Author's response to reviews

Title: Willingness to use a rapid diagnostic test for malaria in a rural area of central Côte d'Ivoire

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Author’s response to reviews: see over
Dear Editor

We refer to your e-mail dated 10 August 2012 and thank you very much indeed for sending us a second set of peer-review comments from the initial two referees who were willing to re-evaluate our revised manuscript entitled “Willingness to use a rapid diagnostic test for malaria in a rural area of central Côte d'Ivoire” (your reference no. MS: 956810926611895). We are deeply grateful to both referees for all their efforts and for offering additional sets of constructive comments and helpful suggestions.

We have now further revised our manuscript in light of the referees’ comments and our point-by-point response is given below. We clearly indicate how and where in the manuscript (line numbers) changes have been made. To readily assist you in tracking our changes made, we used blue ink to highlight our changes.

We very much hope that our further revised manuscript meets your and the external referees satisfaction. We look forward to hearing from you again and remain.

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

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Reviewer: Zeno Bisoffi
Reviewer's report:
I found the paper greatly improved, easier to understand, much clearer in the definition of the scope and methodology and of the results. Overall, a nice paper that I enjoyed reading and that proves how a qualitative and quantitative approach can be usefully combined to provide excellent pieces of information. The language has been much improved, too.
Compared to my previous report, I would now therefore answer YES to all points of the checklist.
We are grateful to Dr. Bisoffi for his generous comments and for the additional suggestions made, which helped us to further improve our manuscript.

Major essential revisions:
I remain with only one major doubt, as regards the weaknesses duly acknowledged by the authors in this version, and specifically at lines 347-348 (sample size). While I do not see any problem with a convenience sampling in this case, what is not at all clear to me is why the number of interviewed subjects should correspond to the number of tests available. As about 2/3rd refused to be tested (unless I have wrongly assumed), using all the available tests would have provided about thrice as many subjects for the interviews, giving more robustness to the conclusions. I would appreciate an explanation, that could simply be related to time or budget constraints (which I would perfectly understand…).
This point is well taken and we shall keep it in mind in our future research. In the current instance, we made available 100 RDTs for malaria diagnosis at the health center. Within 2 months, 100 people who visited the health centre who met our inclusion criteria were offered an RDT. However, only about one-third of the patients were willing to have an RDT for malaria performed, and hence we made every effort possible to trace these 100 people for an interview to find out, among other issues, why they were willing or refused to have an RDT.
for malaria performed. This issue is now made clearer in the revised version of our manuscript. 
(see manuscript, lines 142-151).

Minor essential revisions:
1. Lines 207-208. If I look at Table 2, it seems that responders were not actual patients with suspected malaria, as it would appear from the text. The referee is right, but it is important to note that most of the people who sought care at the health centre and who were willing to have an RDT for malaria performed were presumptive malaria cases (84.8%) (see revised manuscript, lines 211-213).

2. Line 231. The high proportion of patients declaring fear of the test result, concerning malaria, is surprising, and I would appreciate some comments in the Discussion. Normally, most/all febrile patients would be diagnosed/treated as malaria cases on clinical grounds, thus I do not see why they would fear this to be confirmed by a test...
We were also surprised by this finding. However, in our case, we think that fear to get an RDT for malaria performed might have been linked to some confusion of RDTs that had been available for HIV testing at the health centre before our study introducing RDTs for malaria. We speculate that people might have heard some stories about the hardship and fear while awaiting results from rapid testing for HIV/AIDS. Our finding might therefore highlight the importance of setting- and context-specific information, education and communication (IEC) and good quality of patient-medical staff interactions. This issue has now been made clearer (see revised manuscript, lines 329-341).

3. Line 276. I would rather say “do not necessarily agree…”, otherwise such a statement should be corroborated by citations. We followed the referee’s suggestion and have amended the manuscript accordingly (see revised manuscript, lines 281-28).

4. As a last comment, it is surprising that when we did our study in the neighbour country of Burkina Faso (your citation n. 51), we did not have any problem of refusal to do a blood test (just a few refusals to give the informed consent over more than 5,000 patients…), and generally, our impression was that most of them were rather happy to have a test done at no cost (though our study was not designed to assess willingness). The high proportion of refusals could have been related to local problems such as a bad reputation of the health centre (as it is somehow hinted by the authors), therefore I would suggest that this further limitation of the study (that is, the difficulty of generalizing finding from a single health unit) be added to the discussion.
We thank the referee for sharing his personal experience with us. We have now added a sentence and mention this potential additional limitation of our own study (see revised manuscript, lines 365-367).

Reviewer: Lindsay Mangham
Reviewer’s report: The revisions that have been made so far have strengthened the paper, though I think there are some areas which some further edits would be required to improve the clarity of the study findings. I hope the comments below are helpful in presenting the results.
We thank Dr. Mangham for her second set of detailed and most useful comments and suggestions.

Major Discretionary Changes
Line 36: I don’t think there is good evidence on the use of malaria RDTs in reducing malaria mortality in most parts of the world. Certainly they offer potential, but it is my understanding that this has not yet been achieved. The respective sentence has been reworded (see revised manuscript, lines 33-35).
Lines 46-56: I find some of the results a little difficult to follow – for example you say less than half (44.4%) complied with RDTs – but you also report 34 of 100 considered RDTs favourable (by which I think you mean they reported they were willing to undergo an RDT for malaria).

Indeed, we found that only 34 out of 100 patients seeking care at Bozi health centre were willing to have an RDT for malaria performed. However, among those who reported that blood is a sacred fluid, the percentage of people who were willing to undergo an RDT for malaria was considerably higher. This has now been clarified in the Results section of the Abstract (see revised manuscript, lines 45-47).

Lines 151-160: I find the description of the study population a little confusing – am I right in thinking that the information in Tables 1-4 refers to a survey of 100 patients that were offered an RDT at Bozi health centre and then in-depth interviews were conducted with different types of provider to get their perspective on some key themes.

We present results from those 100 people who sought care at the Bozi health centre and were offered an RDT for malaria. However, only 34 of these 100 people were willing to have an RDT for malaria done, whereas the remaining 66 refused. This point has now been clarified both in the Abstract and in the main body of the manuscript (see revised manuscript, lines 9-47, 143-152).

Lines 172: I am not a statistician, but wondered whether the use of a mixed effects model makes sense when there are only two groups at level 2 (Bozi and Yoho)?

We do believe that our analysis is correct. There are differences between the two villages (see village characteristics, Table1), and hence we used a random effects model to adjust for these village differences. Consequently, no further action has been taken.

Lines 186-190: Greater care may be needed in describing differences by demographic group. For example you refer to the percentage of men aged 14-24 as 53.3% - but in Table 1 53.3% seems to refer to both sexes. Also I note from Table 1 that differences by sex, age, number of children and religion were not statistically significant, but this wasn’t clear from the description.

We thank the referee for reading our manuscript so carefully. The respective sentences have been carefully reworded (see revised manuscript, lines 188-190, 194-199).

Lines 190-194: According to Table 1 Muslims were not more willing to have a malaria test than Animists.

The referee is right, and hence we have revised the respective sentence (see revised manuscript, lines 194-196).

Lines 196-208: Having a better description for the categories would help the reader interpret the findings and whether some of the conclusions you draw make sense. At present, some of the categories used in Table 2 are unclear – for example I’m not sure what is meant by “Vital Blood” “blood is life”, or “Justification”. Also I’m not certain how to interpret “reason for blood examination”, its different categories and how this relates to use of HIV tests at the facility when I refer to the data in Table 2.

We considered this point carefully and have now replaced “vital blood” with “blood keeps life in the body” (see revised manuscript, lines 205-206). Moreover “Reason for blood examination” has been replace by “Reason to have a blood examination performed or not” (see revised manuscript, Table 2). Finally, “Justification” has been replaced by “Justification to do blood testing by suspected malaria patients” (see revised manuscript, Table 2).

Lines 207-208: Please check the interpretation – I’d understood the results to show that 84.8% of patients that agreed to the malaria RDT perceived a need for patients to be tested for malaria – but that’s not what you write.

What we meant is the following: “A high percentage of people with suspected malaria were willing to have an RDT for malaria performed (84.8%)” (see revised manuscript, lines 209-210).
Lines 209-223: In presenting the qualitative findings from the in-depth interviewers with providers alongside the patients’ perceptions need to be careful not to muddle who said what. Rather than report on patients, then traditional healers then patients again it might be easier to follow report on patients’ perceptions and then compare or contrast to views of providers.

In response to this important point, we have rewritten this part of the manuscript and feel that clarity has been enhanced (see revised manuscript, lines 209-225).

Lines 226-229: Please check the explanation – are you saying that both 19.0% and 72.7% of patients that were found the test useful? I find the current explanation quite confusing.

In the multiple choice questions, patients who were in favour of an RDT found it useful (only 19.0%), but most found it a useful tool for malaria diagnosis (83.3%) (Fisher’s exact test, p <0.001). When considering utility, 85.1% of patients rejecting an RDT found it of no utility ($\chi^2 = 30.39, p <0.001$) (see revised manuscript, lines 228-231).

Lines 232-234: Please check explanation – in reading Table 3 if was apparent to me that HIV test referred to knowledge of HIV status.

We have replaced “HIV test” with “HIV status” (see revised manuscript, Table 3, lines 573).

Lines 251-254: I found the interpretation difficult to follow – do you mean people were more willing to accept if they were not fearful or were unsure if they were suffering from malaria?

This is exactly what we meant, and hence no further action has been taken (see revised manuscript, lines 231-234).

Lines 259-264: Again I think improving the description of the categories might help the interpretation – I find what you write about a willingness to undertake an RDT and acceptance and opinions about HIV test difficult to follow.

We have carefully reworded this paragraph to enhance overall clarity (see revised manuscript, Table 4, lines 258-267).

Discussion: As a general comment, I think the structure and logic of the points made in the discussion could be improved. In places the discussion becomes speculative, in which explanations are given which do not necessarily follow from the results presented. For example, the discussion refers to perceptions of quality of care and perceptions about community health workers, neither of which were referred to in the results. Similarly, the discussion emphasizes differences by religious group or urban/rural areas, though I’m sure whether the evidence presented elsewhere was as conclusive. In other places, it is uncertain which findings refer to this study and which refer to the broader literature.

Thanks you for these observations. Each point mentioned above has been carefully addressed.

Line 271: resistance of what?

We were talking about drug resistance and this has now been made explicit (see revised manuscript, line 274).

Lines 285-287: no information about the perceptions about the quality of care available is presented in the results.

Concerning quality of care, this was primarily based upon direct observations on while we worked with the health centre. We have now added “However, based on direct observation by researchers, Bozi residents largely negated help-seeking, as they perceived quality of care as low (see revised manuscript, lines 292-293).

Lines 285-290: are there differences in the demographic characteristics of populations in Bozi and Yoho that may also contribute to willingness to use RDTs – e.g. education which would found to be a significantly differ between those that were and were not willing.

The following sentences have been added: “Educational might play an important role. Indeed, the majority of household heads attended primary school in Yoho unlike Bozi. In addition the
percentage of children from mothers attending primary education was higher in Yoho. [36]. This facilitates belief on some unreal and/or improper things said about malaria (see revised manuscript, lines 285-288).

Lines 294: You refer to two key determinants – but I’m not sure whether you refer to this study or other studies. Please review.
We referred to others studies and this now clearly mentioned (see revised manuscript, lines 294-299).

Lines 347-352: As I understand it 100 RDTs were made available and 100 patients were surveyed. So did you survey all patients that were offered RDTs or only a sample of them (i.e. were only 100 patients offered RDTs and did 66 RDTs go unused?). How did you go about finding the patients that were offered an RDT? Any potential for selection bias at stage patients were offered an RDT and in the sample that were followed up?
People were interviewed at home as mentioned in the method. Using the name, phone number and approximative household location, the key informant helped us to identified people. We think there was not selection bias because all people who presented with a fever regardless of whether or not they had an RDT performed were registered and interviewed. Within 2 months we got 100 patients registered with only 33 malaria RDT performed. The study focused on these 100 patients registered (see revised manuscript, lines 148-149).

Minor Discretionary Changes
Line 3: In the title I suggest replacing “Willingness of using a ...” with “Willingness to use a ...”
Changed as requested (see revised manuscript, line 3).

Line 85: It would be more accurate to say RDTs provide reliable results within 15-20 minutes, than a couple of minutes.
We have replaced “A couple of minutes” with “15-20 minutes” (see revised manuscript, lines 84-85).

Line 163: may be more accurate to refer to data analysis since it includes qualitative data analysis, which by its nature will not be statistical.
“Statistical analysis” has been replaced by “Data analysis” (see revised manuscript, line 162).