Reviewer’s report

Title: Program level implementation of malaria rapid diagnostic tests (RDTs) use: Outcomes and cost of training health workers at lower level health care facilities in Uganda.

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Reviewer: Hugh Reyburn

Reviewer’s report:

This is an interesting study that addresses a current public health problem, i.e. how to deploy and gain benefit from malaria RDTs. While the writing is generally clear I do have a number of points that the authors might like to consider

The objective is defined in the introduction but repeated in the first para of methods. The objective could be a little more specific than stated.

National malaria guidelines are referred to in several places but it’s not clear what these were at the time of the study. My understanding is that the WHO 2010 guideline was still not policy in Uganda at that time, and if so it’s not clear what staff were told in terms or prescribing antimalarals to RDT negative children <5yrs old.

The evaluation of training is a little unclear, especially in regards to the ‘questions’ and ‘concordance’ referred to at the time of training. Was this data based on ‘before and after’ and was it a questionnaire or observation of RDT use etc.

The sampling of health facilities could be clearer. I understand that this was not a probability sample but based on some degree of judgement but it’s not clear what database was used and how HFs were actually selected.

In my opinion the results of prescribing is the most important but the data in Table 3 are difficult to understand.

- ‘Clinical malaria’ is a confusing term. Malaria is ‘clinical’ by definition in that it is the illness caused by plasmodium parasites. Presumably patients in this category had ‘illness suggestive of malaria’? How was this recorded and from what source?

- The sites were from a range of malaria ecologies including hypoendemic yet the % RDT positive varied little and was above 25% in all. That’s not at all consistent with a wide range of transmission.

- It’s not clear what % of patients (either ‘all’ or ‘suspected malaria’) were RDT tested, and of those what proportion of RDT negatives were treated with AMD. Again, the source of the data is not described in the methods. If it was the routine health facility register then the limitations need to be discussed.
I think the economic analysis is very superficial. I think the costs of preparing the training materials should be included, and more importantly the costs of alternative treatments (e.g. antibiotics) to AMDs and illness episode costs need to be included. Clearly the study design does not allow for all of this but more caution should be used in claiming a cost saving.

I hope the authors can address these points as the paper does contain some potentially interesting results.

**Level of interest:** An article whose findings are important to those with closely related research interests

**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.

**Declaration of competing interests:**

I declare that I have no competing interests