Author's response to reviews

Title: Malaria Rapid Diagnostic Tests in fever case management: a study of the experience with their use at lower level health centres in Uganda

Authors:

Caroline Asiimwe (caroline.asiimwe@finddiagnostics.org)
Daniel J Kyabayinze (d.kyabayinze@malariaconsortium.org)
Zephania Kyalisiima (zkyalisiima@ss.mak.ac.ug)
Jane Nabakooza (jinksuganda@yahoo.com)
Moses Bajabaite (cmbaja@yahoo.com)
Helen Counihan (h.counihan@malariaconsortium.org)
James K Tibenderana (j.tibenderana@malariaconsortium.org)

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Author's response to reviews: see over
Dear Editor - Implementation Science Journal of the BMC,

We appreciate your reviewers comments and recommendations. Over the last few months, we have re-worked the paper to reflect most these comments, highlighted below;

**Reviewer's report**

**Title:** Malaria Rapid Diagnostic Tests in fever case management: a study of the experience with their use at lower level health centres in Uganda  
**Version:** 3  
**Date:** 27 January 2011  
**Reviewer:** Katie Reed  

**Reviewer's report:**

Major compulsory revisions
This is an interesting paper about an important and topical subject. Thank you for this important note.

The authors have stated that it is part of a larger research project and I think that at present the current paper struggles to have an identity of its own. We have appreciated this observation and have re-shaped the paper’s objectives and flow to establish the unique identity of the paper.

The background makes a good case for use of RDT’s in the management of malaria, but spends less space on looking at relevant research into feasibility and acceptability of new health technologies, which appears to be at the heart of this particular piece of research. We have taken this observation into consideration and remodeled the title, introduction and background to reflect the feasibility and acceptability of new health technologies in resource constrained public health settings. As a result, this has changed the entire structure of the paper.

A more focused and evidence based introduction would build a foundation for the rest of the paper, and also hopefully help the authors focus on the results which are most relevant to their research question. This would lead to a major rewrite which would be my recommendation and I would encourage the authors to consider this. We have considered this and a major re-write has been done.

However the following comments refer to particular parts of the paper in its current form that I think require review.

Abstract ‘but the root causes for disregarding test results in treatment are elusive’ this is part of the first sentence of the abstract, and it is not clear what it means, the abstract needs to be crystal clear.

Background pg 4, end of paragraph 1. ‘Given that the majority of fevers…. ‘this statement is very emphatic that money and ITNs have dramatically changed fever presentations in Africa. But it does not tell the reader that fevers caused by malaria have always been difficult to diagnose and have seldom been the cause of the majority of fevers. There needs to be more balance possible references to IMCI. References to the current IMAI and IMCI guidelines by the WHO (2010) have been inserted, to remind the reader that non malarial febrile illnesses have been treated presumptively as malaria for a long time
I like the idea of using a conceptual framework to help understand the factors relating to RDT use. However, I think that it needs more work. We appreciate this. Given the major re-write, the conceptual frame work has been reviewed into a table reflecting the variable, proxy parameters and audience (Figure 2 below). Please note that this framework (Figure 1) was not our finding, rather, theoretical, based on earlier research (Jeng 2004, 2005). We believe this gives a clearer diagrammatic illustration of the intended information.

Figure 1
The paragraph beginning ‘Feasibility.. does not quite agree with Figure 1. Later in the paper it is confusing then the framework becomes a matrix for the sorting of the qualitative data. We agree with this observation. However, this concern has been taken care of within the changes resulting from the major re-write.

Lastly Fig 1 needs to reference the models from which it is adapted. References were inserted as [25,26 – Jeng, 2005 and Vakentash, 2008]. However, these will now appear in the text describing the characteristics in the second paragraph of the methods section.

Page 11 ‘Since parasitological based diagnosis was not part of the national recommended practice at the time of data collection, health workers were at liberty to use the diagnostic option that they felt most comfortable with.’ … This sentence is tucked away in the middle of the paper, but it is important and the implications should be spelled out in the background. We agree with the reviewer and we have moved this sentence into the introduction. Its implications are reflected in the discussion section (line 434), which also bears a quote of the national malaria case management guideline confirming this statement.

Some of the results refer to the fact that current malaria diagnosis guidelines do not incorporate the use of RDT’s, we are not told whether the researchers supplied any new
protocols so it is not surprising that the RDTs were used in very different ways. We appreciate this note, this omission was an oversight on our part. The study had a protocol to guide the larger study and it also used the WHO training manual and job aids which were in draft form by then. We have included the reference to the WHO resources (The WHO RDT trainers manual, [ref 27]) in the Methods section, line 172. This might also be included as a limitation of the study. We would like to suggest that this was not a study limitation given that we have made reference to the resources used to guide the RDT end users.

Page 12 the word 'consenting' is used twice in the next paragraph. Consenting suggests that the attendees had already been informed about the research, and have given their informed consent to be part of it. We have noted this repetition and would like to clarify that it is intentional, seeking to clarify that consent was sought from the mother/guardian as well as young adults they escorted (mature minors) in accordance with the national law.

Pg 14 data analysis. This section requires more detail. You appear to be describing a basic thematic analysis, and as you quote Green and Thorogood in the references you might consult that text, to support your methods section.

Minor essential revisions
I would recommend the whole article to be professionally proof read, as small grammatical errors and typos make some sections difficult to understand. This also applies to the references some of which are inconstant in style or incomplete.

Background page 5. ‘In Uganda to our knowledge…… ‘this sentence is difficult to understand as it offers some facts but not conclusion.

Methods page 8. ‘Study HC’s… the following sentences and bullet points are not consistent, or not clear. ‘No previous involvement in a / any research’ perhaps relevant research might be better.

Page 9. ‘Conveniently’ I think ‘purposively’ would be a better choice of word.

Page 10 middle paragraph. This paragraph is unclear and has some repetition and could be improved

Page 12 middle para last two sentences. These are not consistent, first there is one test then three carried out.

Page 15 ‘some workers felt they did not have sufficient experience with RDT’s to complete the questions’ this affects the sample and should be commented on later.

Discretionary revisions
Methods page 8. The text suggests that Kapchorwa, Mubende and Iganga are rural; it would be useful to confirm this.

Page 9. The reader will be really interested to know how you decided to choose the ICT Pf, at present it suggest you choose it on ‘ease of use’ alone, but I presume that the kits tested were also equally efficacious and in an acceptable price band.

Why did Iganga and Jinja initially miss out on supplies? Later in the results there are statements talking about lack of supplies with kits. It is useful to know
whether the researchers provided sufficient materials or whether these were delivered along with other medical supplies.

Page 16 Performing the RDT and adhering to the job aid. It is not clear whether these results were from the initial training session of the on the job review.

Fig 4. I think this is a very useful overview of the results and in that section (not in the discussion), perhaps this could substitute for some of the ‘quotations’ which you can use more sparingly to support your arguments.

The discussion is generally well written, but I think that it could make more of the results. The fact that the HC’s were using the kits with instructions but no protocol has created a lot of interesting data, but it’s not clear how relevant it is, except to say that RDT’s require to be part of a well planned, pretested and supported fever management protocol. The data about changing staff satisfaction and the acceptance (or not) of the community is something new and I think could be made more of.

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

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**Reviewer's report**

**Title:** Malaria Rapid Diagnostic Tests in fever case management: a study of the experience with their use at lower level health centres in Uganda

**Version:** 3  **Date:** 12 October 2010

**Reviewer:** Richard Ndyomugyenyi

**Reviewer's report:**

Malaria Rapid Diagnostic Tests in fever case management: a study of the experience with their use at lower level health centres in Uganda

Authors: Asiimwe Caroline et al.

This is an interesting article, but it has some major limitations which the authors need to address/discuss. The following are my comments on the manuscript.

**Major comments**

1. This study would have been informative if the authors conducted focus group discussions in addition to exit interviews with health facility attendees to get a wider community perceptions and perceptive on acceptability of RDT use by community members. For instance, caretakers of children who did not want their children pricked for the test might have gone to the health facility, and such perceptions of those people can only be got from focus group discussions. Therefore, the conclusion that RDTs use is acceptably by health attendees without putting into consideration the views of those people who did not attend the health facility should be interpreted with caution and such a finding should not be generalized to the entire community. The authors should discuss this as a limitation of the study. **We do not consider this was a core element of our study focus. This study is clearly described as assessing the perceptions of users (both health**
workers and HC attendees) when a new health technology is introduced and is not about creating a model for the best method of introduction.

2. Under community sensitization on page 10, the study was introduced to local political leaders to garner their support and get them inform their communities. This was not a proper approach of community sensitization. Why was community sensitization not done with community members through village meetings? This is important because when community members are well sensitized and appreciate that not all fever is due to malaria; they accept the results and do not seek for further treatment elsewhere. Is there any evidence that the political leaders informed the community members? Was there a follow-up with community members to ascertain whether the political leaders gave the right information to the community members? What information was given to political leaders to pass over to community members? Community sensitization was carried out in accordance with the MoH communication strategy.

3. Evaluation of feasibility and acceptability of RDT by health workers and attendees on page 10 was done shortly after introducing RDT (2 months). The authors should realize that it was too short a time to assess acceptability because it may change over time with experience of the intervention, and it would have been interesting to document this. Acceptability could improve over time, but it could also drop. The authors should have quantified acceptability over time to allow measurement of any change in acceptability. They should have measured acceptability much later on once the intervention had fully bedded down, more people had experienced it, and the community had longer to reflect on their experiences with RDTs. This has been acknowledged as a limitation of the study in the Discussion section.

4. The study was done in different malaria epidemiological settings: hypo, meso and hyper-endemic. The question is what was the rationale for conducting the study in different malaria epidemiological settings if the authors do not present data on health workers and health centre attendees acceptability of RDTs among the different epidemiological malaria settings? The acceptability of RDTs results by health workers might differ by malaria epidemiological settings. For instance, in areas of high malaria transmission, where more than 85% of fevers are due to malaria, the health worker may not believe that a negative RDT test is truly negative and goes ahead to give anti-malarial treatment despite the negative test. However, in an area of low transmission where only about 10% of fevers are due to malaria, a health worker is more likely to believe in the test than a health worker in an area of high transmission. Data on acceptability of RDT by malaria epidemiological settings should be presented in this article. We have indicated that these findings are reported in another paper (Kyabayinze JD, 2010).

5. The data presented in this article was corrected through in-depth interviews with health workers and health centre attendees, which makes it possible to quantify the results. There are some good quotations which could be quantified. For instance, a quotation by a health worker on page 18 under integration of RDTs into routine work, “How did you expect us to do RDTs without clocks and gloves” and another one on page 19, the community now has confidence in us and the services we offer because of RDTs” How many health workers
mentioned these statements? There are some generalizations in the discussion which are made from few individual statements. For instance, the statement which reads: We noted that religious conviction was regarded as a facilitator amongst health workers. This was mentioned by only one health worker and it might not be right to use such a statement from one individual to generalize for the entire community. This information has changed as a result of the major revision.

Minor comments
1. Under Methods; sub-section on RDT selection and deployment on page 9, ICT pf was used. What was the sensitivity and specificity of the test? Was the sensitivity communicated to the health workers? Health workers might ignore RDT results if they perceive that its sensitivity is low. A sentence about informing the health workers about the high sensitivity of the ICT Pf mRDT has been included in the Methods section.
2. The sentence on page 8 that reads: Study HCs are level II and III centres which provide basic health services such as out-patient and antenatal care. Health centres II in Uganda do not provide antenatal care. The statement as it is written is incorrect.
3. On page 13, the authors mention quality assurance by recording temperature but they do not present temperature recordings anywhere during the study period. There is a sentence stating that the temperatures stayed within the range indicated by the mRDT manufacturer for storage.
4. A high proportion of health attendees were not satisfied with RDT results, especially when the results were negative. It would have been interesting to know whether these patients sought treatment from elsewhere i.e. from private clinics and what treatment they were given and whether such treatment made them stratified. This is not within the scope of this study.
5. Figure 3: How does the picture show that people are seeking for RDT test? At any health facility in Uganda, one would find many patients waiting for health services, with or without RDTs being offered to patients. Therefore, the authors presenting this picture that health centre attendees were waiting for RDT might not be correct. The readers of this article could view it as a stage managed photograph. We understand this concern. This photograph has been deleted.

Yours sincerely,

Caroline Asiimwe