

Interactive comment on “Using multi-source satellite data for lake level modelling in ungauged basins: a case study for Lake Turkana, East Africa” by N. M. Velpuri et al.

Anonymous Referee #2

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The authors here present a technique to estimate fluctuations in lake levels based on remotely sensed data products offering estimates of rainfall and reference evaporation, and a known topographic grid for the catchment of interest.

The authors have applied the method to Lake Turkana and validated their data against satellite altimetry data for the Lake.

While the study itself is coherent and sound, I have some concerns about its publication in HESS. Specifically, I believe the authors need to better justify:

i) the transferability of the approach used to other basins

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ii) the simplistic nature of the approach used (in particular the 1D treatment of the basin with the lateral contribution of the hydrology only accounted for by invoking an empirically calculated lag time)

iii) the scientific contribution of this paper

To elaborate:

i) transferability of the approach

The authors argue that this paper describes an “approach that would supplement current satellite altimetry based systems to monitor variations in lake water levels and support operational monitoring in ungauged basins”.

However, the application of this approach has been to a single case study which must represent one of the most simple lake systems to compute a water balance for. Lake Turkana has:

- o no groundwater input,

- o minimal seepage fluxes,

- o no outlet through river flow (closed catchment)

- o no human abstraction,

- o minimal modification in its catchment (groundwater pumping or irrigation in the catchment for instance might greatly alter the water balance model proposed)

- o minimal modification in its rivers (other than the hydroelectric dam, which the authors acknowledge poses an issue that the current framework cannot handle).

These conditions are not generally met in lakes worldwide. Indeed, much of the justification for this study explicitly considers the importance of lakes as surface water resources - i.e. for human use – and yet the proposed technique could not be applied in lakes where human abstraction was a significant component of the water budget,

because the model formulated does not consider abstraction, and the remote sensing approach suggested does not offer insight into human water use. The proposed method, in fact, could only be justifiably applied in closed, near pristine basins. While such basins certainly provide important information about climatic variations (although it seems impossible that the water balance approach proposed here, which is driven by climate, could validly be used to interpret anything new about climate variations), they could not be said to represent the preponderance of global surface water bodies.

That said, the case can certainly be made that Lake Turkana is an excellent place to trial an initial modeling approach, for all these reasons that make its water budget simple. If the authors wish to convince their readers that they have developed a general method for inferring lake level data, however, the complexities of most “real world scenarios” cannot be ignored, and the tremendous simplifications offered by Lake Turkana as a case study should be explicitly highlighted. I would really have liked to see a discussion of how remote sensing could help with the challenge of estimating human water use, which could then be used to address the problem of estimating lake levels in areas where there are immediate concerns about water depletion. How could the model here, for instance, be adjusted to apply to Lake Chad?

ii) The approach presented here is essentially a 1D water balance with any “excess” water routed to the lake. It is hydrology simple in the extreme, and, in an era when many open-source distributed hydrologic models are available, the authors need to defend their decision to

a) exclude any explicit routing model (even the rational method!)

b) ignore potential losses due to groundwater recharge (these may be minimal but should surely appear in the soil water balance?)

The “calibrated lag” approach is clumsy, and ignores simple factors like the relationship between flow depth and flow velocity – which suggest that the lag is likely to be non-stationary between seasons. Why is the model forced on daily timescales but only

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evaluated on monthly timescales (or is that the timescale of the altimetry – I couldn't find reference to the altimeter return period)?

iii) In essence, the data presented in this paper boil down to the results a water balance model with a calibrated lag time and calibrated parameters, applied for several years. And yes, the model does appear to capture the general trends in the Lake Turkana level. However, given the relative simplicity of the system considered here, and the fact that measured estimates were available of all but one of the fluxes considered, it would be quite concerning if reasonable results on monthly timescales weren't obtained.

What have we really learned by going through this exercise? The hydrology is a simple water balance, the insights into the hydrological functioning of Lake Turkana are slender, and I dispute that much new can be learned about climate fluctuations from a model that is driven by climate fluctuations. Assuming the input remote sensing products have been independently validated, there is every reason to assume that the water balance approach would reflect climate variations. In other words, although I don't disagree with the analysis and conclusions drawn, I don't think the authors have made a strong case for new science in this paper – particularly given the concerns about the transferability of the approach proposed for ungauged basins in general.

I have a few other technical and editorial comments:

1) The large drop in correlation coefficient between the calibration and validation periods suggests the possibility that the correlated parameters are not stationary in time. The authors may wish to consider repeating the calibration for a later period in time and evaluating whether or not the calibrated parameters are changing. If such a change is taking place, it might mean that the parameters of interest are sensitive to climate conditions, or it might reflect that processes that are not well represented in the model are affecting the "effective" value of the parameters. Either way it might be worth checking.

2) Page 4853, Line 6-7: "several surface waters are rapidly being depleted" Suggest including Vorosmarty et al 2010 (Nature) as a relevant reference

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- 3) Page 4834, Line 11 – MODIS data - -monthly: raises the question of what are the necessary scales and what are the intended uses of this data? The material on page 4855 lines 1-5 could come earlier to motivate the need for this kind of data.
- 4) Page 4855, lines 10-15 – it might be worthwhile mentioning the general tradeoff between spatial and temporal resolution in RS data means that there is a relatively small subset of suitable datasets for the time and space scales of interest here.
- 5) Page 4857 line 2 – do you mean crop coefficient rather than cropq coefficient?
- 6) Page 4857 equation 2 – the interception losses are a little unclear - to some extent the evaporation of intercepted water might be expected to form a considerable component of the latent heat flux. This is not accounted for in the ET model – do you risk over-estimating the role of transpiration as a result, particularly after large storms? It would be good to quantify the magnitude of interception evaporation as a proportion of the ET budget from comparable locations to justify this point.
- 7) Page 4857 equation 3 – this comes back to the lack of a runoff routing mechanism, and seems very odd as a first choice. Even employing a method as crude as the Rational Method accounts for the travel time associated with routing runoff from the extremes of a catchment to the outlet. Why not at least consider something like a Rational Method approach to provide coarse runoff routing, especially when considering a sizable watershed where travel times are unlikely to be negligible?
- 8) Page 4857 equation 4 – would be helpful to express the surface area as a function of the depth so that the reader doesn't infer that you are describing a static surface area.
- 9) Equation 6 – please clarify the difference between groundwater outflows and seepage? I interpreted seepage as loss from the lake floor – i.e. drainage into groundwater – but here you state that these losses are minimal.
- 10) The sections on uncertainty and calibration are overly philosophical. I suggest you

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simplify them to remove the definitions of uncertainty and the significance of uncertainty in modeling, and the definitions of calibration — in the former case this is rather extraneous to the study and in the latter quite redundant to the HESS readership. Also please avoid using statements like: “we believe” ... in a scientific article it is not usually the authors’ beliefs that are in question! It would be more appropriate to simply highlight that there are significant uncertainties associated with using RS data and to proceed with the evaluation of those uncertainties.

11) Are there no estimates of evaporation from lakes in arid regions that could be used to assess ET_f? Israeli, US South West, Australian data for instance? I find it hard to believe that data are only available for Idaho and suggest the authors attempt to clarify this parameter estimate. This could help validate the calibrated value.

12) Finally the manuscript contains several typos and areas where the English language use is not standard. Please double-check all citations in the text (e.g. I think “Alsdrof” should be “Alsdorf”). I would suggest having the manuscript edited for English use.

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