The Role and Impact of Private Schools in Developing Countries: A Response to DFID’s “Rigorous Literature Review”

James Tooley and David Longfield,
E.G. West Centre, Newcastle University

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References
Executive Summary

In April 2014, a DFID-commissioned report, *The role and impact of private schools in developing countries* (henceforth the ‘Rigorous Review’) was published, 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?’*  

The Rigorous Review selected 59 studies from a much larger sample according to quality and other criteria. It explored three ‘thematic fields of analysis’, Supply, Demand and Enabling Environment. These were further analysed under eight Hypotheses and 17 Assumptions, the propositions against which research evidence was tested. Of the 12 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 (Table 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 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*. In several cases, literature which 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 37 out of 40 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 of 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 are in private secondary schools.
• On equity (gender), 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.

• On affordability, studies that are reported to show only very small percentages from the poorest quintile accessing private schools (e.g. Härmä and Rose, 2012) in fact show much larger proportions, once the literature is properly ‘interrogated’.

• On affordability, 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.

2. 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; one in particular seems like a ‘straw man’ assumption, the wording of which makes it impossible to see private schools in a favourable light. For example:

• On equity (gender) ‘private schools are equally accessed by girls and boys’ 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 compare 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 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.
• 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% 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.

3. 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 turn around 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 suggestion 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) – which 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, un-paginated document explicitly reporting provisional (not finalised) evidence, were accepted. Detailed discussion about

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1 In our bibliography, this is Härmä 2011a, but for consistency with Day Ashley et al. (2014), throughout the text we have referred to this as Härmä 2011.
research methodology is not entered into; however it is clear that many of the articles accepted used research methods which should not have allowed 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 12 of the Assumptions are positive in favour of private schools, with the most important 10 out of these 12 moderately or strongly supported in terms of overall strength of evidence (Table 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.
<table>
<thead>
<tr>
<th>Assumption</th>
<th>Overall strength of evidence</th>
<th>Result</th>
<th>Revised Assessment</th>
<th>Overall strength of evidence</th>
<th>Result</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Better learning outcomes</td>
<td>Moderate</td>
<td>+</td>
<td>Strong</td>
<td></td>
<td>+</td>
</tr>
<tr>
<td>2. Better teaching</td>
<td>Strong</td>
<td>+</td>
<td>Strong</td>
<td></td>
<td>+</td>
</tr>
<tr>
<td>3. Geographically reach poor</td>
<td>Weak, by definition (neutral findings)</td>
<td>0</td>
<td>Moderate</td>
<td></td>
<td>+</td>
</tr>
<tr>
<td>4. Equally accessible to girls</td>
<td>Moderate</td>
<td>−</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4*. Improve education for girls</td>
<td></td>
<td>Moderate</td>
<td>+</td>
<td></td>
<td></td>
</tr>
<tr>
<td>5. Cost of education delivery lower</td>
<td>Moderate</td>
<td>+</td>
<td>Moderate</td>
<td></td>
<td>+</td>
</tr>
<tr>
<td>6. Financially sustainable</td>
<td>Weak, by definition (small number of countries and studies)</td>
<td>−</td>
<td>Moderate</td>
<td></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>
<td>Strong</td>
<td></td>
<td>+</td>
</tr>
<tr>
<td>8. Affordable as state schools</td>
<td>Weak, by definition (small number of studies)</td>
<td>−</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>8* Nearly as affordable as state schools</td>
<td></td>
<td>Moderate</td>
<td>+</td>
<td></td>
<td></td>
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<tr>
<td>9. Perceived quality underpins choice</td>
<td>Moderate</td>
<td>+</td>
<td>Moderate</td>
<td></td>
<td>+</td>
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<tr>
<td>10. Choice is informed</td>
<td>Moderate</td>
<td>+</td>
<td>Moderate</td>
<td></td>
<td>+</td>
</tr>
<tr>
<td>11. Users participate in decisions</td>
<td>Weak, by definition (small number of studies)</td>
<td>+</td>
<td>Weak, by definition (small number of studies)</td>
<td>+</td>
<td></td>
</tr>
<tr>
<td>12. Responsive to user demands</td>
<td>Weak, by definition (small number of studies)</td>
<td>+</td>
<td>Weak, by definition (small number of studies)</td>
<td>+</td>
<td></td>
</tr>
</tbody>
</table>

Key: + evidence supports assumption; − evidence counters assumption; 0 evidence is ambiguous.
1. Introduction

In April 2014, an important report, *The role and impact of private schools in developing countries* (Day Ashley et al., 2014), was published. 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’ (p. 1). 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, b), 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 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., CEDAW, 2014; Rolleston and Adefeso-Olateju, 2014; Day Ashley et al., 2014, 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 ‘denationalize’ 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, 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 65 years of development consensus that emerged from the Universal Declaration of Human Rights in 1948.

The DFID-sponsored report (Day Ashley et al., 2014; henceforth the ‘Rigorous Review’), 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[^3]

[^3]: 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 10 published studies conducted in more than five countries, with 75% of the studies’ findings pointing in the same direction.
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. 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, 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’ (Day Ashley et al., 2014, 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.** In several cases literature which 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.

2. **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. One in particular seems like a “straw man” assumption, the wording of which makes it impossible to view private schools in a favourable light.

3. **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 turn around 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.
These three areas will be discussed in relation to the Rigorous Review’s Hypotheses and Assumptions in Sections 5-12 of this paper. Identifying problems in the analysis requires going into some detail, hence this forms the main body of the Response.

In this Response, we will not (apart from a few warning remarks) challenge the quality of the 59 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 generalizability, 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’ (Day Ashley et al., 2014, 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 59 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 (Section 4). In addition, we will briefly discuss the Rigorous Review’s Theory of Change (Section 3), suggesting it includes Assumptions which do not seem to be at the core of the debate. Before embarking on this, Section 2 discusses Definitions used in the Rigorous Review and this Response.

Our concluding remarks can be found in Section 13.

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4 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 Day Ashley et al. (2014, 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 and/or founded independently of the state.

Given these three criteria, they then distinguish 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 which cover ‘all or part’ of their costs, are managed largely independent of the state and are owned and/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, churches as well as proprietors: in fact, there is just one proprietor-run school (Table 1, p. 771, 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, p. 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, amongst 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 or NGOs, and ‘for-profit’ private schools, those run by proprietors, partnerships or companies (see Tooley and Longfield, 2014a, b). 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 not therefore clear how the subject of their second rigorous review will differ from their 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’ (LCPs) 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’ (Footnote 1, p. 4). This suggests that low-fee private schools 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 which 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.

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’ (Day Ashley et al., 2014, 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’ (Day Ashley et al., 2014, p. 1) and on ‘non-elite private schools’ (Day Ashley et al., 2014, p. 5). Mostly these are likely to be low-fee private schools; this caveat is

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5 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.
assumed in the discussion that follows. How low is low, however, will remain undefined for this report.

Elsewhere we have suggested a more exact definition (e.g. Tooley, 2013; Tooley and Longfield, 2014a, b; Longfield and Tooley, 2014), which we hope will gain traction. This works backwards from poor families’ incomes, to estimate what they could afford to spend on private schooling. Using the internationally accepted $1.25 and $2 per person per day poverty lines (at 2005 exchange rates and purchasing power parity), we first calculate, for a specific country/region, the average total annual income for an average-sized family. We then take some percentage of that total annual income, and specify that this is the maximum amount that can be spent on total school fees. (We used 10%, inspired by the discussion in Lewin, 2007. But the figure can be adjusted higher or lower as more is known about what families can afford.) Finally, we divide that ‘maximum amount’ by the average number of school-aged children in a family. This gives us the maximum annual per child school fees, in other words, the maximum fees affordable in private schools. For the $1.25 poverty line calculation we specify this as ‘lowest cost’, while the $2 poverty line gives ‘low-cost’ private schools. For instance, the study in the slums of Monrovia, Liberia, found 78 percent of for-profit private schools were ‘lowest-cost’. Looking at all areas of Sierra Leone (Western Area), i.e. not just the poorest, found 66% of ‘for-profit’ private schools were lowest cost and an additional 15% low cost (Tooley and Longfield, 2014a, b).

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3. Theory of Change

The Rigorous Review seeks to answer the research question ‘Can private schools improve education for children in developing countries?’ (Day Ashley et al., 2014, p. 1). More specifically, given the context of their 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 work in this area, the team finally selected 59 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’. Under each Hypothesis were constructed ‘Assumptions’, 17 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.

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 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. They are 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 17] at the heart of this debate are in fact weakly
evidenced’ (Day Ashley et al., 2014, p. 50, emphases added). It is our contention that, of the 17
Assumptions, only 12 could be considered 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 12 Assumptions; for the sake of completeness brief notes are made
about the discussions of state capacity and markets (Assumptions 13 to 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 brief observations:

First, the Rigorous Review was to look at ‘the latest quality published evidence’ 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, un-paginated document explicitly based on provisional, rather than confirmed data. On the other hand, however, their final selection excludes some studies on low-cost private schools published in reputable academic journals that have already gone through the rigours of peer-review (and were published in the correct time period and focused on countries of interest to DFID). In total, less than half of the sources (27 out of 59, 46%) were published in academic journals, while a further 11 were published in academic books, which may have also have had some form of (usually less rigorous) peer review. That leaves 21 out of the 59 sources which are unlikely to have been peer reviewed at all.

Second, the team narrowed down the focus from the past 10 years to the past five years for date of publication, which they said gave a ‘“natural” cut-off date’ (p. 10), as 79% of the studies in their initial bibliography were published after the beginning of 2008. Using other selection criteria on this 79% 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 59 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 16 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 the research was conducted. This means that some studies included 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 authors excluded from the Rigorous Review, perhaps for obvious reasons, there is work from certain authors which is so strangely excluded that the sources have to be named. We mention only two notable examples – there are of course others. First, while Härmä’s work in India (which draws rather negative conclusions about private schools) is very well-represented (as we shall see, three articles 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, c). 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.

Second, another strange omission is Dixon’s (2013) book on low-cost private schools, 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 there featured on pp. 4, 6, 8, 9, 11, 12, 14, 15, 17, 18, 20 and 21. 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), e.g. 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 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 amongst others by The Economist (2012) as pioneers of research in the field of low-cost private education. Had

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7 Two other relevant research-based papers from the correct time-period and included in Mcloughlin (2013) but excluded from the Rigorous Review are a survey from Lagos by Tooley (2013) and one from Bihar by Rangaraju, Tooley and Dixon (2012). Again, these have very different conclusions to those reached by the Rigorous Review.
this Centre been consulted, more research might well have been brought to light that would lead to the Rigorous Review drawing conclusions different from those found in Day Ashley et al. (2014).

We now turn to examine the evidence adduced by the Rigorous Review, to explore their conclusions about the role of private schools in improving ‘education for all’. First, Sections 5 to 10 take us through what is considered to be the major Hypotheses concerning Supply and Demand, while Sections 11 and 12 make brief comments about the remaining hypotheses.
5. Quality

The first Hypothesis of the Rigorous Review is that ‘Private schools are better quality than state\textsuperscript{8} schools’ (p. 14). This leads to two ‘testable assumptions’ that 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 21 studies, as follows:

- Finding: Positive (14), Neutral (6), Negative (1).

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 (Day Ashley et al., 2014, p. 15). Other rigorous studies find similar effects from other settings, including Africa (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% of the evidence (i.e. 16 articles rather than the 14 found) to support the thesis (Day Ashley et al., 2014, 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.’ (Day Ashley et al., 2014, p. 17). This is an odd suggestion, true only if the

\textsuperscript{8} ‘State’ schools are used by Day Ashley et al. (2014) to mean ‘government’ or ‘public’ schools in the international sense.
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, distance to school or government policy. This seems unlikely. A single study is cited to support this claim:

- Ohba (2012), researching in Kenya, ‘finds that government primary school leavers were more likely to enter government secondary schools than private school leavers’ (p. 17). This is quoted as counter-evidence to the Assumption that private schools are superior in terms of quality.

Now, Ohba (2012) is a small-scale study, with 12 opportunistically-chosen private schools and two government schools, so it is odd that the Rigorous Review assumed it was possible to generalize 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’ (p. 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 headteachers 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 headteachers 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 headteachers ‘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 (2012) ‘finds that government primary school leavers were more likely to enter government secondary schools than private school leavers’ (p. 17). Figures from Ohba’s Table 2 (p. 774) confirm this: 34.9% of private and 43.4% of government school leavers go on to some form of government secondary school. Ohba expresses it in this way:

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, p 775).
Ohba goes on to give a reason why the government has taken this course of action and two supporting pieces of evidence for the assertion that the difference is due to the quota system. The reason is given for the government action:

*This is because private schools out-perform the public schools in the KCPE examination by a considerable margin (Somerset 2009). Thus, there is an attempt by the government to provide pupils graduating from government primary schools with vacancies for the top national secondary schools (Ohba, 2012, p 775).*

So the quota system exists because the private schools are doing well.

The supporting evidence is in the form of a request to the government by KISA to review the system. This was reported in the papers and included the complaint that “*denying children access to public resources because of the primary schools they attended is .... discriminatory*” (p 775). The second piece of evidence was from a government school principal who had “*received some senior class pupils from elite private schools because by such transfer, the newcomers would have a better chance of admission to a prestigious national secondary school*” (p. 775). Both point to the pressure by the government against the private schools which appear to be performing well.

Day Ashley et al., do refer to the quota system (2014, p.18), but appear to conclude that the slightly lower proportion of private school pupils progressing to the government schools is an indication that the private schools are doing poorly, when it is because they are doing well. The selective use of progressing to *government* secondary schools is perhaps also misleading as a slightly higher proportion of private than government pupils progress to *private* secondary school. When all destinations are considered 77.9% of private primary school leavers are known to continue in some form of education, compared to an almost identical 79.0% of government primary school leavers.

This is *positive*, not counter-evidence⁹.

**Neutral evidence**

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

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⁹ 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.
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’ (Day Ashley et al., 2014, p. 17, emphasis added).

Now, Wadhwa (2009) is a rather thin document, explicitly based on provisional (not finalised) evidence from ASER 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%.’ 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.’ This means that 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%) which is reduced to a 10.8 percentage points, or 41%, 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 17 out of 20 states, and in a majority of these (10 out of 17), the difference is greater than eight percentage points. In ‘reading in English’, after controlling for background variables, private schools have the advantage in all 20 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 40 sets of data, and in 37 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 (Day Ashley et al., 2014, 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.)

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10 The 5% seems to be incorrect: A fall from 8.6 to 2.9 percentage points goes from 20% to 6.7% (not to 5%) which gives the 1/3 and 2/3 split between school effect and other factor effect which is cited.
advantage of 3.39, whilst 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’ (Day Ashley et al., 2014, 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 ‘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, p. 499).

This study is clearly positive not neutral.

**Revised finding**

In the Rigorous Review, there are now 21 studies, as follows:

- Finding: Positive (17), Neutral (4), Negative (0).

This finding now has a ‘strong’ level of consistency (as more than 75% 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 grassroots initiative is found to be delivering education of a higher quality than that provided by government and extensively funded by international agencies. Instead of their 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 14 pieces of evidence as follows:

- Finding: Positive (12), Neutral (1), Negative (1).

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. In fact, the evidence is even more positive for private schools than the Review suggests. For instance, looking at Hartwig’s (2013) evidence from 56 villages in Tanzania, the Rigorous Review claims that ‘private secondary schools on average have a PTR [pupil-teacher ratio] of 33:1 and government ones had a PTR of 48:1’ (Day Ashley et al., 2014, p. 20). In fact, these figures have been misread: they are in fact 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’ (Day Ashley et al., 2014, p. 21, emphasis added).

As noted earlier, Ohba (2012) gives evidence from 14 opportunistically-selected schools, 12 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, Table 1, p. 771). It is normal to use averages when dealing with data of this kind. On average (mean), in the 12 private schools, the PTR is 28:1, while in the government schools it is 51:1. (In fact of the 14 schools in the study the 10 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 14 pieces of evidence as follows:

- Revised Finding: Positive (13), Neutral (1), Negative (0).

**Caveat**

Again, the Rigorous Review comes up with a caveat here, an *unintended consequence* (Day Ashley et al., 2014, 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 grassroots 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’ (Day Ashley et al., 2014, 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:

- Findings: Positive (3), Neutral (4), Negative (1).

The findings being inconsistent, by definition, the ‘overall strength of evidence’ is ‘weak’ (see 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:

- (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’ (Day Ashley et al., 2014, p. 23).

- (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.’ (Day Ashley et al., 2014, p. 23).

11 They found that 28% 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%), Bihar (54%), Uttar Pradesh (57%), Punjab (65%) and Haryana (68%).
(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 but also larger populations’ (Day Ashley et al., 2014, p. 23).

It would seem to us with regard to the Assumption (private schools geographically reach the poor) that each of these pieces of evidence actually goes some way towards confirming the Assumption. The first shows a minority of rural children with access to private school, with poorest states (see our footnote 11) even better served than others. The same study finds that the private schools are more likely to be found in the larger villages of these poorer states. That is entirely consistent with the supposedly negative finding (from Pal, 2010), because that was using data from four of India’s poorest states (Bihar, Madhya Pradesh, Rajasthan and Uttar Pradesh) (p.785). That study found, for example, that 57% of the villages in Uttar Pradesh had a private school, although it was the better-off villages which were more likely to have a private school. Similarly, Andrabi et al. (2008) show private schools reaching poor rural areas, this time in Pakistan, although not necessarily the poorest villages within those areas. They point out, however, that wealth and size are not the unique determinants of private school location. Private schools also tend to be located in villages where women with secondary education live (providing a source of local female teachers).

In this way all three studies point to the existence of low-cost private schools in some, but not all, poor areas and all suggest that there are factors other than just wealth that determine the likelihood of the presence of a private school.

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 generalizability 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 are 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.

For instance, the following three studies, classified as neutral in the Review, should, we believe, be reclassified as positive.

- ‘In South Africa, Schirmer’s (2010) analysis concludes that private schools exist in ‘unexpected places’ geographically, and in larger numbers than previously thought but they caution that their analysis does not imply LFPs are financially accessible to the poorest.’ (Day Ashley et al., 2014, p. 22). The study found schools geographically accessible serving significant numbers of children in “even in the most remote rural areas in Limpopo and the Eastern Cape” (Schirmer 2010, p. 28) (the two poorest provinces in South Africa).

- ‘Woodhead et al. (2013) in India ... argue the biggest growth ... in recent years has occurred in rural regions of Andhra Pradesh, but they note that the largest share of private schools remains in urban areas’ (Day Ashley et al., 2014, pp. 22-3). Again, this study shows large growth of private schools geographically in rural areas where many of the poor live.

- ‘Baird’s (2009) nationally representative analysis in India finds no statistical relationship between a particular region’s wealth and levels of private school enrolment. According to their data, private schools in India are as likely to exist in poor areas as they are in rich ones.’ (Day Ashley et al., 2014, p. 23). Private schools are as likely to be found in poor areas as rich areas, therefore they are geographically reaching the poor.

Revised finding

Given this discussion, we suggest the following revised assessment for the evidence given:

- Revised Finding: Positive (8), Neutral (0), Negative (0)
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, e.g., by narrowing educational gaps between more and less disadvantaged groups. Pal and Kingdon (2010) 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.*’ (p. 19).

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 which 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, 12 studies are included, with the following assessment:

- Finding: Positive (2), Neutral (3), Negative (7)

Their headline finding is: *‘Most of the evidence reviewed indicates that girls are less likely to access private schools than boys’* (Day Ashley et al., 2014, 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, and how does it relate to the overriding research question?**

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/or 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, Srivastava (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, India (Day Ashley et al., 2014, 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’ (Srivastava, 2008a, p. 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% of girls, 58% boys), as well as in government school (e.g. 37% girls, 63% 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, pp. 341–342).

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*.

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% as opposed to 40% of girls) are

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12 It would have been helpful if Day Ashley et al. (2014) 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.
ever-enrolled in our sample while a larger proportion of ever-enrolled girls (19.6% as opposed to 15.6% 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% for both boys and girls)’ (footnote 17, p. 790, 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’s (2010) study from four schools in Madhya Pradesh shows that before private schools were created, ‘girls were already severely limited in accessing secondary schooling’ (Day Ashley et al., 2014, 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’ (Johnson and Bowles, 2010, p. 487).

- Fennell (2012) reports from Pakistan on qualitative evidence collected from interviews with 25 parents and 25 school-attending youth, plus interviews with teachers and headteachers. It notes that ‘there is evidence that girls’ enrolment has risen as a consequence of a growing number of private schools’ (p. 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*.

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.’ (Day Ashley et al., 2014, 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),’ (Härmä and Rose, 2012, p. 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’, Day Ashley et al., 2014, 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:


One of these studies is clearly incorrectly categorised:

Hartwig’s (2013) study from 56 villages compares government and private schools at the secondary level only (where there are 20 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’ (p. 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% 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% 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% 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:

‘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’, (Day Ashley et al., 2014, 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’. (p. 333, emphases added).

Aslam (2009) then goes on to control for ‘observed and unobserved household characteristics’ (p. 334) and find that, once these are taken into account, boys are now more likely to attend private schools than girls (pp. 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, p. 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.

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.13

Finally, Nishimura and Yamano (2013) do find that ‘girls have a 3.6% 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% level. Many high-quality studies choose to disregard such probabilities, so some might have considered this evidence to be insufficient to show gender disparity.14 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

13 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’ (Day Ashley et al, 2014, 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 should be doing this, given that girls are considered a separate category of disadvantage.

14 Moreover, this evidence is also about the probability of girls going to school once other pupil and household characteristics are controlled for, so falls under the same caveat as footnote 13 concerning Aslam (2009).
northern states having significantly higher female disadvantage rates when compared to southern ones’ (Day Ashley et al., 2014, p. 24).

This may be a slightly misleading way of presenting what the study actually reports: Maitra et al (2011) 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.’ (Maitra et al., 2011, p. 17).

In fact, looking in detail at Maitra et al. (2011), it is clear that in five out of the 14 states researched, neither the ‘GIRL’ variable, nor any of the other combined variable measures they examine, are significant. There is no evidence (even at the 10% level) of gender inequality in private schools in states as diverse as Orissa, Jammu and Kashmir, Tamil Nadu, Gujarat and Kerala (Table 9, p. 36). That is, there is no evidence of female disadvantage in five of the 14 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 amongst other things to lower economic development in the north (pp. 17-8), 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 well 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 of 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*.

- 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, p. 1). But they also mention 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’ (Pal and Kingdon, 2010, p. 4). 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 gender gap in literacy ... among 10-14 year old children’ (p. 14). (The age group 10-14 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’ (p. 17).

So it is suggested 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 2. 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 (7), Neutral (2), Negative (2).

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 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.
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<th>Assumption 4 (as Rigorous Review)</th>
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| Number of studies | 2 | 3 | 7 |

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| Number of studies | 7 | 2 | 2 |
7. Cost-Effectiveness and Financial Sustainability

The third Hypothesis is that ‘Private schools are cost-effective and financially sustainable’ (Day Ashley et al., 2014, 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 (7), Neutral (0), Negative (0)

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’ (Day Ashley et al., 2014, 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:

- Findings: Positive (7), Neutral (0), Negative (0)

Assumption 6: Private schools are financially sustainable

Initial finding

The Rigorous Review says that there are only two studies available here, as follows:

- Finding: Positive (0), Neutral (0), Negative (2).

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’ (Day Ashley et al., 2014, 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 schools’ (p. 27). Regarding this measure, there are only two applicable studies, it is claimed, both negative.

**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’ (Day Ashley et al., 2014, 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, footnote 6, p. 164). That is, a quarter of the 16 reported private schools had closed within 12 months. Here we have two oddities: first, 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. Second, why is it not noted that in the original paper it is a quarter of the schools closing within 12 months, whilst 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.

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”’ (Day Ashley et al., 2014, p. 27, emphases 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 amongst 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, p. 163).

In fact, rather than ascribing made-up quotes to their work, the Rigorous Review could have used Tooley et al. (2008) for evidence for 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 which also provide evidence on school longevity, yet are ignored:

- 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 (Table 1).

- Andrabi et al. (2008) show that in the context of extremely rapid expansion of private schooling, the median age of a private school in 2000 was four years (p. 335), even though over one fifth of the schools had opened in the past two years.
• Härmä and Adefisayo (2013) point to evidence that one quarter of the private schools in Lagos, again in a time of rapid expansion, were 13 years old or more (p. 133).

• Srivastava (2008b), in her study of 10 randomly-selected recognised private unaided schools finds the average age of her schools is around 11 years (using her Table 1, p. 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 whilst missing this other work from the same state which explicitly tabulates evidence about school longevity.

• Schirmer (2010) 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.’ (p. 49).

• Ohba (2012) gives the foundation year of the 12 private schools in the Kenya study, (Table 1, p. 771). These are very old schools in general (the modal date was 1982). On average, the age of the schools was just under 20 years (assuming the data was collected in 2011).

• Kremer and Muralidharan (2008) report that more than 50% of the rural private schools in their sample were more than five years old, again in a context of ‘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.

**Revised finding**

There are now (at least) nine studies available, as follows:

• Finding: Positive (8), Neutral (0), Negative (1)

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); Härmä and Adefisayo (2013) report over 12,000 private schools in Lagos State alone (p. 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 (2009) reports over 24,000 private schools in Punjab alone, even as far back as 2001 (p. 333). So many entrepreneurs, especially from poor communities, are not likely to be in this market unless they have good reason to believe 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’ (Day Ashley et al., 2014, 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 13 studies falling under this category, as follows:

- Initial finding: Positive (0), Neutral (8), Negative (5).

In their text however they 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’ (Day Ashley et al., 2014, 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:

1. Some of the poorest families are sending their children to private schools
2. Some of the poorest who would like to send their children to private schools are not, for financial reasons
3. Moving up income or wealth quintiles brings increasing proportions using private schools, presumably in part for financial reasons
4. 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 to have done that already? Or (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’d 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’d 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 upfront). 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 from governments and international agencies over decades; low-cost private schooling by contrast is a grassroots initiative which 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 here. 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’ (Day Ashley et al., 2014, 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% of the poorest quintile accessing private schools is neutral evidence.

Given our discussion above, our preferred approach would be to disagree – 10% 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 which 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% of the poorest quintile accessing private schools should then be taken as positive evidence for the Assumption, while less than 10% would be negative.

*Second*, indications of unaffordability have been taken from qualitative research which found parents expressing a preference of private school for their children, but instead enrolled 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 their 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% to 30% of an average household income in the poorest quintile, and in another study 29.8% of average household income in the poorest quintile for total schooling costs of one child (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 don’t neatly fit into the categories above (Baird, 2009, Tooley et al., 2008, 2011; Phillips and Stambach, 2008). 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:

- Kremer and Muralidharan (2008) is reported as finding ‘while private schools mainly cater to the better off in rural areas, many children within them come from the more disadvantaged backgrounds’ (Day Ashley et al., 2014, p. 28).

Indeed, the survey found, for example, that ‘20% of the private school students are first generation learners, which while lower than the 30% in public schools, is still quite significant. Thus … many of their students come from disadvantaged backgrounds’ (Kremer and Muralidharan 2008 p. 1415).

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15 Page numbers for the identical 2007 pdf on which the book chapter is based.
• 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’ (Day Ashley et al., 2014, 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\textsuperscript{16} suggests, to explore appropriate public policy, not to report details of data.

For these two studies, assuming ‘first generation learners’ are to be classified as amongst 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)’ (Day Ashley et al., 2014, p. 28).

This figure can indeed be read off from the graph in Härmä and Rose (2012) p. 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: e.g., ‘quintile five represents the richest families in the sample ’ (Härmä, 2011, p. 354, emphasis added), they are ‘relatively well-off’ (p. 354, emphasis added). So the study finds that 10% of the poorest quintile of ‘a “typical” rural UP 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. 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

\textsuperscript{16} ‘Low cost private schools for the poor: What public policy is appropriate?’
Table 3 below. The last two columns fit Härmä’s income quintiles for her 13 villages in UP 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 8th 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’ (Härmä, 2011, p. 351); ‘only the most well-off’ own ‘even carts’ (Härmä, 2009, p. 152). In another of her articles using the same data, we are also told that ‘48% percent of sampled families own no land at all’ (Härmä, 2010, p. 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, p. 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, pp. 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% of the lowest quintile but 30% of the second lowest are using low-cost private schools. Both are likely to be classified as the poorest families. Moreover, 47% of the third, and 56% of the 4th quintile are also using low-cost private schools. These are the poor in India. Similarly for income, just over 10% (lowest quintile) and around 20% (second lowest quintile) are using private schools. Again, these should be classified as the poorest families. Meanwhile, more than 50% 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% given in the Rigorous Review. Given our discussion above, this evidence is positive, not neutral.

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17 Assuming that the asset index quintiles are somewhat similar to the equivalised income quintiles discussed.
Table 3 Deciles and quintiles of wealth, India

<table>
<thead>
<tr>
<th>By decile, All India (Vanneman &amp; Dubey)</th>
<th>Descriptor</th>
<th>Mean household income (All India)</th>
<th>Mean household income (13 villages UP)</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>Quintile 1</td>
</tr>
<tr>
<td>Decile 2</td>
<td></td>
<td>10,701</td>
<td>7,049</td>
<td>Quintile 2</td>
</tr>
<tr>
<td>Decile 3</td>
<td>Poor</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>Average</td>
<td>24,791</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decile 6</td>
<td>Rich</td>
<td>31,914</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decile 7</td>
<td></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>Very rich</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>

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’ (Day Ashley et al., 2014, 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, p. 11). Their table shows that, for eight-year-olds in 2009, 44.1% of the total children enrolled in school were in private school, including 31.3% of rural, 29.3% of Scheduled Castes, 21.7% of Scheduled Tribes and 44.2% 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% cut-off mark, it is suggested this evidence is taken as positive for this Assumption.

- Schirmer (2010) and Fennell (2012\(^{18}\)) are also cited as negative evidence concerning the affordability of private schools. Schirmer, it is said, ‘concludes similarly’ to Singh and Sarkar (2012) for South Africa, while Fennell (2012) reports ‘that parents claim that poverty deters them from sending children to private schools’ (Day Ashley et al., 2014, p. 28).

The first study was not aiming to be a comprehensive search for all private schools, but certainly found significant numbers in very poor areas, suggesting their affordability to many of the poor. Fennell (2012) reveals the distaste with which parents viewed sending their children to government schools – as one rural mother put it in her study: ‘Mostly everyone wants to enrol their children in private schools’ (Fennell, 2012, p. 265). But some cannot afford it. However, many do find private schools affordable in her study.

Without any quantitative evidence from either regarding how many fall into the category of being able to afford private schools, we could exclude these studies from our analysis, or we could follow our preferred interpretation of Assumption 7, as discussed above, and consider these as positive evidence, as they indicate some (albeit unspecified proportion) of the poor and poorest sending their children to private schools, (even though a greater proportion actually desire to do so). (It makes no difference to the final conclusion whether they are included or excluded in this way. In Table 6 below we include them).

- Härmä (2011) and Härmä (2009) are also used as negative evidence, the former to illustrate the mismatch between parental preference and the latter concerning the unsustainability of fees. Regarding the former, she ‘finds that despite a vast majority of parents indicating a preference for private schools over poor quality government alternatives, only 41 percent of the children in the sample were actually attending private schools’ (Day Ashley et al., 2014, p. 28).

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\(^{18}\) The Rigorous Review has Fennell, 2013, but this is an error.
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% 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 Scheduled Caste (SC) or minority religion (predominantly Islam) as one measure of deprivation (Härmä, 2009, p. 158). Her figures show that for these most deprived groups, 23% of SCs and 31% of Muslims are using low-cost private schools (Härmä, 2009, 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’ (Day Ashley et al., 2014, p. 29).

We shall say 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 Day Ashley et al (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, further considerations must also be brought to bear here – and here we return to the fourth proposition highlighted at the beginning of this assumption, that ways of investigating affordability found in the literature by examining average school fees and average costs may
not actually be very helpful when considering how the *poorest* families can access private education.

Surely what is of most interest here is behaviour of *actual* families in actual schools. Private schools vary in price: Härmä’s study gives the monthly fees as ranging between Rs. 27 and Rs. 60, suggesting that the amount that different parents pay may vary considerably. The poorest families who decide to send their children to private school will probably not send them to an averagely priced private school, but will look for the one most affordable to them. Härmä’s evidence is clear: some 10% of those in the poorest quintile even for those villages (i.e., the poorest of the poorest by Indian standards) have found a school that they can afford.

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*’ (Härmä, 2009, p. 163), which suggest 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*’ (Srivastava, 2008b, p. 454, emphases added). This type of negotiation both with the school and even in the community is described extensively by Phillips and Stambach (2008).

All of this shows the flexibility of private school costs to poor parents – and makes us realize 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% of the poorest of the poorest attending private schools with the figure of 30% 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% of the poorest (of the poorest) are sending their children

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19 Parents had this power because of the ‘*schools’ interests in retaining clients*’, (Srivastava, 2008b, p. 454).
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)\textsuperscript{20} 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 percent) of its income’ (Day Ashley et al., 2014, 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’ (Akaguri, 2013, p. 143)

Second, we note that Akaguri is using ‘criteria suggested by Lewin (2007)’, that ‘no more than 10%’ of a poor household’s income should be ‘expended on one child’s education’ (Akaguri 2013 pp. 154f). Given this, then clearly the 29.8% 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% 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% 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’ (p. 159).

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

Third, we must note that Akaguri was researching in a very poor area, where ‘About 60% of its inhabitants live on less than one dollar a day’ (Akaguri, 2013, p. 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

\textsuperscript{20} We hesitated about critiquing this article, given the author’s tragic and untimely death in 2012. Given that 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, p. 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.
are adjusted for inflation (only), we see (Table 4 below) that all but the highest quintile in the study is 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% 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%. 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.

Table 4 Mean household income by quintile, Ghana

<table>
<thead>
<tr>
<th>Mean Household Income by Quintile (GHS)</th>
<th>National</th>
<th>Mfantseman Municipality</th>
</tr>
</thead>
<tbody>
<tr>
<td>Q1</td>
<td>728</td>
<td>1034</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Q2</td>
<td>1020</td>
<td>1448</td>
</tr>
<tr>
<td>Q3</td>
<td>1098</td>
<td>1559</td>
</tr>
<tr>
<td>Q4</td>
<td>1263</td>
<td>1793</td>
</tr>
<tr>
<td>Q5</td>
<td>1544</td>
<td>2192</td>
</tr>
</tbody>
</table>

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, even sending to government school, apart from those in the richest quintile. Our Table 5 below shows, using Akaguri’s preferred 10% 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.

Table 5 Schooling costs per term, per year and by income quintile (Akaguri)

<table>
<thead>
<tr>
<th>GHS</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Schooling costs (public) per term</td>
<td>32.7</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Schooling costs (private) per term</td>
<td>62.06</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>School costs (public) per annum</td>
<td>98.1</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>School costs (private) per annum</td>
<td>186.18</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Income quintiles</td>
<td>208.02</td>
<td>355.24</td>
<td>518.74</td>
<td>875.17</td>
</tr>
<tr>
<td>% one child (public)</td>
<td>47%</td>
<td>28%</td>
<td>19%</td>
<td>11%</td>
</tr>
<tr>
<td>% one child (private)</td>
<td>90%</td>
<td>52%</td>
<td>36%</td>
<td>21%</td>
</tr>
</tbody>
</table>

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’ (Day Ashley et al., 2014, 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’ (Day Ashley et al., 2014, 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 generalize 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 ‘nationlly representave analysis of rural and urban India indicates that unrecognised schools do, in some cases, serve the poorest of the poor’ (Day Ashley et al., 2014, p. 28).

In the absence of quantitative evidence, as 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’ (Day Ashley et al., 2014, 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, 2011) are used to provide ‘different … explanations for affordability’ (Day Ashley et al., 2014, 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 says that it did not (unfortunately) measure the percentage of children enrolled on these concessionary and free places, although it acknowledged their existence, and pointed to the team’s earlier studies which showed these places making up between 5% to 18% of all places (Tooley, et al. 2008, 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 (p. 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 6. Overall we now suggest the following:

- Revised finding: Positive (10 or 11), Neutral (2 or 1), Negative (0).

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’.
## Table 6 Matrix of research evidence for Assumptions 7

<table>
<thead>
<tr>
<th>Assumption 7 (original)</th>
<th>Positive</th>
<th>Neutral</th>
<th>Negative</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Tooley et al (2011)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Phillips and Stambach (2008)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of studies</td>
<td>0</td>
<td>8</td>
<td>6</td>
</tr>
<tr>
<td></td>
<td>(in the Rigorous Review text)</td>
<td>(in the Rigorous Review summary)</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Assumption 7 (revisited)</th>
<th>Positive</th>
<th>Neutral</th>
<th>Negative</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Härmä and Rose (2012)</td>
<td>Akaguri (2013)?</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Singh and Sarkar (2012)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Schirmer (2010)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Fennell (2012)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Härmä (2011)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Härmä (2009)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Baird (2009)</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Phillips and Stambach (2008)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of studies</td>
<td>10 or 11</td>
<td>2 or 1</td>
<td>0</td>
</tr>
</tbody>
</table>
Assumption 8: Private schools are as affordable to users as state schools

Initial finding
The Rigorous Review has five studies, as follows:

- Findings: Positive (0), Neutral (0), Negative (5).

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* (Day Ashley et al., 2014, p. 29). The overall strength of the evidence is ‘weak’, by definition because of the small number of studies (p. 12), but is negative towards private schools.

Straw man assumption
Unless one was an extreme libertarian who believed that 100% 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 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 prefer a qualified assumption such as:

- 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% or more of sending a child to private school, then this would count as positive, 50% to 74% would be neutral and below this 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* (Day Ashley et al., 2014, p. 29).

Akaguri had found that the cost of sending a child to government school was 53% 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 (Table 4, p. 151)\(^{21}\). These figures are given in the first four columns of Table 7 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*, p. 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, p. 2, emphasis added). So it seems possible that the difference between figures here is because the public school headteachers did not know how much was spent on transport, whereas the private school headteachers 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?’ For 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

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\(^{21}\) 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).
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 7 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% of the cost of sending a child to private school, rather than the 53% 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*.

**Table 7 Relative costs of sending a child to public and private school, Ghana**

<table>
<thead>
<tr>
<th>Cost item</th>
<th>Per term (GHS), according to Akaguri</th>
<th>Public/private %</th>
<th>Per term (GHS), realistic assumptions</th>
<th>Public/private %</th>
</tr>
</thead>
<tbody>
<tr>
<td>Transport</td>
<td>0</td>
<td>4.08</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Food</td>
<td>17.46</td>
<td>25.27</td>
<td>17.46</td>
<td>17.46</td>
</tr>
<tr>
<td>School fees</td>
<td>0</td>
<td>8.1</td>
<td>0</td>
<td>8.1</td>
</tr>
<tr>
<td>PTA</td>
<td>1.3</td>
<td>0.99</td>
<td>1.3</td>
<td>0.99</td>
</tr>
<tr>
<td>Examination fees</td>
<td>0.76</td>
<td>1.07</td>
<td>0.76</td>
<td>1.07</td>
</tr>
<tr>
<td>Extra classes</td>
<td>0</td>
<td>7.53</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>School uniforms</td>
<td>7.17</td>
<td>8.02</td>
<td>7.17</td>
<td>8.02</td>
</tr>
<tr>
<td>Stationery</td>
<td>6.05</td>
<td>7</td>
<td>6.05</td>
<td>7</td>
</tr>
<tr>
<td><strong>Total per child</strong></td>
<td><strong>32.74</strong></td>
<td><strong>62.06</strong></td>
<td><strong>53%</strong></td>
<td><strong>32.74</strong></td>
</tr>
</tbody>
</table>

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*

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22 In this case, Akaguri (2013) does suggest that in the four private schools under investigation, extra tuition and lunch were compulsory (p. 151). This is unusual in the Ghanaian context.
nine times as much as the cost of a government school.’ (Day Ashley et al., 2014, p. 29, emphasis added).

In fact, Härmä (2009) shows that sending a child 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 schools (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., p. 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 1/5 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, p. 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’ (Day Ashley et al., 2014, p. 29). Moreover, ‘In Dimla, Bangladesh, Sommers (2013) finds that government schools charge less for books and uniforms than private tuition-charging schools’ (Day Ashley et al., 2014 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 (2010)23 in fact finds something similar to the three times more expensive that we suggested was more appropriate for Härmä’s data,

23 It is 2010, not 2011.
giving us confidence in the working outlined above: Siddhu (2010, Table 3, p. 12) 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 Day Ashley et al. (2014) 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, p. 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 it 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’ (p. 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 (2012) gives evidence on fees in Kenya (Table 1, p. 771). 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% of private school costs). This is positive evidence for our revised Assumption 8*. Moreover, the author writes ‘although the government schools under study were cheaper than some private
schools, the extra levies charged by the former meant that they did not offer completely fee-free education’. In fact exactly half (six) of the private schools were cheaper than one of the government schools and exactly a quarter (three) were cheaper than both government schools. Again, this shows how important it is not to rely on averages when trying to answer questions about affordability – some of the private schools are in fact the cheapest option for parents. This appears to be positive evidence even for the Rigorous Review’s Assumption 8. Moreover, ‘In addition, private schools in Kibera were more flexible in terms of management and regulations. For example, each school had its own uniform, but it was not necessarily a prerequisite for admission (which was not the case with the government school). … Some parents also mentioned that the collection of supplementary levies by government schools became more stringent after the introduction of FPE. In contrast, the mode of payment was flexible in the private schools under study, parents/guardians being afforded room for negotiation. Moreover, some pupils in private school received scholarships, a form of assistance that was not available to their government school counterparts. These factors indicate that there were several advantages to the private school style of management that met the needs of poor households.’ (p. 777). All this could bring down the costs of private schools to be closer still to the government option.

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

**Revised finding**

Table 8 shows the revised findings for Assumption 8 – which not surprisingly still remains negative – and the more realistic Assumption 8*, which is as follows:

- Revised finding: Positive (3), Neutral (1), Negative (2).

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% supporting the Assumption.
Table 8 Matrix of research evidence for Assumptions 8 and 8*

<table>
<thead>
<tr>
<th></th>
<th>Positive</th>
<th>Neutral</th>
<th>Negative</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Assumption 8</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(original)</td>
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<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Number of studies</strong></td>
<td>0</td>
<td>0</td>
<td>5</td>
</tr>
<tr>
<td><strong>Assumption 8</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(revisited)</td>
<td></td>
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<td></td>
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<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Number of studies</strong></td>
<td>2</td>
<td>1</td>
<td>4</td>
</tr>
<tr>
<td><strong>Assumption 8</strong></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>*</td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Number of studies</strong></td>
<td>3</td>
<td>1</td>
<td>2</td>
</tr>
</tbody>
</table>

Summary: 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% to nearly 40% 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 presently cannot afford to do so.
9. Choice

Hypothesis 5 is ‘Demand for private schools is driven by informed choice and a concern for quality’ (Day Ashley, et al., 2014, p. 30). Two Assumptions follow:

- Assumption 9: Perceived quality of education is a priority for users when choosing private schools: 11 studies, Positive (8), Neutral (3), Negative (0)
- Assumption 10: Users make informed choices about the quality of education: 7 studies, Positive (6), Neutral (0), Negative (1)

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.

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’ (Day Ashley et al., 2014, p. 30). Moreover, positive support is found for ‘Users make informed choices about the quality of education’ (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’ (Day Ashley et al., 2014, 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 environment, as well as academic quality. It is not for outsiders to say what should or should not count as a valid choice in the market.

The Rigorous Review, on the other hand, does seem to judge that some ways of choosing private schools are better than others. While the ideas about quality seem broad enough (covering academic quality, discipline and infrastructure), the evidence given as ‘neutral’ – e.g. ‘English-language instruction’, ‘a short journey to school ... small class sizes, scholarships, free meals and friendly teachers’ (Day Ashley et al., 2014, p. 31) – suggest that these reasons for choosing private schools are not as valid as the others. It is hard to see why not.

Evidence elsewhere of the narrow view the Rigorous Review takes of valid choice occurs when they examine Accountability (see below). Here they note that in a study from Punjab, Pakistan ‘when parents were informed that their school performed worse than expected according to exam results, they did not respond by enrolling their child elsewhere – i.e. pursue the exit strategy’ (Day Ashley et al., 2014, p. 34). This seems to imply that academic exam results should be the only reason on which choice of school is based. But there could be a myriad of other reasons, including those listed above, which made parents still prefer this school even though the exam results were not as good as expected.

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 (like 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’ (p. 28). Härmä reports that there is a ‘near universal preference for private schools’ (p. 353, emphasised in the original), with ‘94.4 percent of sample parents’ preferring private over government school (p.

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24 The one study that is given as negative evidence for Assumption 10 would presumably also be negative here.
Indeed, in her large-scale household survey, ‘the majority of families (84 percent) view government schools negatively and LFPs positively (77 percent)’ (Härmä, 2011, p. 353).
10. Accountability

Hypothesis 6 is that ‘Private schools are accountable to users’ (p. 32). Two Assumptions follow from this:

- Assumption 11: Users actively participate in or influence operational decision making in private schools: 3 studies – Positive (3)
- Assumption 12: Private schools are responsive to users’ demands and complaints: 5 studies – Positive (5)

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.

Private schools are accountable

As for Choice, we shall explore the two Assumptions together. Here the way the Assumptions and discussion are made seems to suggest at least partial misunderstanding that there can be different forms of 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’ (Day Ashley et al., 2014, 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’. 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). They also note that ‘In principle, exit is a market strategy that depends on choice, while voice is more likely in a public sector setting’ (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 which 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’ 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’ (Day Ashley et al., 2014, 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’ (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’ (Day Ashley et al., 2014, p. 34, citing Andrabi et al., 2008).
11. Financing and Partnership

The seventh Hypothesis is ‘State collaboration, financing and regulation improves private school quality, sustainability and equity’ (Day Ashley et al., 2014, p. 34).

Under this Hypothesis there are (unusually – meaning that this is more strongly weighted for their conclusions) three Assumptions:

- Assumption 13: States have the knowledge, capacity and legitimacy to implement effective policy frameworks for collaboration and regulation of the private school sector: Studies – 8; Negative (8).

- Assumption 14: State regulation is effective and improves the quality, equity and sustainability of private school provision: Studies – 11; Positive (3), Neutral (2), Negative (6).

- Assumption 15: State subsidies improve the quality, equity and sustainability of private school provision: Studies – 3; Positive (3).

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’ (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 in the countries examined at regulating private schools. However, there are a small number of studies showing that targeted vouchers could improve inputs and outputs (p. 39); the evidence comes only from Pakistan. In line with the comments in Section 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.
12. Markets

Hypothesis 8 is that ‘Private schools have positive effects on the overall education system’ (Day Ashley et al., 2014, p. 40).

Two Assumptions follow:

- Assumption 16: Private schools complement government school provision: Studies – 4, Positive (4), Neutral (0), Negative (0).
- Assumption 17: Market competition enhances quality in state and private school sectors: Studies – 3, Positive (1), Neutral (1), Negative (1).

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’ (Day Ashley et al., 2014, 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’ (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’ (Day Ashley et al., 2014, p. 42).

How can state systems be incentivised? One way is through per capita funding, so that it matters to a headteacher of a government school if the school loses students – for then funding to that school will diminish. Similarly performance-related pay of teachers and headteachers could lead to incentives to improve. However, if there are no incentives like this, then public schools will not have any incentives to improve, whatever the dynamism of the private school market. In other words, this again is about state capacity to improve, rather than 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).
13. Conclusions

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 grassroots 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 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’ (Day Ashley et al., 2014, p. 50). This Response has suggested that it is has not succeeded in doing so in a rigorous and objective enough 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 they 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 sometimes been an 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 conveys private schools in a poor light, or at least does not allow their potential to show through. One, particularly, appears to be ‘straw man’ assumption, 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 17 Assumptions, only 12 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’ve 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 12 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 12 of the Assumptions are positive in favour of private schools, with the most important 10 out of these 12 ‘well-supported’ in terms of overall strength of evidence (see Table 9 below). 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 want to see their contribution fairly acknowledged. We see parents in challenging circumstances choosing private schools for their children and 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 of course 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 9 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></td>
<td>Overall strength of evidence</td>
<td>Result 25</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</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</td>
<td>Moderate</td>
<td>+</td>
</tr>
<tr>
<td>6. Financially sustainable</td>
<td>Weak, by definition</td>
<td>−</td>
</tr>
<tr>
<td>7. Poor(est) are able to pay fees</td>
<td>Weak, by definition</td>
<td>0</td>
</tr>
<tr>
<td>8. Affordable as state schools</td>
<td>Weak, by definition</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>+</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</td>
<td>+</td>
</tr>
<tr>
<td>12. Responsive to user demands</td>
<td>Weak, by definition</td>
<td>+</td>
</tr>
</tbody>
</table>

25 Key: + evidence supports assumption; − evidence counters assumption; 0 evidence is ambiguous.
To conclude, we summarise the evidence for the most important Assumptions about private schools, using the literature database of the Rigorous Review.

**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.

**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.

**Private schools are more cost-effective than government schools and are financially sustainable**

Private schools, the evidence clearly show, 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.

**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% to nearly 40% 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 presently cannot afford to do so.

**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.

**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.
References


