

Rejoinder to “Private Schools Rigorous Review Team” on the *Role and Impact of Private Schools in Developing Countries: A response to the DFID-commissioned “Rigorous Literature Review”*

James Tooley and David Longfield, 28th March 2015

Introduction and summary

We are grateful to the “Private Schools Rigorous Review Team” (henceforth “Team”) for their reply to Tooley and Longfield, (2015), itself a response to the Team’s DFID-commissioned “Rigorous Review” (Day Ashley et al., 2014). In our Rejoinder we have confined our remark to three areas. First, we are pleased that the Team acknowledges it made *eight* of the errors and misrepresentations we highlighted in our Response (section 1). However we are disappointed they did not address more of the errors/misrepresentations we had pointed out. Second, we are also disappointed that the Team forbids our reformulation of two (out of 17) of their assumptions (section 2), even though it turns out that others had been allowed to make changes earlier. Third, we also question the way in which the Team has tried to reduce the impact of some of the studies included in the research sample, now that they seem to point in favour of private schools (section 3).

1. Errors and misrepresentations accepted

We are very pleased to note that the Team acknowledges *eight* of the many errors/misrepresentations that we pointed to in our Response¹ (these were by no means the most serious of the errors/misrepresentations that we highlighted, but we are grateful that these at least have been accepted):

- i. On quality: The Team accepts that they misrepresented Ohba (2012) as reporting pupil-teacher ratios were higher in private than government schools. They now agree that on this proxy for quality, private schools are better than government schools (6.2, p. 6)² and that the study should be reclassified.
- ii. On quality: The Team accepts that they misreported Hartwig (2013) on pupil-teacher ratios (6.2, p. 6, fn 9). We had pointed this out in passing and agreed it was not

¹ Incidentally, on the UKFIET website, the introductory comment implies that some of these errors that we’d highlighted were merely typographical. This is absolutely not the case. They were all points of substance, some of course more important than others. There *were* an uncharacteristically large number of typographical errors in the Team’s report, but we chose not to highlight these. (This Rejoinder has not been proof-read, as we wanted to upload it to the web quickly, so may contain some typographical errors for which we apologize).

² Where a reference is not given, we are referring to the Team’s “Reply to Tooley and Longfield (2014)”, dated October 2014 but first posted on the web on 26th March 2015.

particularly significant, apart from, it might be thought, illustrating a cavalier approach to evidence. We reported far more serious misrepresentations concerning Hartwig (2013) however which the Team unaccountably ignores (see below).

- iii. On gender equity: The Team accepts that they incorrectly included Härmä and Rose (2012) as a separate study, thus exaggerating the evidence against gender equity in private schools. They agree this went against their stricture of not duplicating studies, as Härmä and Rose (2012) added nothing to the two Härmä studies already included (6.4, p. 6).
- iv. On financial sustainability (or “stability”, as they now call it in their Reply): the Team accepts that they misrepresented Tooley et al, (2008), by making up a quote that was not in the article at all (6.5, p. 7) or indeed in any of the literature surveyed. Correcting this peculiar misrepresentation results in a reclassification of the study, from negative to positive concerning the stability of private schools.
- v. On affordability: the Team says that Härmä and Rose (2012) should again be removed because of duplication, as it adds nothing to the two Härmä studies already included under this heading, this time exaggerating the number of studies which convey a negative picture of the affordability of private schools (6.6, p. 7).
- vi. On affordability: the Team agrees that they incorrectly included Tooley et al., (2011) under this assumption, misreporting what the article was actually about (6.6, p. 7).
- vii. On affordability: the Team accepts that they incorrectly omitted evidence from Ohba (2012), which they now agree should have been included (6.7, p. 7). Ohba (2012) gives crucial *positive* evidence for the affordability of private schools, which the Team unaccountably failed to include.
- viii. On affordability: the Team accepts that they incorrectly omitted evidence from Heyneman and Stern (2013), (6.7, p.7). Heyneman and Stern (2013) report that unregistered private schools can be the cheapest option for parents, crucial evidence for the affordability assumption, again unaccountably ignored by the Team.

Now, if we published a report which had (at the minimum) eight not insignificant misrepresentations and errors such as these, we'd be grateful to those who had pointed out the errors and keen to explore with them whether any further errors could be identified; it would also be a priority for us to withdraw the report until corrections had been made. Instead, the response from at least some of the Team on social media has been condemnatory of our approach, as if they, oddly, deem their work should be above reproach.

We don't need to stress that many of the errors and misrepresentations acknowledged are very serious indeed. However, as noted above, they are by no means the most serious of the errors and misrepresentations highlighted in our response (Tooley and Longfield, 2015). Unfortunately they are the only ones specifically addressed by the Team. In their Reply, they frequently repeat something along the lines that "the team does not accept any of the challenges" to other criticisms (6.4, p. 6), or "the team stand by their original assessments" (6.6, p. 7), etc. However nowhere do they tell us how they've arrived at these sanguine re-appraisals of their work that allow them to dismiss our criticisms. Here are six further errors and misrepresentations identified to which we are still seeking answers (there are still more identified, see Tooley and Longfield, 2015 for a complete summary):

- a) On gender equity: will the Team agree that they seriously misrepresented Hartwig (2013), which clearly shows private secondary schools having a much higher percentage of girls than boys, but which they report as showing precisely the opposite? (Tooley and Longfield, p. 28).
- b) On gender equity: will the Team accept that they wrongly classified Pal (2010) as 'neutral' evidence about gender parity, when it clearly states that there *is* gender parity in private schools in their Indian sample, so is 'positive' evidence? (Tooley and Longfield, p. 27).
- c) On gender equity: will the Team accept that they were at least unhelpful in the way they presented Maitra et al (2011), by not telling readers that in five of the Indian states surveyed there is *no* evidence of gender inequality in private schools, although they use the research as evidence against private schools? (Tooley and Longfield, p. 30).
- d) On quality: will the Team allow that they've incorrectly classified Wadhwa (2009) as 'neutral' evidence, when 37 out of her 40 datasets, controlling for background variables, report often huge private school advantages in achievement over government schools? (Tooley and Longfield, p. 17).
- e) On quality: will the Team agree that they have misrepresented Ohba (2012) as showing that quality is lower in private than government primary schools? Ohba explicitly says that it is government quotas that limit the number of children from private schools going on to government secondary schools, not private school quality, quotas brought in because private schools are of *higher* academic quality than government schools. (Tooley and Longfield, p. 17).

- f) On affordability: will the Team agree that Sucharita (2013), included as negative evidence against the affordability of private schools, has in fact nothing useful to say on this issue? Its simplistic approach that “education in government schools was free” (quoted Tooley and Longfield, p. 54), entirely ignores the issue of the additional costs parents sending their children to government schools have to bear, crucial to any discussion of affordability.

As we note, there are no answers to these, or any of the further criticisms we highlighted in their current Reply. The Team says that they “have thoroughly investigated each of the challenges proposed by Tooley and Longfield” (p. 1); unfortunately they don’t tell us how these investigations showed that our criticisms were incorrect. This is important. In our Response we show how correcting these kinds of misrepresentations and errors leads to a completely different appraisal of the role of private education in developing countries.

It may also be worth noting that none of the above noted errors or misrepresentations involves any re-interpretation of the evidence. They are simply errors/misrepresentations of the research which can be easily observed by a simple examination of the articles in question. We have deliberately avoided any discussion here of issues where deep probing of the research or context is required in order to support our challenges to the Team’s work (e.g., see Tooley and Longfield, 2015, pp. 41-43 and pp. 46-48 on the affordability of private schools, or pp. 29-31 further exploring gender equity in private schools). We have also avoided any discussion concerning private schools geographically reaching the poor, where we agree that our challenges rest on the interpretation of what the Team’s assumption actually means. We stand by our interpretation, but have nothing more to add to our earlier discussion (Tooley and Longfield, 2015, pp. 22-25), which has not been challenged by the Team.

2. *Reframing of assumptions*

In their current Reply, the Team state that we suggested “a rewording of *three* assumptions” (1.2, p. 2, emphasis added). In fact it was just *two*; it is not clear where they arrived at three. That is, we suggested, modestly, a rewording of two out of their 17 assumptions (see e.g., Tooley and Longfield, Table ES 1). These assumptions concern gender parity and affordability. In both cases we strongly felt the Team had created ‘straw man’ assumptions, biased in their design in order to show private schools in a negative light.

In their Reply, the Team say that reframing of their assumptions is not allowed, because “it would be methodologically unsound to reformulate any of the assumptions now *after the*

analysis has been done" (4.1, p. 4). This prohibition is repeated several times (e.g., 6.3, p. 6; 6.4, p. 6; 6.7, p. 7).

We suggest that here the Team seems to be taking these methodological strictures rather too seriously. It's not as if the original assumptions were set in stone, handed down to Moses on Mount Sinai. They were simply *created by the Team members themselves*, based on their initial "rapid appraisal of policy debates and research findings undertaken in the inception phase of this review" (Day Ashley et al, 2014, p. 6).

As such, they could easily be reformulated if others point out that they are biased and unhelpful in gauging the real contribution made by private education. We suggested in our Response that their "rapid appraisal" led to assumptions which would inevitably show private schools in an unfavourable light. We spelled all of this out very clearly in our Response (Tooley and Longfield, pp. 10-11) and it is disappointing that this is not addressed in the Team's Reply.

For instance, when we first read a summary of the Rigorous Review's assumptions we could see clearly how likely it was that their Assumption 4, "Private schools are equally accessed by boys and girls" (Day Ashley et al, 2014, p. 24) would be negatively supported by research conducted in countries where it is known that there is not equal access of girls and boys to school *in general*, so highly unlikely to show equal access to private schools alone. Before reading any of the evidence adduced, we felt that the way their assumption had been expressed would clearly determine the findings – in this case, a negative finding about gender equity in private schools. In our Response, we suggested a rewording which still fits precisely with their desire for assumptions with 'clear practical implications for policymakers and ... testable' (4.2, p. 4), but which doesn't determine the outcome from the beginning.

So for instance, our proposed rewording allows for research showing private schools *improving* access and achievement for girls to be included as *positive* evidence for equity in private education. We thought that would be much fairer, and more useful to policymakers, rather than rejecting such evidence as with the currently-worded assumption. We stand by that conclusion.

Similarly, for the affordability Assumption 8, 'Private schools are as affordable to users as state schools' (Day Ashley et al, 2014, p. 29); it struck us as very odd indeed that any reading of the literature or understanding of policy debates, however 'rapid' an 'appraisal', could possibly lead to that assumption. No one, we thought, would suggest, *a priori*, that the cost of sending a child to heavily subsidised government schools was going to be the same as sending a child to an entirely non-subsidised private school. Again we put forward an alternative wording for this

assumption which didn't prejudge the conclusion and which allowed us to assess the *relative affordability* of public and provision to poor families, again something we felt was much more useful to policymakers. Knowing that the cost to parents of sending a child to government school is over 75% of the cost of sending a child to a private school is very useful to inform the debate; this kind of information was completely lost because of the restricted way the Team had formulated their assumption.

Our reformulations are not allowed, the Team says. However, it turns out that it's mainly *we* who are not allowed to reformulate their assumptions, others have been allowed to do so: we are told that the "assumptions were reviewed by DFID and by external peer reviewers appointed by DFID, and *changes* were made on the basis of their feedback" (4.2, p. 4, emphasis added). If a reframing process was allowed when proposed by earlier reviewers, why is it methodologically unsound for us to propose the same now?

Perhaps it's because the earlier reformulation took place before the Team's analysis of the literature had taken place, but after their initial and quick reading of the evidence? But that is exactly what we did. We saw the unhelpful and biased nature of the assumptions before we went to the literature to review and check what it and the Rigorous Review had said. At worst then, our proposed reformulation of these two assumptions (we repeat, out of the 17 they proposed) could require them, for the sake of methodological purity, going back to re-read the literature in the light of these slightly revised assumptions. That doesn't seem too much of a burden – in fact, we've done the work for them already (Tooley and Longfield, 2015, pp. 25-33 and pp. 50-55).

3. *Having their cake and eating it*

In our Response to the Rigorous Review (Day Ashley et al, 2014), we were very careful not to get involved in discussions of the research methodology of any of the literature included. As we remarked "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" (Tooley and Longfield, p. 7). We did have serious reservations about the methods in several of the studies, in particular that sampling methods meant that results were not generalizable, and sometimes commented on this, in passing. However, now that we have pointed out their mistakes, the Team seems to want to use methodological considerations to lessen the impact of our critique. For instance, when they accept that they erroneously classified Ohba (2012) as *negative* evidence, because they reported the opposite of what Ohba had actually said about

pupil-teacher ratios in government and private schools, they then propose to move it to only 'neutral' rather than *positive* evidence. Their reasons? As the schools in the sample "were purposively selected for the study and involve an incomparable number of private and government schools, this evidence should be reclassified as *neutral* and not as positive as suggested" (6.2, p. 6).

This is very odd. It appears that the methodology of Ohba (2012) was strong enough when they reviewed it to support a powerful *negative* conclusion. It appears that now the evidence doesn't go in their preferred direction, against private schools, they are prepared to admit to doubts about the study's methodology. We agree – the study should probably not have made it through their quality criteria on methodological grounds. But you can't have your cake and eat it: if you include it, then you can't nullify its impact by raising these methodological doubts *post hoc*. And you certainly can't say that all articles which have suspect methodology can only support 'neutral' conclusions, as perhaps is what they are suggesting; this of course would grossly distort the findings.

Conclusion

In the Team's "Closing Comments", it is noted that 'The *small number* of inadvertent factual inaccuracies and pieces of evidence missed or duplicated in specific assumptions can be *easily* rectified' (p. 8, emphases added). While the 'easily' seems to be stretching it a bit, as does the 'small number', we agree that this correction process needs to be done. This is why we spent three months last summer doing precisely this, addressing the errors and misrepresentations found in the Rigorous Review. We hope that our Response will be read alongside the Rigorous Review until it is substantially corrected, so that a misleading impression of the role and impact of private schools in developing countries is not conveyed by a reading of the Team's work alone.

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