorously and objectively interrogate a number of hypotheses and assumptions’ underpinning this ‘polarised debate’ (Rigorous Review, p. 50). This response has suggested that it is has not succeeded in doing so in a sufficiently rigorous and objective fashion.

Having only taken the studies that passed the Rigorous Review’s selection process, we find that the evidence is more strongly positive about the role and impact of the private schools and their potential to improve education in the developing world than the Rigorous Review concluded. Had we been able to include quality studies other than those selected by the Rigorous Review team, it is likely that an even more positive picture of the contribution of low-cost private schools would have been made.

Reading the way evidence has been summarised in the Rigorous Review and then reading the articles themselves has been a sometimes-odd experience. Several times articles have been reported as saying the opposite of what they actually say, or are far more nuanced in their findings than the Rigorous Review states.

Several of the assumptions appear to have been expressed in a way that intended to convey private schools in a poor light, or at least that does not allow their potential to show through. Two in particular appear to be ‘strawman’ assumptions, written so that the case for private schools can swiftly be demolished.

The way the ‘theory of change’ is constructed seems itself to lead to an unwarranted ambiguous position: the Rigorous Review says that ‘the majority of assumptions at the heart of this debate are in fact weakly evidenced’ (p. 50, emphases added). In fact, of the seventeen assumptions, only twelve can be considered to be at the ‘heart’ of the debate.
We have left to one side how evidence was allowed through the quality and other criteria, not wanting to get involved in discussion of technicalities regarding the methodology of educational research. However, we have mentioned how odd it is that the Rigorous Review team missed some evidence that had already gone through the rigours of peer review, published in reputable academic journals, while including work 'published' on the web. Moreover, while some datasets have seen multiple articles included, work from others in the field is largely neglected. The arbitrary cut-off date including only work published in the past five years also excluded the research pioneers in this field.

Overall, this suggests that the Rigorous Review was, in the end, flawed in its conclusions. The evidence we suggest here, if properly 'interrogated', leads to a much stronger, more positive response to private education than that given by the Rigorous Review.

What difference does our revised assessment of the literature make? Of the twelve assumptions that are at the heart of the discussion, the Rigorous Review found the majority of these (seven) positively in favour of private schools, although for two of these the overall strength of evidence was weak (by definition, because of the small number of studies). The other assumptions were neutral in outcome (two) or negative concerning the role of private schools (three). This led to the review’s ‘lukewarm’ conclusions about private schools.

With the revised analysis, correctly reading the articles and making minor adjustments to the wording of two of the assumptions, we now find that all twelve of the assumptions are positive in favour of private schools, with the most important ten out of these twelve ‘well supported’ in terms of overall strength of evidence (see Table 13.1). This revised conclusion obviously leads to a much more positive assessment of the role of private schools in development.

Why does it matter to us that the evidence is correctly portrayed? It matters because both of us are aware of the struggles and successes, against the odds, of entrepreneurs who run low-cost private schools, and we want to see their contribution fairly acknowledged. We see parents in challenging circumstances choosing private schools for their children, and we want the evidence to be fairly assessed as to whether their sacrifices are worthwhile. And it matters, of course, because many readers of the rigorous review involved in educational development are influential and their actions make a difference. Some will make far-reaching decisions. We want these to be aligned firmly with what is really happening on the ground.

None of this is to say, of course, that low-cost private schools are already solving all the educational problems of the poor; still many are excluded from education altogether, and standards can be further improved. But what the research analysed here shows clearly is that the entrepreneurs who run low-cost private schools are showing the way, having demonstrated the feasibility of bringing affordable quality education to the poor; it is up to others to come alongside them, to help ensure improved education for all.
Table 13.1 Findings of Rigorous Review and our revised findings.

<table>
<thead>
<tr>
<th>Assumption</th>
<th>According to Rigorous Review</th>
<th>Revised assessment</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Better learning outcomes</td>
<td>Moderate</td>
<td>+ Strong</td>
</tr>
<tr>
<td>2. Better teaching</td>
<td>Strong</td>
<td>+ Strong</td>
</tr>
<tr>
<td>3. Geographically reach poor</td>
<td>Weak, by definition (neutral findings)</td>
<td>0 Moderate</td>
</tr>
<tr>
<td>4. Equally accessible to girls</td>
<td>Moderate</td>
<td>−</td>
</tr>
<tr>
<td>4*. Improve education for girls</td>
<td>Moderate</td>
<td>−</td>
</tr>
<tr>
<td>5. Cost of education delivery lower</td>
<td>Moderate</td>
<td>+ Moderate</td>
</tr>
<tr>
<td>6. Financially sustainable</td>
<td>Weak, by definition (small number of countries and studies)</td>
<td>− Moderate</td>
</tr>
<tr>
<td>7. Poor(est) are able to pay fees</td>
<td>Weak, by definition (neutral findings)</td>
<td>0 Strong</td>
</tr>
<tr>
<td>8. As affordable as state schools</td>
<td>Weak, by definition (small number of studies)</td>
<td>−</td>
</tr>
<tr>
<td>8*. Nearly as affordable as state schools</td>
<td>Moderate</td>
<td>−</td>
</tr>
<tr>
<td>9. Perceived quality underpins choice</td>
<td>Moderate</td>
<td>+ Moderate</td>
</tr>
<tr>
<td>10. Choice is informed</td>
<td>Moderate</td>
<td>+ Moderate</td>
</tr>
<tr>
<td>11. Users participate in decisions</td>
<td>Weak, by definition (small number of studies)</td>
<td>+ Weak, by definition (small number of studies)</td>
</tr>
<tr>
<td>12. Responsive to user demands</td>
<td>Weak, by definition (small number of studies)</td>
<td>+ Weak, by definition (small number of studies)</td>
</tr>
</tbody>
</table>

Key: + evidence supports assumption; − evidence counters assumption; 0 evidence is ambiguous.
the entrepreneurs who run low-cost private schools are showing the way ... it is up to others to come alongside them, to help ensure improved education for all.

To conclude, we summarise the evidence for the most important assumptions about private schools, using the literature database of the Rigorous Review.

CONCLUSION 1: PRIVATE SCHOOLS ARE BETTER QUALITY THAN GOVERNMENT SCHOOLS

The evidence given in the Rigorous Review is well supported: private schools are of higher quality, in terms of educational outcomes and teacher commitment, than government schools. It does not mean to say that they already satisfy international standards, or that improvements do not need to be made. Although low-cost private schools have emerged without any of the resources of government or international agencies behind them, they are already achieving better results than government schools. This alone is a remarkable and powerful finding.

CONCLUSION 2: PRIVATE SCHOOLS MEET THE DEMANDS OF EQUITY

Research evidence shows that low-cost private schools geographically reach the poor. There is no suggestion of a geographical limit beyond which they have not or cannot pass. Low-cost private schools also appear better to narrow achievement gaps for disadvantaged groups than do government schools. While there is some evidence that private schools have not reached gender parity, the evidence is well supported that private schools are improving education for girls in developing countries.

CONCLUSION 3: PRIVATE SCHOOLS ARE MORE COST-EFFECTIVE THAN GOVERNMENT SCHOOLS AND ARE FINANCIALLY SUSTAINABLE

Private schools, the evidence clearly shows, have lower cost of education delivery than government schools; in combination with their higher quality levels, this would suggest greater cost-effectiveness. Using the proxy measure of length of operation of private schools, private schools are very clearly financially sustainable. Even stronger circumstantial evidence comes from the vast number of private schools: so many educational entrepreneurs would not be entering these markets if they did not believe the schools to be financially sustainable.

CONCLUSION 4: PRIVATE SCHOOLS ARE AFFORDABLE TO THE POOR, SOMETIMES NEARLY AS AFFORDABLE AS GOVERNMENT SCHOOLS

Private schools are affordable by significant minorities of the poorest and most disadvantaged groups in society: findings show anything from 20 per cent to nearly 40 per cent of these groups accessing private schools. Studies that find private schools unaffordable by the very poorest sometimes suggest that public schools
are also unaffordable. In some contexts, the total cost to parents of sending a child to private school is surprisingly competitive compared to the total cost of sending to a government school. Low-cost private schools have developed something quite remarkable: they have managed to find some ways of bringing their costs down low enough to serve poor communities. They are not serving all of the poor and poorest of course, but they demonstrate what is possible; it may be up to others, including readers of the Rigorous Review, to explore ways to help those families who wish to send their children to private schools but who presently cannot afford to do so.

**CONCLUSION 5: PRIVATE SCHOOLS ARE THE PREFERRED OPTION FOR POOR PARENTS**

Parents make informed choices within the private-school market. Overwhelmingly, poor parents appear to prefer private over government schools.

**CONCLUSION 6: PRIVATE SCHOOLS ARE ACCOUNTABLE**

By paying fees, parents keep private schools accountable to them. They have the right to ‘exit’ from private schools; whether or not they use this, private schools are aware that they might so are responsive to the needs of poor parents and children.
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