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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assessing the potential hydrological impact of the Gibe III Dam on Lake Turkana water level using multi-source satellite data" *by* N. M. Velpuri and G. B. Senay

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Author's Response to Referee Comment #2 by Dr. Micha Werner

We would like to thank Dr. Werner for his constructive and insightful comments. The following responses aims to address the comments provided.

General Comment #1: 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. Authors Response: To address this comment, we will introduce a new section on uncertainty which will discuss about the accuracy of different datasets used in this study including the accuracy of GDAS evapotranspiration data and RFE rainfall data. This section will also address the consequence of the uncertainty in the data used.

General Comment #2: 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.

Author's response: We agree with the reviewer and we will correct for all grammatical errors and also we will have the revised manuscript reviewed by a native English speaker as suggested. We thank reviewer for pointing out several typos and English errors in the published manuscript.

General Comment #3: 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 Qinf is the runoff from the entire upstream catchment, except for the part upstream of Gibe I. Also I assume that Qinf is the inflow to the lake or reservoir [L3/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.

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Author's response: We understand the concern raised by the reviewer. We will change the units of lake level at [L] in the revised manuscript instead of [L/T]. We will also improve sections 4.3 in the revised manuscript as suggested.

General Comment #4: 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.

Author's response: We agree with reviewer's concern that section 3.2.2 on calibration needs to be improved. We will explain the calibration processes clearly in the revised manuscript.

Yes, as the reviewer pointed out we performed calibration on VegET runoff only. Over the lake rainfall contribution is negligible when compared to the lake inflows and hence RFE rainfall data is not calibrated directly. Other errors in the datasets (including errors in ET) are accounted for by the ε term in equations 7 and 12. The estimate of ε term (2mm/day) for Lake Turkana obtained by Velpuri et al. (2012) is used in this study.

We assume that inflows into the Lake Turkana are stationary which means that the long-term mean inflows into the lake have not changed. This assumption holds good as trends in annual and seasonal rainfall fall has not undergone any significant change in Ethiopia (Seleshi and Zanke, 2004; Cheung, et al., 2008). We assume that the volume of lake inflows in any given year would follow the long-term distribution observed in the past (1961-2001). We calibrated mean monthly observed (1998-2009) lake inflow data with the mean monthly observed flows (1961-2001). The multiplicative factors derived for each month are applied to individual years of modelled lake inflows (1998-2009) to obtain calibrated lake inflows.

General Comment # 5: In the same paragraph on calibration it is noted that the base flow for Gibe III is derived by analyzing the available data – while for Gibe I the values of EEPCo are used. How do these two sources compare - it is unclear how the latter is derived.

Author's response: We estimated base flow from the long-term mean monthly hydrographs using a constant discharge method (Linsley et al., 1975) for Gibe III and Turkana. After the base flow was estimated for each month, combined estimate of base flow and modelled runoff were used to estimate the calibration parameters (multiplier values) for each month. Base flow estimates for Gibe I (9 m3/s) were obtained from EEPCo publication (EEPCo, 2004). We are not sure how base flow for Gibe I were estimated. However, we used percent area approach to estimate Gibe I base flow using Gibe III base flow (60 m3/s) and found that published estimates are reasonable. The area of Gibe I is nearly 4500 km2, which is around one seventh of Gibe III basin area (33000 km2). Thus based on the percent area approach base flow estimate for Gibe I is (4500/33000)*60 = 8.2 m3/s.

Other comments:

1. P2988 L10: Rephrase sentence starting "We use . . .." to "We use a calibrated water balance model"

Response: Sentence will be rephrased in the revised manuscript as suggested.

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

Response: Sentence will be restructured in the revised manuscript as: "The model evaluates the impact of Gibe III dam using three different approaches (historical approach, regional climate-based approach, and nonparametric bootstrap resampling approach) to generate rainfall-runoff scenarios."

3. P2990 L2: a hydroelectric

Response: Sentence will be corrected in the revised manuscript as suggested.

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

Response: Sentence will be corrected in the revised manuscript as suggested.

5. 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.

Response: We agree with the comment made by the reviewer. We would change "impact assessment study" as "hydrologic impact assessment study" to be more specific in the revised manuscript.

6. P2991 L24: and extends up to

Response: Sentence will be corrected in the revised manuscript as suggested.

7. P2992 L8: with an amplitude

Response: Corrections will be made in the revised manuscript as suggested.

8. P2993 L5: decadal

Response: By the word Dekadal NDVI, we refer to 10-day NDVI dataset. We do not mean decadal or 10 year NDVI. Hence we left the sentence as is.

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

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this.

Response: Sentences will be corrected to improve grammar.

10. P2998 L17: Since Gibe III was not

Response: Corrections will be made in the revised manuscript as suggested.

11. 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.

Response: As suggested by the reviewer, the use of simple NBR approach can be used for rainfall as serial dependency is very low and rate of variability in day to day evapotranspiration is insignificant and thus does not show serial dependence.

On the other hand, we agree with the reviewers comment that the use of simple NBR approach to generate stochastic lake inflow data would not preserve serial dependency. This is the current limitation of the approach used in this study. We also tried nonparametric block bootstrap resampling (Hardle and Mammen, 2003; Politis, 2003) and nonparametric weighted bootstrap resampling or nearest neighbor resampling (Lall and Sharma, 1996) approaches (not presented in the HESSD manuscript) which would preserve the serial dependency. We found out that results from the three approaches were not very different since we ran the simulation very large number of times (100 000) using a Monte Carlo approach, the impact of the different combinations of daily runoff including the scenario with serial dependency is captured by the mean and upper and lower 95% confidence intervals. To minimize the length of the text in the manuscript, we did not present results from the other two methods, i.e., reported the simpler approach.

12. 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.

Response: We deleted equations 13, 14 and 15 in the revised manuscript. However we retained equations 10 through 12 for the sake of clarity and completeness.

13. 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.

Response: The impact of initial lake levels is mainly because of the lake elevation-area-volume curve. This shows the combined effect of the regulated/moderated lake inflows into the lake and evaporation losses from the reservoir. To explain this phenomenon clearly, let us assume two cases as shown below:

Case 1: Lake is at higher initial lake level

Without Gibe III: The lake would receive unregulated inflows (with an average flow of 650 m3/s) that would compensate most of the ET losses and hence lake levels would show little reduction in lake levels.

With Gibe III: The lake would receive regulated minimal inflows (with an average flow of 400 m3/s) especially during the first impoundment period and since at higher lake level, larger volume of inflows are required to compensate the ET losses, the lake levels will decline rapidly when compared to without Gibe III. Hence the impact (different between without and with Gibe III) is higher at higher initial lake level

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Case 2: Lake is at lower lake level

Without Gibe III: The lake would receive unregulated inflows (with an average flow of 650 m3/s) that would compensate ET losses from a smaller surface area and hence lake levels would show little reduction in lake levels.

With Gibe III: The lake would receive regulated minimal inflows (with an average flow of 400 m3/s) and since at low lake level, smaller volume of inflows are enough to compensate most of the ET losses, the lake levels will decline at a smaller rate when compared to without dam. Hence the impact (different between without and with dam) is lower at higher initial lake level

Furthermore, we believe that the result from our analysis (Figure 7) could be used to better understanding the hydrologic impact of dam commencement in relation to initial lake levels.

14. 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?).

Response: The issue of error/bias in RFE and the role of ET data will be addressed by the inclusion of a new section on uncertainty in the revised manuscript. The range of values of the multiplier used for calibration of runoff are analyzed and presented in figure 1.

We believe that the multiplier values are reasonable and realistic. These values are within the bounds of errors observed in the satellite based rainfall estimates (RFE data). It has to be noted that RFE underestimates up to 50% in general and the multiplier values are thus with the maximum value of 2.0 for Lake Turkana inflows. Also, please

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note that multiplier values for Gibe III are little higher. This is because Gibe-III basin falls in the mountainous region where orographic rainfall is common. As RFE also underestimate orographic rain, the multiplier values are little higher over the peak rainy month. The multiplier values demonstrate seasonality and serial dependence on a monthly time-scale. We will discuss and modify section 3.2.2 in the revised manuscript as suggested by the reviewer.

15. 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.

Response: Our model results under Approach I (Historical approach) indicated that during the difference between 'with Gibe III' and 'without Gibe' increased up to 3 m by the end of second year (1999) after the commencement of the dam in 1998. This is because of the combined result of (a) highly reduced lake inflows into the lake during initial impoundment (b) moderated inflows after Gibe III is full and, (c) evaporation losses from the lake. Under normal conditions, lake evaporation losses alone can account up to 2 m of loss in lake level under zero inflows. However, due to combined effect of moderated lake inflows through the Gibe III dam and evaporation losses from the lake, model results indicated that lake lost up to 3 m by the end of second year (year 1999).

16. P3006 L27: After the first impoundment

Response: Corrections will be made in the revised manuscript as suggested.

17. P3007 L12: in dry years

Response: Correction will be made in the revised manuscript as suggested.

18. P3008 L1: (UCI) → (LCI)

Response: Corrections will be made in the revised manuscript as suggested.

19. 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.

Response: We believe that the impact of initial lake levels shown in Figure 7 is due to lake elevation-area-volume curve. For more explanation, please refer to our response to the comment # 13 above.

20. P3009 L23: Sentence starting "The data ..."does not make sense – please rephrase

Response: Corrections will be made in the revised manuscript as suggested.

21. 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.

Response: The impact of the Gibe III has been already considered in the lake shoreline change analysis. Our analysis includes three different model realizations under three different rainfall situations (approach such as near-normal, above normal and below normal rainfall) produced using NBR approach under with Gibe III condition. The model is run 'with Gibe III' so that lake shoreline changes under the impact of the dam can be simulated and analyzed. We will make necessary changes in the revised manuscript to explicitly make this message clear.

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Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C1863/2012/hessd-9-C1863-2012-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 2987, 2012.

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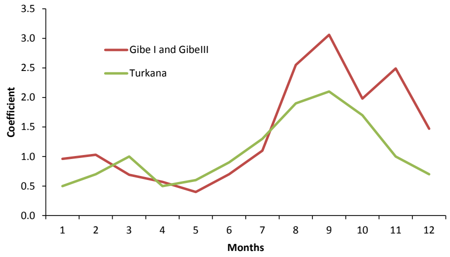

Figure. Monthly calibration coefficients derived for Gibe I, Gibe III and Turkana basin runoff.

Fig. 1. Figure. Monthly calibration coefficients derived for Gibe I, Gibe III and Turkana basin runoff.

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