Assessing the potential hydrological impact of the Gibe III dam on Lake Turkana water level using multi-source satellite data.

Velpuri and Senay

Overall this is an interesting and reasonably well written paper. Providing further insight on the water balance of the Omo basin, and the impact of the proposed Gibe III dam and its operation is very topical. I think the paper is worthy of publication. Despite this there are some aspects of the paper that to my mind require significant attention/revision.

General Comments

Although the different parts of the water balance are discussed at the start of the paper where the inputs to the proposed water balance model are described, the rest of the paper focuses primarily on the (remotely sensed) precipitation. In particular the importance of evaporation is not discussed, and is only briefly mentioned in the introduction. The authors do not discuss the evaporation (actual & potential) from the basin. First little attention is given to the reliability of the estimates of evaporation as obtained from GDAS. I think that it is important to consider the reliability of these data. In the predecessor paper, Velpuri et al., 2012 there is some discussion on the accuracy of these estimates, which is reported to be in the order of 15-30%. Additionally the accuracy of RFE rainfall estimates is reported in that paper to be in the order of 50%. I would agree that these orders of accuracy are quite realistic. However, the paper does not address the consequence of these large uncertainties, and how these impact the results found. I think the paper would benefit from some discussion on the use of this (uncertain) data. It is immediately clear that this is quite important when considering that the water balance of the basin is determined by I expect high values of (actual) evaporation and rainfall. Long term runoff is a function of the difference between evaporation and precipitation. This means that errors in the estimation of the two contributing terms can have quite significant impacts on runoff estimates.

Analysis of the actual evaporation may also help explain some of the results found, which are in part currently not well explained. For example the historical approach shows the impact of the initial filling of the Gibe III reservoir. However, the water levels in Lake Turkana then converge until the difference between the two scenarios is < 1m (and probably converge further for even longer simulation times). Given that this is an endorheic basin, and that there is no change in the consumptive use of water between the scenarios with and without the dam, the only term I can think of that results in this convergence is the difference in evaporation over Lake Turkana where there is less evaporation due to the lower levels (although there would be an increase in evaporation from the Gibe III reservoir itself – but this is a smaller surface area and will have lower evaporation due to its elevation). The other possible explanation may be in the level-area relationship as indicated in the paper, but it should be clear which of the two is dominant as the consequences on the volumetric balance of the basin are quite different. I think the authors should provide some explanation of the results found, given the proposed model, as well as reflect on the influence of uncertainty in the estimates of the terms on these results.

The English used in the paper can be improved. Some sentences contain small grammatical errors. In many cases, for example, the article is left out. There is also a tendency to use long sentences. The

detailed comments below suggest some corrections, but are by no means complete, and a thorough review by a native speaker is suggested.

The notation of the Lake Level Model is confusing. There appears to be a mix of flux and state variables. Li is defined as a reservoir levels (which is a state variable), but to keep the units consistent these are defined as [L/T] (ie a flux). This is confusing as that would suggest a change in level over time and not a level. If it is indeed the change in level, then it would seem the change in level at the previous time step should not be considered. Please try and improve the notation and make it more consistent with e.g. the notation normally used when describing Level-Pool routing which this LLM is similar to. Also the formulation of the balance over Give III suggests that the outflow of Gibe I is directly into the reservoir, and G3 Q_{inf} is the runoff from the entire upstream catchment, except for the part upstream of Gibe I. Also I assume that Q_{inf} is the inflow to the lake or reservoir [L^3/T] divided by the area of the lake (which depends on the level). Finally both lake evaporation and rainfall will depend on the level of the lake (which may vary some 22% as reported in section 4.6). It may help to describe this more explicitly.

The calibration approach described on page 2996 needs clarification. It is unclear to me what the calibration parameters are. Perhaps these can be added to equation (2) or (3) for clarity. If I understand it correctly, the parameter is applied directly to the calculated runoff for each month (i.e. to the result of the model, and not to a parameter in the VegET model, or to the rainfall or evaporation). If this is the case, then this does to my mind make some quite large assumptions on the stationarity of the flows for each month in the Omo river between the two periods.It is known that the levels in Lake Turkana have not been constant as described also in section 2.1 of the manuscript. This may cast some doubt on the stationarity of the flows. I think that this assumption therefore warrants some discussion. It is also not quite clear to me how the estimated baseflow is used. Does this replace the modelled value? Or is the base flow calculated used to calibrate the factors applied to the flow in the low flow months. Please try and clarify this section, relate what is calibrated in the VegET model described in the previous section, and indicate/discuss the major assumptions made.

In the same paragraph on calibration it is noted that the base flow for Gibe III is derived by analysing the available data – while for Gibe I the values of EEPC o are used. How do these two sources compare – it is unclear how the latter is derived.

Detailed comments

P2988 L10: Rephrase sentence starting “We use ….” to “We use a calibrated water balance model”

P2988 L14: The brackets do not quite make sense, try restructuring this sentence.

P2990 L2: a hydroelectric

P2990 L21: Recently a few studies … …have become available.

P2991 L6-8: I would suggest to avoid using “impact assessment study”, as this may be confused with the environmental and socio-economic impact assessment mentioned previously. The scope of this assessment is quite a bit more limited.

P2991 L24: and extends up to

P2992 L8: with an amplitude

P2993 L5: decadal

P2998 L5-L15: The grammar in the bulleted phrases is not correct – please improve this.

P2998 L17: Since Gibe III was not

P3001 In step 1 of the NBR method it is suggested that the same approach can be equally applied to rainfall, runoff and ET variables. While this is mathematically true, I would think that in the case of runoff in a basin of this size ignoring the serial dependency will lead to a physically unrealistic (sampled) time series. For ET this may also be the case, though here the dominant seasonality may reduce the issue. For rainfall serial correlations are low and I agree that this dependency is low. I would suggest some comments are made to clarify.

P3002-P3003 I am not sure the revised formulae of the LLM contribute much. Also the derivation of the upper and lower uncertainty bounds is somewhat superfluous. Simply stating that the bounds are derived from the empirical distribution would to my mind suffice.

P3003-P3004: In the hypothesis of why the lake would stabilise faster depending on the initial water levels because of the smaller inflows does not fully make sense to me. Volumetrically there is no dependence of the inflow on the lake levels. I agree that the level increment depends on the area of the lake, but does this mean it stabilises quicker? I think that the evaporation from the lake and its dependence on the lake area also needs to be considered, and suggest the authors revisit this paragraph after looking carefully at the influence of evaporation

P3005 L2-10: An initial error of 49% seems to be quite significant. Again the role of the evaporation data in this should be included in the discussion. Given this quite large bias – it may be interesting to look at the range of values of the multiplier for each month that has been calibrated, and discuss if these are realistic (e.g. is there consistency between wet/dry season corrections, or is there no serial correlation between months?).

P3006 L13: How can the increase to a difference of 3m be explained between the scenario with the dam and without? After all in both cases the dam has been filled before this difference established itself. Is this due to the moderated flows and/or the influence on evaporation over the lake? It would be interesting to know due to changes in which fluxes these differences are caused.

P3006 L27: After the first impoundment

P3007 L12: in dry years

P3008 L1: (UCI) → (LCI)

P3008 L11: Again it would be interesting to explore what is the cause of the dependency on initial lake levels, and understand the role of evaporation from the lake. Please investigate and expand the discussion.

P3009 L23: Sentence starting “The data …”does not make sense – please rephrase

P3008 L13-L28: In the analysis of the lake shoreline changes, I understand that the Gibe III dam is not considered and the influence of the three scenarios of rainfall is investigated. Given the results of the three methods on the impacts of the dam, it may be interesting to discuss or show these shoreline changes if the dam is in place. I assume that these would be smaller, particularly for the below normal rainfall scenario (as discussed in the results of the knowledge based method). It may be interesting to comment on this.