Response to the Reviewer
Soil-2015-84
Calculating the Sediment Budget of a Tropical Lake in the Blue Nile Basin: Lake Tana
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We thank the reviewer for his extensive specific comments. We appreciate the reviewer's suggestion that the manuscript addresses an important topic. We agree that revisions are in order. Many of the points noted in the specific comments will improve the manuscript greatly.

However, we find that the reviewer's evaluation that the paper should be rejected is unduly harsh and unfair. Substantive flaws were not found given that purpose was to obtain a sediment balance for Lake Tana with the limited data available in the Lake Tana basin in Ethiopia.

Below we respond to the main reasons for rejection. We clearly show that the comments for rejection of a manuscript are unjustified. In our reaction to the comments, we first repeat the comment of the reviewer and then give our response just below the comment

Reviewer's Comment 1
The objectives in the introduction need to be put clearer. A hypothesis was not formulated.

Response
A hypothesis is not a requirement for a manuscript as is clearly indicated by a manual on writing and two randomly selected papers cited in "Soil"

The extensive scientific writing manual on the Wiley website by Cargill and O'Connor (2009 p. 50) states the following concerning the end of the introduction (and does not state that a hypothesis is required)

> "At the end of the introduction authors set up the readers' expectations of the rest of the paper they tell them what they can expect to learn about the research being researched"

In addition we looked up the first two papers that were featured on Feb 16 on the "Soil" webpage. Below we cite the text that introduces the rest of their papers. In the manuscript of Groeningen et al. (2015) we find in their manuscript on the nitrogen cycle the following:

> "Here, we review important insights with respect to the soil N cycle that have been made over the last decade and present our view on the key challenges of future soil research (Fig. 1). The approach adopted in this paper is three-fold:"

Ping et al. (2015) in a paper on permafrost states:

> "In this review, we highlight and discuss important factors affecting the patterns, processes, and carbon stocks of permafrost soils, and summarize recent research developments.
> ,

The authors of these two papers (chosen at random) and the scientific writing manual are in agreement that a hypothesis the exception rather than the rule.

Our objective as introduction to rest of the paper is clearly understandable

> "The objective of this study is to combine current knowledge on sediment transport and to quantify the sediment budget for the Lake Tana and its watershed"

However, reading over the introduction, a valid criticism is that we did not introduce the article sufficiently.

Reviewer's Comment 2

It seems that measured precipitation, discharge and sediment data were not analyzed for consistency sufficiently. Measured data that are proven to be not correct need to be excluded or corrected before further analysis and cannot be used for model calibration and validation. The authors should redo the analyzation of the used data. After that the results might be different from the results presented here.

Response

The precipitation is highly variable, gages are far apart and even over a 200 m distance rain varies greatly (Bayabil et al. 2016). The discharge changes due to development in the watershed and it is difficult to decide a priori what the cause of any of the slight deviations.

It is very difficult under these circumstances in developing countries to use techniques developed for more developed countries with a temperate climate and less rugged terrain to decide with any certainty the input data that should not be considered (as suggested by the reviewer). With limited data availability, how does the reviewer propose to check the data other than for obvious outliers? In our opinion, suggesting a different approach is appreciated.

It is common practice that this suggestion is accompanied with a specific procedure that has been used in the refereed literature or that the reviewer is familiar with. Without specifics, this comment is superfluous and we cannot comment on it. It certainly should not be used to reject a manuscript.

Finally we would like to point out that most of the hydrological data used by us for obtaining the sediment balance has been used in the refereed literature and apparently of sufficient quality to warrant publication. These publications are: Chebud and and Melesse (2009); Easton et al. (2010; 2012); Gebrehiwot et al (2014); Haile et al (2009); McCartney et al (2010); Rientjes et al (2011); Setegn et al. (2009, 2010, 2011) and Wale et al. (2009)

Reviewer's Comment 3

The manuscript needs a major restructuring. Methods and results need to be strictly differentiated.

Response

The above comment is the reviewer's perspective. However, there are various ways that the methods and results can be written. As an example, we cite below the style manual by Cargill and O'Connor (2009) concerning the "methods" section'

> "Traditionally, students are taught that the Methods section provides the information needed for another competent scientist to repeat the work. In your experience of reading papers, is this what you find? Many participants in workshops we have conducted report that they have had problems in replicating what authors have done in their published studies even after reading the Methods section thoroughly.
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> Another way to think about the goal of the Methods section is that it establishes credibility for the results and should therefore provide enough information about how the work was done for readers to evaluate the results; i.e. to decide for themselves whether the results actually mean what the author claims they mean. Referees are likely to look in this section for evidence to answer the question: Do the methods and the treatment of results conform to acceptable scientific standards?"

This is not an experimental paper where this requirement of differentiation is very important. We give sufficient detail in the material and methods section (and establish credibility for the result section) for the reader to follow the remaining part of the paper. Revisions yes, but rejection is unwarranted in our opinion!

Reviewer's Comment 4

The description of some methods and used data is missing (see Specific comments for details) so that some argumentation cannot be understood.

Response

We will add these details to the description the methods and used data. Is this really sufficient for a rejection of a paper when **some** methods and used data are missing?

Reviewer's Comment 5

The results should be discussed in more detail and should not be related to errors/anomalies in the measured data.

Response

This, again, is the perspective of the reviewer. We do not understand why we cannot point out inconsistencies in the data in the results section? This information is helpful to the readers that will use the same data.

Moreover, our findings "in errors/anomalies" in the measured data are significant and worthy to be shown. For example, we are the first among all the watershed models that were run in the Lake Tana basin (see for citations in response to Comment 2) to show that the gage of the Rib overflowed about a certain level and the discharge data were not accurate above that level. This is an important result that should not be hidden from future research.

In addition, the discussion of closing the water balance is extremely important in order to assess if there are other ways that the water can leave the watershed. It might be of interest that after submitting this manuscript that in one of the recently monitored watershed in the Rib catchment, only 10 % of the rainfall reached the outlet. Thus, in this case that the water balance is not closed (as often is given as a reason to reject data) would have been sufficient reason to throw out a priori perfectly good data.

Finally, what is the difference if we assess the quality of the data in the result section of the manuscript or beforehand (as suggested by the reviewer)? A simulation model is basically a mathematical construct that relates the input (rainfall) to the output (stream flow) and is a good way to assess the quality of the data. Any technique applied a priori essentially follows the same procedure and at the same time assures that the model will fit the data well, so it can be published easily. However, the disadvantage is that potentially information can be lost on functioning of the watershed not foreseen by the model.

Reviewer's Comment 6

Figures and tables in the supplementary material are either superfluous or could be integrated directly in the manuscript (see Specific comments for details).

Response

The purpose of the supplementary material is to give information to interested readers. We are aware that some might find it "superfluous", but others might find it interesting. We will consider the suggestion of the reviewer, but is this really the right argument to reject the paper for publication?

In summary

This paper is about obtaining the best information from a very limited data set in order to provide an answer to an important problem of the sedimentation of Lake Tana. Standard procedures in analyzing the data as proposed by the reviewer are difficult to use in developing countries where data are limited and are collected under political unstable conditions. The other reason given about the form of the manuscript is not shared by all.

This is a research paper where we can redefine procedures to fit better the situation. Using the argument that we do not apply the standard procedures can simply not be a reason to reject a research paper.

There are a number of excellent specific comments given by the reviewer. We will make the improvements, but will not answer in detail below since it is unlikely that the manuscript will be accepted in "SOIL" after we received the 10-line review of referee 2 stating that: "The topic is attractive but does not fit to the context well. To me, the manuscript suffers from many deficiencies which discourage me to accept it.". The deficiencies were not specified.

Finally we are fully aware that as authors, we are in a weak position to make the arguments given in this response to the reviewer's comments and likely will have to withdraw the paper and submit it elsewhere.

Regards

Tammo, Fasikaw, Essayas, Seifu, Dessalegn and Mamaru

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