Author's response to reviews

Title: Willingness of using a rapid diagnostic test for malaria in a rural area of central Cote d'Ivoire

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Author's response to reviews: see over
POINT BY POINT RESPONSE

Dear Editor

We refer to your e-mail dated 04 May 2012 and thank you very much indeed for sending us the referees’ comments pertaining to our manuscript entitled “Factors determining the willingness to use a malaria rapid diagnostic test in a rural area of central Côte d’Ivoire” (MS: 9568109296611895). We are deeply grateful for the constructive comments and suggestions offered by the referees and your invitation to revise and resubmit our piece.

In the meantime, we have revised our manuscript in light of the referees’ comments and suggestions. We used green ink to highlight changes made and hope that this will assist you in readily checking our revised manuscript. Below, please find our point-by-point response, clearly explaining how and where in the manuscript (line numbers) changes have been made.

We would be delighted if you would reconsider our revised manuscript for publication in the open-access journal BMC Public Health.

Yours sincerely,

Allassane F Ouattara, Giovanna Raso, Jürg Utzinger and Benjamin G. Koudou (on behalf of all authors).

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 analyzing 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.

We thank Dr. Mangham very much indeed for such an authoritative review. Comments and suggestions have been most useful and we trust that the revised manuscript has further gained in quality. We have revised the manuscript according to Dr. Mangham’s suggestions. Specifically, we now provide more details in the Methods, clarified and improved the statistical analysis and substantially rewrote the results and discussions sections.

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).

We have considerably revised this part of the manuscript by removing some text to enhance clarity of the study rationale. Consequently the Background of our paper is now shorter and the rationale articulated more clearly (see revised manuscript, lines 66-94)

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

Our study took place in only one health facility (i.e. Bozi health center in central Côte d'Ivoire). At the time of our study there was no health facility in Yoho. Obviously, this issue lacked clarity, and hence we have revised the study area and population section accordingly (see revised manuscript, lines 111-132)

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).

As peer Dr. Mangham’s request, the description of the residents in the two study areas is now provided in great depth (see revised manuscript, lines 111-118)

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 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.

We thank Dr. Mangham for this series of highly relevant comments. We have now revised the description of the data collection and sample size calculation. (see revised manuscript, lines 134-160)

With regard to informed consent, we added the following text: “The study protocol was reviewed by the institutional research commission of the Centre Suisse de Recherches Scientifiques en Côte d’Ivoire (CSRS) and received formal approval by the national ethics committee of Côte d’Ivoire. The heads of households in the study villages were informed about the objective and procedures of the study.

Oral consent was obtained from each patient (or legal guardian/caretaker for minors) before using an RDTs based on a finger prick blood sample. Illiteracy rate is very high in the study area leading to an oral rather than written informed consent. The aims, procedures and data confidentiality were explained to the participants, so that they could make an informed decision of whether or not they wanted to be enrolled. Participation was voluntary with no further obligations for those who decided not participate. All patients were offered free treatment according to the diagnoses and the national treatment guidelines.” (see manuscript, lines 97-109)

The questionnaire and topic guides are available from the authors upon request (see revised manuscript, lines 159-160).

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?

All questions were asked by a non-medical staff:

The following information was added in the revised manuscript: “In April 2010, one of the authors (CCC), accompanied by a key informant from Bozi, conducted a cross-sectional survey. The survey consisted of an interview, using a pre-tested questionnaire, addressing five main themes: (i) identification of the interviewed; (ii) knowledge, attitudes and practices of malaria therapy; (iii) local perception of blood and blood-related diseases; (iv) perception of RDT for malaria; and (v) socio-cultural ideologies related to RDT. Participants were interviewed in their home. We interviewed two patient groups, those who accepted and those who rejected to have an RDT performed.” (see manuscript, lines 143-149)

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.

We have revised the modelling part. Essentially, we run two generalized linear mixed models. The first model included “acceptance of malaria RDT during initial health
seeking (first use)” as response variable, whereas for the second model the response variable was “acceptance of RDT performed once again (further use)”. We checked models for confounders and set village as random effect to determine whether variability is a finding of the setting (see revised manuscript, lines 162-173).

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

The statistical analysis section has been re-written to enhance clarity. Two response variables were created as explained above. In brief, one response variable “acceptance of malaria RDT during initial health seeking (first use)” denoted patients who accepted to do an RDT for malaria during the consultation agreeing with the procedure of giving blood for malaria testing. The second variable “acceptance of RDT performed once again (further use)” denoted patients who reported to be willing to do an RDT for malaria if required in future consultations.

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.

Qualitative data were gathered along the two main themes “belief” and “medical examination” in connection with RDTs for malaria. Each theme was addressed according to specific topics (e.g. blood, malaria, effectiveness and usefulness). Once data were categorized and coded in an Excel spreadsheet, the major trends and patterns were identified. More detailed descriptions, as articulated by the respondents (freely translated from French to English), were extracted to underscore and/or complement the quantitative (see manuscript, lines 174-179).

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.
As explain before, we have revised and clarified our modelling approach and trust that this part of the manuscript is now much clearer. (see revised manuscript, lines 181-193).

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.

We are grateful to Dr. Mangham for highlighting these issues. While revising our manuscript we paid much more attention to present results without interpretation, whereas the most interested issues are now discussed in greater detail (see revised manuscript, lines 181-351).

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).

This issue has been solved by revising the modelling approach and removing populations characteristic as fixed effect

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.

Our study was conducted in a single health facility and RDTs for HIV testing were available before introducing RDTs for malaria, which is the key focus of current paper.

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?

Our qualitative results revealed the following issues. First, “fear” is primarily related to the results of an RDTs and might be explained by people’s prior experience with RDTs for HIV. With regard to “dangerous”, it is meant in the sense that it can do more
harm than good and could even affect people’s well-being. Lack of trust is only related to the RDT results. Taken together, these different terms might indeed be related, but we used the themes in our context to describe a specific feeling in relation to obtaining a finger prick sample and subjecting the sample for an RDT for malaria.

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).

We have now carefully revised the entire manuscript, placing particular emphasis on clarity, English grammar and style.

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”.

Both points have been clarified. We refered to perform an RDT for HIV before conducting an RDT for malaria. We have replaced “HIV recognition” with “knowledge about HIV” (see revised manuscript, table 3 line 559).

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. After considerable thinking we decided to remove this term altogether, as other readers too might be at unease if our study were termed a “socio-antropological study”. Clearly we used mixed methods, but we acknowledge that much further in-depth investigation would have been required for a truly socio-antropological piece of work.

Reviewer’s report
Reviewer: Zeno Bisoffi

Reviewer’s report:
2. Are the methods appropriate and well described? No. The methods appear really fuzzy, and this section needs to be substantially improved before submitting to this or to another journal. It is not at all clear what the sampling criteria were nor if the 100 persons (quite small sample in my opinion, by the way) were selected randomly.

As detailed in our responses to referee #1, the methods section has been revised considerably and we believe that clarity has been improved.

3. Are the data sound? No. For example, at bottom of page 6, one would understand that 83.3% of Bozi people and 16.7% of Yoho people were not favourable to RDT, but then, when one looks at table 1, one discovers that it is not so. There are several other examples.

All table are revised and the clarity is enhance
6. Are limitations of the work clearly stated? No.

We thanks Dr. Bisoffi for highlighting this issue. Consequently, we have now added a section in the discussion that summarizes key limitations as follows: “Our study has some limitations. First, the sample size is relatively small (100 RDTs provided free of charge, and 100 people interviewed) without formal sample size calculation. Second, our cross-sectional survey was carried out 2 months after the introduction of RDTs for malaria, which might have introduced some recall bias. Clearly, our study was designed as an exploratory piece, and hence larger-scale studies should be undertaken to assess the full validity of the findings reported here. (see revised manuscript, line 346-351).