Hydrology and Earth System Sciences Discussions

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

E. Montosi et al.

elena.montosi@unibo.it

We thankfully acknowledge L. Brocca, A. Viglione, I. Andrés-Doménech and the anonymous Referee #3 for providing constructive reviews and very useful comments on the original version of the manuscript. The referees raise a series of good points that helped us identifying a few technical issues that need to be adjusted and parts of the presentation that need to be clarified. We would like to take advantage of the interactive discussion for illustrating how the revision process is addressing the Referees' major points to improve the technical soundness of the study and the quality of the presentation. Our comment is divided into four different sections that address separately comments made by L. Brocca, A. Viglione, I. Andrés-Doménech and the anonymous Referee #3.

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1   Reply to the comments by L. Brocca:

We really appreciated L. Brocca short comment and his idea of a possible future development of the research we proposed with this paper.

He suggested considering satellite soil moisture data as a possible alternative in order to directly estimate soil moisture without invoking a water balance model dedicated to this purpose. We agree with him, as satellite data can be valuable and would add further novelty to the proposed research; furthermore, satellite data would be perfectly suited to implement an early warning system that helps local health services to forecast malaria epidemic. While we are not planning to include the proposed approach in the present work as its original aim was to highlight the role that soil moisture has in driving malaria incidence, we do agree that the suggestion by L. Brocca is a great starting point for a future contribution. In the paper we now discuss how satellite data can be used to validate our approach, and that such a study will be subject of future research.

L. Brocca, moreover, raised some concerns regarding the clarity of the presentation. We would like to clarify that the values reported in Table 1 represent the Nash-Sutcliffe efficiency index between each model that we tested and the observed malaria incidence. As suggested by the reviewer, we will improve the presentation of these results by deleting the word correlation and by distinguishing between the cases: before and after the removal of the seasonality.

To clearly identify the novelty of our approach, the reviewer suggested us to add some more information regarding the results of previous studies carried out in the same region. To the best of our knowledge, studies linking malaria to climatic variables in the same area focus on two widely separated spatial scales: i) studies referring to the whole southeastern Africa, including Limpopo, Mpumalanga and KwaZulu-Natal as well (e.g. Jury and Kanemba, 2007 and Thomson et al, 2006) and ii) studies focusing only on the KwaZulu-Natal province (e.g. Kleinschmidt et al, 2001 and Craig et al, 2004). We cited all the above studies in the paper, but the paper by Thomson et

al, 2006, by highlighting how the relationship between malaria incidence and climatic factors was assessed. In the revised version of the manuscript we will make clearer that these previous results apply to the same study area.

Finally, as L. Brocca noticed, malaria incidence data are made available by the South African department of Health starting from July 1996; we mentioned it at line 25 of page 2834.

2 Reply to the comments by A. Viglione:

We are very grateful to the Referee for his positive judgment about our work.

The first major point raised by A. Viglione refers to the difference between two of the three ways we adopted to evaluate the association between climatic drivers and malaria cases; in particular A. Viglione noticed that the rationale behind our choice of the transfer function model was not clear. Actually, by looking at the monthly series of malaria and precipitation, we realized that there is a high seasonality not only in the data per se, but also in their standard deviations. To describe such highly seasonal dynamics, we developed the eco-hydrologic model by coupling a model of malaria transmission and a hydrologic model. Through a set of assumptions it was possible to reach the formulation given by the Eqs. (15) and (16), which are linear in both the state variables and the climatic factors (precipitation and temperature). Thanks to the linearity of this formulation, the seasonality can be removed and the anomalies can be analized. Therefore, with the eco-hydrologic model (Eqs. (17) and (19)), we describe the deseasonalized data by using a physically-based approach. However, we thought it would be interesting to assess the impact of the seasonality of the standard deviation of the data as well. Thus, we decided to develop a statistically based model that mimics the eco-hydrologic one, while allowing for a more rigorous statistical analysis, that is performed after both the mean and standard deviation of the data have been

deseasonalised. To better explain our rationale, we will make some changes in the revised text.

About the second concern of A. Viglione, we agree that a physical interpretation of the spatial variability of the results would be sound. However, it is indeed not straightforward to provide an interpretation for model parameter values. In order to obtain additional information on the topography and climate of the study area one may refer to the maps provided by the South African Department of Environmental Affairs and Tourism http://www.environment.gov.za/enviro-info/prov/intro.htm. The areas prone to malaria risk are located mainly in the low-lying parts of the three provinces where the climate is quite variable, as it is possible to assess from the maps of mean annual precipitation provided by the aforementioned Department. It is moreover possible to see from the insets of Fig. 2 that the seasonal incidence of malaria in the province of KwaZulu-Natal is almost double with respect to the other two provinces. This high incidence of malaria cases is reflected by the results we obtained. In fact, in Table 2 one can see that the m parameter, which is the soil water loss rate, shows a not so strong variability among the three provinces; we suppose that its value may be highly connected to the variability of the soil properties, but one may note that significant uncertainty affects the parameter estimates (see Table 2). On the contrary, the parameter b is more remarkably varying. In particular, a higher value is obtained for the KwaZulu-Natal province. As a matter of fact, b quantifies the sensitivity of malaria cases to rainfall. In our view the fact it assumes higher values in the KwaZulu-Natal province is possibly related to its coastal proximity, which implies the presence of a more humid climate. Moreover, wetlands and water-bodies are characteristics more frequent in the land cover map of this province, if compared to what is depicted in Limpopo and Mpumalanga maps. We will include the above qualitative considerations in the revised version of the paper.

We provide a list of the maps we were referring to in this comment:

- KwaZulu-Natal: mean annual precipitation: http://www.environment.gov.za/enviro-

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info/prov/kn/knrain.jpg; land cover: http://www.environment.gov.za/enviro-info/prov/kn/knlcov.jpg; topography: http://www.environment.gov.za/enviro-info/prov/kn/kn3d.jpg

- Mpumalanga: mean annual precipitation: http://www.environment.gov.za/enviro-info/prov/mp/mprain.jpg; land cover: http://www.environment.gov.za/enviro-info/prov/mp/mplcov.jpg; topography: http://www.environment.gov.za/enviro-info/prov/mp/mp3d.jpg

- Limpopo: mean annual precipitation http://www.environment.gov.za/enviro-info/prov/np/nprain.jpg; land cover: http://www.environment.gov.za/enviro-info/prov/np/nplcov.jpg; topography: http://www.environment.gov.za/enviro-info/prov/np/np3d.jpg .

Regarding the minor comments by A. Viglione:

- Page 2833, line 6: we will replace the word "predictability" with "ability to predict".

- Page 2835, line18: we think that the Referee is correct. It really seems that there is less delay in malaria occurrences for the Limpopo province. We will specify in the revised version of the paper that this outcome is probably due to different climatic behaviours.

- Figure 2: we agree that it is important to show the influence of both rainfall and temperature on the malaria incidence. Therefore we will add a figure that will refer to the relationship between malaria cases and temperature.

- Page 2835, line 20: the preliminary inspection was designed in order to compare daily rainfall and temperature data with monthly value of malaria incidence. We observed that daily maximum temperatures above 39 °C tended to be followed by a decrease in the number of malaria cases, meaning that months characterized

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by more than one day with high temperatures usually are followed by a decrease in malaria incidence. For what concerns the link between monthly malaria data and daily anomalously high values of precipitation, we observed that violent and heavy storms are followed by an immediate decrease of malaria cases, which is consistent with reports of declining malaria cases after flushing of the breeding sites (Wyse at al., 2007).

- Page 2836, line 8: we agree with the reviewer on this point. To avoid the inconsistency, we will specify that, in this case, we are dealing with monthly climatic forcing, while in the preliminary inspection we were always referring to the daily data of rainfall and temperature.

- Page 2837, description of Eqs. 1-4: we will follow the Referee's suggestion, by adding the units to all of the variables. Particularly, as we assumed as temporal resolution 1 month, α, used in Eq. (8), is expressed in 1/(months * number of infected individuals), γ is nondimensional, as highlighted in the definition $\widehat{M_I}(t) \cong \gamma \widehat{M}(t)$ just before Eq. (13), while μ, η, η_0, υ are all expressed in terms of (1/months).

- Page 2838, assumption 1: we are assuming that the number of infected is relatively small with respect to the total population (e.g., 1000 infected out nearly 10 million people in KwaZulu-Natal), so that the fraction of susceptible changes little through time and can be effectively approximated by a constant fraction (close to unity) of the total population. Hence we do not need to neglect the demographic growth.

- Page 2839, line 2: we agree with A. Viglione and we will add $\frac{dM(t)}{dt} = 0$, indicating not a steady value of the mosquito population, but an 'instantaneously-equilibrated' population, that is, M changes rapidly through time in response to the environmental fluctuations.

- Page 2840, Eq. 16: we agree with the Referee; we will change the symbol with v.

- Page 2840, assumption 6: we agree the sentence was not clear. We will rephrase it as "The effects of environmental conditions on mosquito growth rate are assumed to be linear, that is, $A(t) = a + bw(t) + cT(t)$". Also note that nonlinear effects can be linearized around a mean environmental condition, so that the parameters of our linear approximation can be interpreted as the coefficients of a Taylor expansion around the mean T and w.

- Page 2842, line 17: with this sentence we aim to explain that the posterior probability distributions for the parameters are unimodal, implying that these parameters take values in a limited area of the space of potential solutions. In this case we are not referring to the convergence of the MCMC algorithm, which is monitored through the R-statistic of Gelman and Rubin (1992) and with a visual inspection of the likelihood behavior of each individual chain. Thanks to this comment we realized that the paper did not mention the convergence of the MCMC algorithm, therefore we will change that sentence as follows: "MCMC algorithm convergence is monitored through the R-statistic of Gelman and Rubin (1992) and with a visual inspection of the likelihood behavior of each individual chain. To approximate the posterior probability density functions of the model parameters a total of 80.000 model evaluations are performed with DREAM; the first 40% of the samples of each chain are discarded and considered as burn-in, in order to evaluate the marginal densities of the parameters from the samples whose likelihood is close to convergence. Figure 4 shows the posterior probability distributions for the parameters of both models applied to the Mpumalanga data; it can be seen that they occupy a well defined region.".

- Page 2842, line 25: we have unsuccessfully tested if a two-month delay would provide more explanatory power. Because of the negative result of such a test,

we only report that lags longer than 1 month did not explain significantly more variability.

3 Reply to the comments by I. Andrés-Doménech:

We would like to thank the Referee for the positive comments.

For what concerns the spatial variability of the results, please see our reply to the analogous comment by A. Viglione.

Regarding the minor comments and typographic errors pointed out by the Referee:

- Page 2837, lines 5-16: α, is expressed 1/(months * number of infected individuals), while τ_I is measured in (months). We will add the missing definitions and units as suggested.

- Page 2837, lines 17-21: we will slightly modify these lines in order to include the definition of the parameter m as well, changing the text as follows "and $mw(t)$ is a linearized soil water loss function accounting for plant transpiration, surface evaporation, and deep infiltration, where m is a lumped soil water loss rate (with units of months^{-1})".

- Page 2838, lines 12-14: to clarify, we will re-phrase as follows: "Assumption 3a: when climate is the only limiting factor to mosquito emergence, the effect of $M(t)$ on the growth rate can be neglected, effectively approximating the exponential growth resulting from Eq. (7) with a linear growth with moisture- and temperature-controlled rate, which can be expressed as:".

- Page 2838, lines 15-18: we agree that some more explanation is needed. After Eq.(10) we will add the sentence: "where r is the maximum growth rate and K the carrying capacity of the mosquito population".

- Page 2829, line 1: we will adopt the sentence suggested by the Reviewer.

- Page 2840, lines 23-24: we will add the units to the values that represent the standard deviation.

- Page 2842, line 2: the Referee is correct, we have to use lower case $_i$ in the Eqs. (20) and (21).

- Page 2842, line 18: we agree with the Reviewer and we will maintain the description of the significant parameter uncertainty only at page 2842.

- Page 2844, lines 8-9: we thank the Referee for the suggestion and we will use "in the three time series".

- Page 2845, line 1: we will make use of the suggested sentence ". . . with available climate and malaria data.".

- Figure 2: we will specify in the caption that precipitation is depicted with dashed black lines, while the incidence of malaria is drawn with continue grey lines.

- Figure 2 and 3: we will try to improve the readability of the figure and we will add the units to the variables represented into the insets. We will also increase the size and resolution of the figures.

4 Reply to the comments by Anonymous Referee #3:

We thank the Reviewer for the positive comments and the constructive critiques. Our replies to each individual comment follow here below.

- Eq. 3: depending on the goal of the model, the total population may be assumed to have different growth and death rates. In our case, considering that the number of susceptible is about three orders of magnitude larger than the number of infected, it is reasonable to consider the total population with same growth and death rates, i.e., effectively static with respect to the dynamics of the infected population.

- Page 2837, line 25: by surface water storage we mean primarily soil moisture, but also seasonal ponds and rivers that might be breeding sites for mosquitoes. Over the large spatial scales we consider, we think this broad definition is reasonable. To clarify, we now explain what we meant.

- Pages 2837-2838: we will add an introductory statement: "In the following, the assumptions employed to reduce the full eco-hydrological model (i.e., Eqs. 1-4 for malaria dynamics and Eq. 5 for surface hydrology) to the linear model are described."

- Page 2838, Assumption 1 (reply to two comments): to clarify, we will change the sentence explaining the first assumption as: "Because the number of infected is much smaller than the total population in the considered areas, as a first approximation we can assume that the susceptible fraction of the host population, $\frac{H_S}{H_{TOT}} = 0$, is relatively stable through time. We also agree that Eq. 3 should hold under the given assumptions. The model is formulated for a constant total population H_{TOT} where the number of susceptible is much larger than the number of infected or immune. Hence, assuming that $\frac{H_S}{H_{TOT}}$ is constant also implies that $\frac{dH_S}{dt} \sim 0$. This was the rationale that led us to dismiss Eq. 3. The derivation proposed by the Reviewer is correct, but note that the conclusion is that $\frac{\eta_0 M_I}{H_{TOT}} \ll 1$, with units of time^{-1} (that is, a characteristic time for infection is relatively long). Testing this inequality does not seem trivial. However, we do know that the number of infected is relatively small, hence this infection timescale has to be large

as required by the equation and correctly pointed out by the Reviewer. By adding the above sentence, we hope we have clarified the reasons for the assumption made.

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