Reviewer's report

Title: Factors determining the willingness to use the malaria Rapid Diagnosis Test in rural area of central Côte d’Ivoire

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Reviewer: Lindsay Mangham

Reviewer's report:

Major Compulsory Revisions

Overall this paper has the potential to be an interesting piece of work, though I would recommend extensive revision to ensure the paper a clear focus and rationale, to make the methods and approach, and to separate the results from the discussion. I also have some questions about the results presented. These in part reflect a lack of detail provided in the methods used in the study for collecting and analysing data, but also possible shortcomings in the statistical and qualitative analysis. I hope my comments will help the authors revise the paper, and provide additional clarification and precision to what conclusions it is possible to draw from their work.

Background (and beginning of the discussion)

The rationale for the study could be presented out more clearly. While overall purpose of the paper has been articulated the rationale given could be improved. For example, there is some inconsistency in the text with one sentence saying RDTs have reduced malaria mortality and the next saying there have been no significant advances in the field of diagnosis (page 3). Similarly, the relevance to new rapid diagnostic tests for HIV is unclear and potentially confusing as the paper is focused on malaria (page 4). Arguments about demand for RDTs given high cost of treatment could be stated more clearly (page 10). The authors may also be interested in Clare Chandler’s work which has used qualitative methods to examine perceptions of RDTs (though primarily from the perceptions of the provider).

Study area and population

The information given in this section could be improved. Considerable, somewhat unnecessary, detail is provided on the history of the Bozi health centre, though other relevant background such as the staff employed and the services offered is not provided.

No information is provided about the second study facility – at least I assume that the study takes place at two facilities.

It would also be useful to provide some description of the residents of these study areas (especially as the results suggest quite different preferences in these two populations).
Data collection and sample size

The description of the data collection should be enhanced to be explicit about the process with which patients were offered a RDT and the eligibility criteria for inclusion in the study. For example, it may be important to understand whether there may be differences in the recruitment or participation in the study between facilities or how/when the patients were recruited. How consistent was the approach taken to being offered an RDT, what information was the patient provided?

It is not clear if the patient was offered the RDT before, during or after their consultation with the health worker and whether it was the health worker or a member of the study team that offered the RDT to the patient.

The authors should explain if the patients incurred any costs for diagnosis and treatment here rather than in ethical considerations. This is important for understanding their choice.

The process for obtaining consent to participate in the study should also be described in this section.

Further information should be provided about the process of data collection since it lacks clarity on some key points. For example, it would be helpful to know whether data were collected during the consultation or on exit, and whether this was by the same person as that offered the RDT since these factors could influence responses. This is important since perceptions of RDTs may be influenced by the process of having been tested.

The description could also be clearer about who was asked which question – were all types of respondents were asked both the structured survey questions and the open-ended questions in the semi-structured interview? The authors should also consider providing a copy of the questionnaire and topic guides used for the survey and semi-structured interviews as an appendix to the paper.

Statistical analysis

Could the authors explain why three different models are presented (Tables 4, 5 and 6) as I did not understand the rationale behind the approach? I would have thought some of the patient characteristics included in Table 4 would be confounding factors that would be relevant across the different models. The response variable for each model – malaria testing with RDT and repeat malaria testing - were not clear to me. I would encourage the authors to seek advice from a statistician.

There are some qualitative results presented – largely in the discussion – though the paper contains no information about how the authors approached the qualitative analysis. I can see the merit in applying mixed methods, but further consideration and description of the approach to the analysis may be needed.

Results

As I understand the sample size calculation, the authors sought to define the proportion that accepted an RDT with a degree of precision, but were not
powered to detected differences by population characteristics. Even if that is not the case, the description of the population (Table 1) may overstate some of the differences by population characteristics. For example, only 4 of 100 were married, so noting differences in acceptance lacks meaning.

The authors refer to views in the population, though should be aware that sample from which they present results is limited to those people attending facilities (and possibly satisfying additional eligibility criteria).

It may be helpful to seek advice from a statistician on how to interpret the P values presented for those variables with multiple categories. I would have expected reporting since p value for the overall category of religion based on joint F-test rather than for each category.

As mentioned earlier, the rationale for the three different models and the extent to which confounders are controlled for the later models should be explained, and this makes the results difficult to understand. It is interesting to see such differences between the study sites, but to interpret this finding the information suggested under data collection would be useful.

Results / Discussion

It is common practice in scientific papers for the results section to present findings without interpretation and for the discussion to reflect on these findings without presenting new evidence from the study. There is both some interpretation contained in the results section, and a considerable amount of new information is presented in the discussion drawing on the open-ended questions.

In general, there is scope to strengthen the discussion by focusing on the results presented rather than introducing new information. As mentioned earlier, it would also be helpful to describe the process of analysing the qualitative data to understand the approach taken and the themes that emerged or whether the themes were structured by the quantitative questions. This may help the reader to distinguish between the opinions presented of respondents and perspectives and views of the authors.

In general I would advise caution about sub-group analysis given the sample size, unless the authors have good reason to expect there to be a difference of opinion. For example, it could be argued that differences in the opinions between the group that accepted and the group that declined would be relevant and of interest. For example, I would question the emphasis placed on the unfavourable attitude to RDTs from unmarried persons (page 13).

The concerns expressed about the RDTs for HIV raises some interesting issues, but to make sense of these findings it is important to know more about the methods deployed in the study, such as what information was the patient provided, were HIV RDTs available in both (all?) study sites.

In describing the respondents’ concerns the concepts of fear, danger, and lack of trust, were mentioned. These are related but distinct concepts and the description is rather muddled. On a related point, the suggestion that RDTs were perceived to be dangerous would benefit from clarification – dangerous in what sense?
Minor Compulsory Revisions

While on the whole the quality of English is acceptable, some editing is required. Examples for which the phrasing doesn't make sense include: social layers (page 4) and shallow inside (page 4).

Some of the variables listed in the Tables would benefit from additional explanation. For example, in Table 3 I was unsure what was meant by “HIV test after malaria test” or “VIH recognition”

Is it really a socio-anthropological study? (page 10). I’m exactly not sure what this term means, but question whether the study is ‘anthropological’ in its approach.

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: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests.