Author’s response to reviews

Title: Cost-effectiveness analysis of malaria chemoprophylaxis for travellers to West-Africa

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Author’s response to reviews: see over
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Dear Prof. Akintunde Sowunmi,

Thank you very much for the further review of our manuscript „Cost-effectiveness analysis of malaria chemoprophylaxis for travellers to West Africa“. The new text sections/deleted sections are shown in blue. We submit a revised manuscript according to the reviewer’s recommendations and look forward to your consideration for publication in BMC Infectious Diseases.

Yours sincerely,

PD Dr. Patricia Schlagenhauf, corresponding author

PD Dr. Pat Schlagenhauf
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REVIEWER COMMENTS – 2nd round

Reviewer's report
Title: Cost-effectiveness analysis of malaria chemoprophylaxis for travellers to West-Africa
Version: 2 Date: 15 July 2010
Reviewer: Dirk H Müller

Reviewer's report:
The manuscript of Widmer et al is improved. Several remarks mentioned by the reviewers are revised appropriately. However, there are still some points to clarify:
1) Abstract (background, last sentence): suggest taking this sentence as the second sentence.
   Authors’ response: We have changed the position of this sentence as requested.

2) From my point of view, information provided for Supracycline and Malarone is still too much and also confusing for readers. Why not make some remarks about these drugs in the introduction. If I understand the model correctly, the analysis was run with Mephaquin. Data in the method section should be limited to data which is important for the cost-effectiveness analysis. If Supracycline and Malarone were not evaluated in terms of cost-effectiveness, they should only be mentioned at the beginning or in the discussion part. Maybe I am wrong but in my opinion, the 2 other drugs here are overrepresented. The same is for figure 1. Please clarify.
Authors’ response: We have reduced the text sections on Supra-
cycline and Malarone in the “Methods” as much as possible (de-
leted sections in blue). However in the context of the manuscript,
at least some information on comparator regimens is of impor-
tance as these alternative regimens are used as frequently as me-
floquine (in some cases more often) and their position in the pri-
ority chemoprophylaxis line-up needs to be clear. Furthermore,
the cost-effectiveness data for the three regimens are compared
in Table 5 and this is useful for other countries with other patterns
of chemoprophylaxis prescription.

3) Methods, para 5: There is an additional sentence provided (“because
there is no accepted willingness…”). To compare the results of an
analysis with other analyses does not require knowledge about willing-
ness to pay. In addition, the authors have compared their results with
those of others (discussion). This is confusing.

Authors’ response: We agree and will remove this sentence as re-
quested.

4) For the probabilistic sensitivity analysis, the authors have chosen a
triangular distribution. I am not much experienced in statistical details
but this distribution appears to be peculiar in this case. The reason(s)
for using a triangular distribution have to be provided.

Authors’ response: Parameter estimates which are employed in a
cost-effectiveness model are not known with certainties, hence, it
is essential to explore the consequences of such parameter un-
certainties for the outcome of the analysis. Conventional univari-
ate sensitivity analysis, where individual variables are varied
while maintaining all other parameters at the baseline, is expected
to underestimate the uncertainties, as in reality, parameters do
not change in separation. Therefore, we performed a probabilistic
sensitivity analysis, which involves certain distributions for model
parameters to represent the uncertainty in their estimation and
running a Monte Carlo simulation to select the values at random
from those distributions (REF: Critchfield GC et al, Probabilistic
sensitivity analysis methods for general decision models, Comp

We agree that triangular distribution was not the optimal distribu-
tion for our analysis. In line with your suggestion we have
amended the probabilistic sensitivity analysis and used beta-
distributions for all parameters assessed in the PSA. In addition, a
explanatory reference on this method was added (REF 24: Briggs
et al 2002).