Reviewer's report

Title: The cost of uncomplicated childhood fevers to Kenyan households: implications for reaching international access

Version: 2 Date: 23 August 2006

Reviewer: Gerry Killeen

Reviewer's report:

General

The authors have tackled most of the issues superbly, notably that of opportunity cost estimates. Nevertheless, I'm concerned that the conclusions have been somewhat prejudged before completing the analysis. Specifically, their responses to my questions 1.1. and 2.2 seem to distract from rather than address the these methodologically crucial issues. I also note question 5 of reviewer 3 who goes even further and really suggests the existing analysis may be quite misleading. If addressed, these points may well reverse the conclusions drawn (this would still be a very interesting and worthwhile subject matter!) and therefore should be addressed directly and explicitly. The conclusions can then be reconsidered and this is essential if this piece is to be considered for publication. As is, the authors seem to be shying away from an essential reconsideration of the analytical approach (item 1.1) as well as the literature which reinforces this necessity and would temper their interpretation (item 2.2).

If the authors can address these key issues objectively and directly, the conclusions would be highly important (regardless of the final result!) and should be published in BMC Medicine. If they are not, I question the rigour of the piece and would caution against publication by BMC.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1.1. My greatest concern is that this model assumes all fevers are caused by malaria and will be resolved by antimalarial drugs. There are some quite useful estimates of the attributable fraction of fever which is caused by malaria that have become available (eg recent articles on urban malaria by Wang et al) and should be quoted. Comparable rural estimates are available in previous analytical work by the same group and recent field studies in rural Tanzania (TMIH 11:441) confirm that at least half of all fevers are unlikely to be of malarial origin.

Follow up:
The author’s detailed reply addresses a question other than the one above and seems to distract from rather than address my query. I’m fully aware of the absolute need to treat any fever but that’s not really what I’m questioning. The question is what is the current outcome of this necessity and how would the switch to ACTs change this economic equation?

Rephrasing the original question more plainly: If most fevers non-malarious and therefore are not resolved by ACTs (and this is undoubtedly the case, even in districts designated as being holoendemic; The Rufiji example I’ve quoted has an EIR of circa 300 and high prevalence) and consequently incur the same opportunity and direct costs regardless of which anti-malarial is administered, surely this approach grossly overestimates the saving? This is a major concern that really does need to be addressed.

I also note that reviewer 3 goes further than I have in his question 5 and asks about the increased treatment costs due to repeated treatment of malarial fevers. I had not thought of this and certainly take the point. I suggest that to be an objective piece, this insightful question by reviewer 3 also needs to be addressed in a meaningful way.

1.2. The cost of non-resolving fevers is not considered although I accept this would be very difficult to address. This might be another underestimated cost of a) malaria fevers that are not treated with modern drugs (branch 3) and b) Non-malarial fevers regardless of antimalarial treatment. I note that because only resolved fevers are considered, and fevers which persisted but eventually treated, all go into branch 1, the
duration of home management may be substantially underestimated.

Follow up:
This concern has been addressed satisfactorily and I certainly have better solutions to suggest.

1.3. This decision tree approach should be readily compared with similar approaches by Goodman et al and these should be compared in the discussion.

Follow up:
This concern has been addressed satisfactorily.

1.4. While I’m at a loss to suggest an alternative approach, $1 per day for women’s opportunity costs seems a bit excessive under most typical rural Kenyan conditions. This is not really my area of expertise but I hope some of the other reviewers can suggest more appropriate figures. My understanding and experience is that most rural women do not receive cash wages but rather contribute to household income and wellbeing through providing a) non-income generating essential services including care-giving or b) services that lead to cash income which will be received through the man of the house and is already calculated under his income. I suggest de Plaen et al’s work from Cote D’Ivoire (TMIH 8: 459; Acta Tropica 89: 135) is also useful in understanding the interactions between daily activities, financial flows and decision making ability in impoverished rural households. Kenya is clearly a different setting but some features are probably common to both. If this figure is retained it should be more clearly justified and explained to non-specialists such as myself.

1.5. I also question whether caretakers typically spend 50% of their day on caring for a child with a non-severe fever of the type referred to in DHS. My feeling is that it is probably far less in reality. Perhaps rather than making an assumption of 50% or 25% in the absence of literature reports, a sensitivity analysis with graphs exploring the ranges of values and identifying key thresholds could be conducted?

Follow up:
These concerns (addressed collectively as 1.4 & 1.5) have been addressed satisfactorily. The authors are to be commended for addressing this query clearly and comprehensively.

1.6. Some of the assumptions based on the initial data review sound interesting but should be presented explicitly if they are to be accepted and understood by a broad audience.

Follow up:
This concern has been addressed satisfactorily.

1.7. The finding that less than 10% of fevers were treated within 24 hours and only 23% in 48 hours is a very important observation and should be emphasized. My feeling is that the economic incentives to early treatment may be greater than presented here if the opportunity costs because of extended, unresolved fevers could be considered. For example, what proportion of the DHS-reported fevers are actually recurring surges of parasitemia from the same infection? Indeed much recent work has shown just how persistent infections can be, and one report upon which one of these authors is a coauthor (Nature 438: 492) estimates mean duration of infection of about 6 months, during which time a patient may be febrile half a dozen times (See malaria therapy data plus numerous papers analysing it, notably TRSTMH 96: 205, Parasitology 122:379 and Sama et al TRSTMH E-pub ahead of print plus references therein). If effective treatment and can prevent more than one fever, the potential saving of opportunity costs and perceptions thereof by the community could be much greater than presented here. I strongly suggest a careful sensitivity analysis considering the potential to prevent multiple fevers within the range that can be expected from the malaria-attributable fraction.

Follow up:
I accept that this is a difficult challenge, another day’s work and that certainly don’t have any clear solutions to suggest. The objective of this comment was to perhaps draw out any creative solutions which the authors may have been able to formulate. This concern has been addressed satisfactorily.

1.8. I agree absolutely that financing mechanisms to make ACTs accessible for home management should be developed and evaluated as a top priority. I suggest including this in the abstract immediately following the existing conclusions. The potential of making ACTs accessible though facilities is enormous but reaching out through the retail sector has massive potential and circumvent the leakage otherwise likely to occur with these valuable commodities (Malaria J 5: 25).

Follow up:
This concern has been addressed satisfactorily

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

All the authors replies are satisfactory except for 2.2. I don’t accept that these papers are irrelevant, particularly those which show the malaria-attributable fraction of fevers is less than half, even in the most holoendemic setting. For me, there is plenty of space, the literature list is modest and the discussion should be more balanced. I’m particularly concerned that the subjectivity of this piece is compromised by the exclusion of literature which challenges the assumptions made.

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Discretionary Revisions (which the author can choose to ignore)

None

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests