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Comment

Interactive comment on “An eco-hydrologic model of malaria outbreaks” by E. Montosi et al.

A. Viglione (Referee)

viglione@hydro.tuwien.ac.at

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This paper demonstrates that surface hydrology has a significant role in the dynamics of malaria outbreaks. By coupling a hydrologic soil water content model to a model of malaria transmission (and making some simplifying assumptions) the temporal variability of malaria cases can be explained better than by accounting for temperature and rainfall alone. The paper is enjoyable, well written, concise and inspiring. It is a nice example of using parsimony in hydrology, with assumptions clearly stated and a discussion on the meaning of model parameters and their fitted values. Consequently, I am definitely supportive of the publication of this paper in HESS. I have a couple of comments that should be addressed before publication, but since they mostly involve additional discussion, the resulting revision should be minor.

1) Maybe I missed it, but it is not completely clear to me what is the essential difference

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between the statistical transfer function and the ecohydrologic model. In other words, why is the transfer function also considered in the analysis?

2) The spatial aspect is somehow missing in the paper. I agree that the focus is on temporal dynamics and that there is not much information to also look in detail at the spatial variability. However a brief discussion related to the following questions could be useful: are there differences between the three regions studied? Are these differences reflected in the results?

Minor comments:

Page 2833, line 6: "ability to predict" rather than "predictability"?

Page 2835, line 18: Limpopo seems different from the other two locations by looking at the insets of figure 2, i.e., less seasonal delay. Is there an explanation for this? (Note that for Limpopo also the results of the analysis at page 2843 are different from the other 2 regions).

Figure 2: would it be useful to insert temperature (and insets of the seasonal evolution of temperature vs. malaria cases) in figure 2? Or to add a separate figure?

Page 2835, line 20: in this preliminary inspection, do you use the daily meteorological data or the monthly aggregations? For instance, does a single day above 39 deg produce a decrease in the monthly number of malaria cases or is it a entire month with maximum daily temperatures on average greater than 39 deg? Same thing for the anomalously high precipitations.

Page 2836, line 8: this sentence seems to be inconsistent with line 20 at page 2835 where it is said that daily maximum temperatures over 39 deg are followed by a decrease of malaria cases. Is it because now the monthly means are considered while the daily values were analysed in the preliminary inspection?

Page 2837, description of Eq. 1-4: (just a suggestion) maybe you could add the units when you describe the variables/functions. For example, the unit of the mosquito

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growth rate is [1/days] etc.

Page 2838, assumption 1: does this assumption imply that demographic growth is neglected as well?

Page 2839, line 2: it would be clearer if you add $dM(t)/dt=0$ to the sentence

Page 2840, eq 16: just a typo, the symbol for recovery from infection is different from eq 4

Page 2840, assumption 6: in which cases is this assumption made? I guess always in the paper. If so remove the "if" statement and explain why do you distinguish between Assumption 3a and 3b.

Page 2842, line 17: (just a suggestion) in order to show the convergence of the MCMC algorithm, wouldn't it be better to show the histogram obtained from multiple runs (with different initial conditions)? Or with the sentence you just mean that the obtained distributions are unimodal?

Page 2842, line 25: considering two months lag times is too much? At page 2833 line 30 it is said that correlation of malaria incidence with rainfall amounts with several months of time lag is possible. Is it just the case for the seasonal effect?

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