



SDI Review Form 1.6

PART 1:

Journal Name:	International Journal of TROPICAL DISEASE & Health
Manuscript Number:	MS: 2012/IJTDH/2110
Title of the Manuscript:	Evaluation of field performance of insecticide-treated mosquito nets in north-western Burkina Faso

REVIEWER NUMBER 1	
Rev 1	Introduction (Is the problem/objective of this study original, important and well defined?) <i>The objective of this study is not original to the international community but it may be of importance to the malaria control programme of Burkina Faso. Furthermore in the Introduction section, the authors make the assertion that the impact of pyrethroid resistance is not well understood. The following two references contradict this statement:</i> <i>Abilio AP, Kleinschmidt I, Rehman AM, Cuamba N, Ramdeen V, Mthembu DS, Coetzer S, Maharaj R, Wilding CS, Steven A, Coleman M, Hemingway J, Coleman M. 2011. The emergence of insecticide resistance in central Mozambique and potential threat to the successful indoor residual spraying malaria control programme. Malar J. 2(10):110.</i> <i>Marcombe S, Darriet F, Tolosa M, Agnew P, Duchon S, Etienne M, YpTcha MM, Chandre F, Corbel V, Yébakima A. 2011. Pyrethroid resistance reduces the efficacy of space sprays for dengue control on the island of Martinique (Caribbean). PLoSNegl Trop Dis. 5(6):e1202.</i>
Response	We agree that there is conflicting evidence regarding the public health impact of the emerging pyrethroid resistance in endemic areas, in particular with regard to malaria control in SSA. However, this was already taken care of by giving the reference of the review paper from Ranson et al (2011). To make it more clear that there are already public health problems with pyrethroid resistance and malaria in Southern Africa, we have added the sentence: “However, although an impact of pyrethroid resistance on malaria control programmes was shown in Southern Africa (Abilio et al. 2011), the public health impact of resistance development of malaria vectors against pyrethroids is currently not fully understood (Ranson et al. 2011).
Rev 1	Materials & methods (Kindly comment on the suitability of the methods. Sufficient details of the methods should be provided to allow peers to evaluate and/or replicate the work) <i>The methodology section lacks detail but the authors’ direct attention to a previous study where the methodology is discussed. It would be</i>



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	<i>useful for the authors to include a summary of the key techniques and protocols that were used for the study.</i>
Response	As this is a short communication, it is impossible to go into the details of the methods. However, in addition to the details of the methodology which were already briefly given, we have now added a few more essential details.
Rev 1	Results & discussion (Kindly comment on: 1. Are the data well controlled and robust? 2. Authors should provide relevant references during discussion. 3. Discussion and conclusions should be based on actual facts and figures. Biased claims should be pointed out. 4. Are statistical analyses must for this paper? If yes, have sufficient and appropriate statistical analyses been carried out?) <i>The data is described in a single table where simple statistics was applied. It was stated that statistical analysis was done but there is no evidence of this. It would strengthen the study to compare all the variables by sub-area. This would highlight where the main problems are. Also, the data for Deltamethrin content in Table 1 is nonsensical. The conditions under which the nets were use has not been described so the reader is left wondering what "real life conditions" mean.</i>
Response	To clarify the point about statistics, the sentence has been simplified in the methods section to correspond better to the slightly expanded sentence in the results, namely: The Differences in mosquito mortality between sub-areas disappeared after controlling for confounding variables (ITN age, number of washes, and time since latest impregnation)." With regard to the comments on the results presentation, we do not understand the argument that the deltamethrin content data are nonsensical. The net characteristics under real life conditions (age, washing status) are given in table 1.
Rev 1	- Conclusion (Is the conclusion supported by the data, discussed inside the manuscript? Conclusions should not be biased and should be based on the data, presented inside the manuscript only) - Are all the references cited relevant, adequate? Are there any other suitable current references authors need to cite? - This form has total 9 parts. Kindly note that you should use all the parts of this review form. <i>The conclusion does not follow WHO guidelines for insecticide susceptibility. WHO recommends that an insecticide be discontinued if efficacy is less than 80%. Only the area where no cotton is grown meets this cut-off threshold.</i>
Response	The WHO guidelines apply to insecticide susceptibility studies under controlled conditions. As this study has been



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	performed under real life conditions with an ITN age range of up to 5 years, the 24 hour mosquito mortality results can't be interpreted as suggested by the reviewer. However, to further clarify this, we have modified the corresponding sentences in the last paragraph of the results/discussion section and the conclusion paragraph.
	REVIEWER NUMBER 2
Rev 2	• This paper is a well-written very brief surveillance report describing ITN insecticide properties in a region of Burkina Faso. Conclusion: Deltamethrin remains viable several years after distribution in ITN in an area that includes deltamethrin use for agriculture. Insecticide resistance has been one of many questions about the sustainability of ITNs. The data was collected in 2008, so results here are dated. • Materials & methods. Methods suitable. The paper does a great job of describing methods given the economy of the presentation, however, details about how a random sample was selected would be helpful. For example, it appears only a minority of households had bednets, were they mapped and then randomly selected? What properties of these households? (Normally a Table 1 socio-demographic presentation issue.) How were towns chosen? Also, authors discussed project that distributed those nets. More detail needed. • Results & discussion. Observational study. No mention if nets were dipped, or in fact, how maintained, other than they were washed. This reader disappointed that so many other issues related to use and sustainability were not addressed in the paper (cf. http://www.who.int/malaria/publications/atoz/itn_r62.pdf). Understand not appropriate for purpose of paper as developed here, but perhaps in other publications? Cite? More could have been made of controversies about deltamethrin persistence and resistance in agricultural areas to draw attention to the paper and its conclusion. Overall, not sure of level of interest in a paper written this narrowly. • Citations minimal but adequate for purposes of short communication.
Response	To further specify the recruitment procedure as requested by the reviewer, we have now enlarged the corresponding sentence in the methods section which now reads: "In brief, a total of ninety used ITNs were collected in households selected at random within each locality (30 per sub-area) using the register of the Health and Demographic Surveillance System managed by the CRSN." Concerning other issues related to ITN use (e.g. epidemiological, biological, environmental, economic, operational, social factors), we agree that considering them all would be useful, but this can't fully be addressed in the frame of a short communication. With regard to references on pyrethroids and malaria, we have given some of the key references in the introduction section.
Rev 2	Details of sampling: site selection, household selection, refusals? Some detail of original project and activities that could confound results.



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Response	See our responses above.
Rev 2	In general, not sure of level of interest in a paper written his narrowly about insecticide properties in 2008. This is just a small part of the whole sustainability issue. Publication should reflect journal policy of publishing brief field reports. Could spend a few sentences/citations on issues related to deltamethrin use in areas where used for agriculture. Any update 2012?
Response	As mentioned, we have given already some key references on agricultural pyrethroid use and malaria in the introduction section.
REVIEWER NUMBER 3	
Rev 3	Introduction: The authors need to give more details on the importance of their study. As it is now they have made an assumption that readers of their work have prior knowledge on the current malaria control methods that are in use. This needs to be explained for the benefits of scholars, students and policy makers who are the likely beneficiary of this study. There is need for an explanation why LLINs are preferred to conventional ITNs, giving relevant references. On the pyrethroid resistance, the authors need to evaluate literature on the work of Anto et al 2009, in Kassens-Nankana district of Ghana, Rubaihayo J et al 2008, in western Uganda and Vulule et 1994 in western Kenya.
Response	Again, this is a short communication which can't be very detailed on all malaria control methods. However, we have now more specified malaria in the first sentence of the introduction section. Moreover, we think that we have already included the most relevant references on the pyrethroid resistance issue, in particular the recent review paper by Ranson et al.
Rev 3	Methodology:1. Study area; the author needs to give the geo-reference of the study area. 2. At line 50 and 51 the author states that the ITN evaluation method used was similar to the one used by Mueller <i>et al</i> 2002, for the benefit of readers who may not be able to access the reference, the author needs to describe the method in full. 3. A questionnaire was administered inquiring about ITNs characteristics, it is important to describe the kind characteristics that the author sought from the respondents that were relevant to the study. 4. The author needs to describe the method used to determine insecticide concentrations on nets and reference the original author of the method and should not give a reference of the study that used a similar method. 5. The author should give the reference to the WHO standard cone bioassay method used to measure ITN efficacy. 6. Finally, even though the statistical analysis package used was appropriate, some variable models used were not relevant to the study e.i why were variables like time since latest impregnation and number of washes since latest impregnation used as variables yet the nets that were used in the study



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	were PermaNet an LLIN that are factory treated and do not need further impregnation in the field.
Response	Point 1: A geo-reference has been added in the text of the methodology section. Point 2-5: These points have been addressed already in the frame of what is possible for a short communication (see above and marked changes in the text). Point 6: These variables were included because there are relevant ITN characteristics that influence mosquito mortality outcome. Some users in the field impregnate their ITNs as our questionnaire data showed.
Rev 3	RESULTS AND DISCUSSION. The results are very brief, could do with more. For example with the large number of mosquitoes involved it would have been more informative if WHO tube tests would have been carried out using discriminatory doses of Deltamethrin to confirm the susceptibility status of <i>Anopheles</i> in North western Burkina Faso. More information needs to be given on the <i>Anopheles</i> strains used as negative and positive controls. The discussions are also very brief, with only one reference. There is need for more discussions on the authors findings and how they relate similar studies done elsewhere giving relevant references. The authors should consult the current WHO publications/reports on the same.
Response	These comments have now largely been addressed (see above). Again, in a short communication we have to be very concise. The reference referring to the WHO cones has now been added.
Rev 3	CONCLUSION: Before this conclusion is made, the author should familiarise himself with various WHO reports on definition of insecticide resistance particularly WHO/CDS/CPC/MAL/98.12. The standard criteria for determining resistance are: <80% mortality = resistance confirmed; 80-98% mortality = resistance needs to be confirmed with further testing; 98-100% mortality = population fully susceptible.
Response	We do not fully agree as this has been a study under real life conditions (see our response above). However, the corresponding wording has been changed to better address this fact.
Rev 3	All the section highlighted above: introduction, methodology, results/discussion and conclusion. This is an important subject matter which the authors should be encouraged to publish.
Response	We agree with this comment.
REVIEWER NUMBER 4	
Rev 4	Reliable data on evaluation of field performance of Insecticides Treated bed Nets (ITNs) presented here are very valuable, especially in areas that remain underdocumented such as those highly endemic for malaria. This work is so great and important that it needs to be



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	acknowledged. However, I have a number of concerns about it that I would like to express through the following comments: First, It is true that large scale use of ITNs is the strategy recommended by the WHO to limit malaria transmission, but Pyrethroid resistance is widespread across malaria vector populations, particularly the Kdr mutation which confers cross-resistance to all pyrethroids and may threaten the efficacy of ITNs. For this reason, it would be interesting if you could state in which conditions the nets have been distributed in Burkina Faso particularly in the study areas, giving data on the nets coverage, usage, time of distribution and conditions of acquisition. It is also good if you could describe the effect of bed nets difference of distribution between the three study areas, then tell whether Deltametrin is the only Pyrethroid insecticide approved for treating nets in study area as well as whether any study has been conducted about the impact of nets distribution on the malaria transmission in Burkina Faso
Response	We agree that this information is important. However, given the limited space in a short communication we have referred to the appropriate and informative references.
Rev 4	Secondly I observed that there not any data concerning the vector presence and distribution in the study areas such as number of <i>An. gambiaesl</i> collected, variability of <i>An. gambiaesl</i> in the tree areas in terms of temporal and geographical distribution and/or prevalence
Response	Unfortunately such data have not been collected in the area in recent years.
Rev 4	Last, I observe that the discussion is rather weak and not convincing, with a number of inferences that are not directly supported by the data. Therefore, in light of all the above, I would encourage the authors to seriously re-think the interpretation of their data, paying attention not to over-interpret their findings and keeping on focus
Response	This has now been done (see responses above).
Rev 4	1) Abstract a) RESULTS: To specify the specie of mosquito (<i>Anopheles gambiaesl</i>.) b) To harmonize ITNs and ITN (spelling)
Response	a) added b) harmonized. ITNs, a plural, is used in general but ITN is still used when preceding a noun
Rev 4	2) Introduction • Line 43 = Mueller et al.2007 : reference not cited • Line 43 and 44 –coverage was 28% in 2007 (Mueller et al.2001) = study in 2007 and reference 2001 • Line 44- re-examine (figure):1



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	- Line 47 : number of figure - Line 48 : countries bordering the map of the study area would be very much welcome - Line 62 and 63 : error
Response	- Line 43 = Mueller et al.2007 : reference not cited / REF ADDED - Line 43 and 44 –coverage was 28% in 2007 (Mueller et al.2001) = study in 2007 and reference 2001 /CORRECTED - Line 44- re-examine (figure):1 /CHANGED - Line 47 : number of figure /ONLY ONE FIGURE SO NOT NUMBERED - Line 48 : countries bordering the map of the study area would be very much welcome - Line 62 and 63 : error / we don't understand where there is an error
Rev 4	3) RESULTS AND DISCUSSION - Line 69 : number of table - Line 70 : number of table - Table : Please also include data for An gambiae sl. mortality after 24h
Response	There is only one table so it is not numbered. Mortality data is included
Rev 4	REFERENCES - Line 124: <i>Anopheles gambiaes.l.</i> - Line 127: <i>Anopheles gambiaes.l.</i>
Response	Changed to italics



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PART 2: Review Comments

	Reviewer's comment	Author's comment (if agreed with reviewer, correct the manuscript and highlight that part and write here 'Corrected'/ if not agreed, give suitable justifications)
<u>COMPULSORY REVISION</u> comments	All the weaknesses highlighted above need to be taken into account and adequate changes made before this manuscript can be published.	Addressed as described
<u>Minor</u> REVISION comments		
<u>Optional/General</u> comments	The manuscript is generally well written.	