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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Application of isotopes and water balance on Lake Duluti–groundwater interaction, Arusha, Tanzania" *by* N. P. Mduma et al.

Anonymous Referee #2

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General comments

The manuscript "Application of isotopes and water balance on Lake Duluti–groundwater interaction, Arusha, Tanzania" by Mduma et al. uses major ion and stable isotope data to assess lake water – groundwater interactions in a volcanic area of northern Tanzania. This study presents an important dataset from a data scarce region, and as such it would be of value for the local stakeholders. However, in its current form the manuscript is a mere case study with little global significance, and more importantly it suffers from several major flaws that, in my opinion, prevent it from being published in HESS. In particular, the introduction fails in properly addressing the research background; the water balance calculations are based on incorrect as-

sumptions; the results are poorly presented, confusing and they lack a link between the sections on major ion chemistry and the sections on water balance calculations. There are also many grammatical and typesetting errors all throughout the manuscript, and figures are of poor quality. I would encourage the authors to thoroughly rework each section of their manuscript and to be much more meticulous in their assumptions, presentation of results, analyses and interpretations. Specific details are given below.

Specific comments

Introduction

* The Introduction does not establish the context of the investigation properly. The research objectives need to be clarified, and the authors should strive to go beyond the simple case study by addressing broader questions that will be relevant to the whole hydrology research community. Despite being a well-covered topic, the authors do not provide a single reference on the interactions between lake water and groundwater. Just a few suggestions: Krabbenhoft et al. (1990), Gibson and Edwards (1996), Kebede et al. (2006) (and several other papers in Ethiopia), Bouchez et al. (2016). A more stringent literature review is needed here, and only from the knowledge gaps thus identified should the authors derive their research questions.

* The authors state that their objective is to "quantify the groundwater exchange with Lake Duluti" (L.58-59), but this objective does not seem to cover all the Results section. The water types and hydrochemical processes occurring in each hydrological component are also described, which needs to be reflected in the research questions presented in the Introduction.

Methods

* The structure of the Methods section could do with some adjustments. I would recommend adding two subsections '2.1 Sampling and laboratory analyses' (L.81) and '2.2 Water balance approach' (L.111). As it is currently written, subsection 2.1 goes

back and forth between ionic and isotopic analyses, which tends to confuse the reader. This needs to be improved.

* The reported analytical errors for deuterium and oxygen-18 are extremely high compared to most studies. Any comment on why this may be? Error bars should be added to the graphs in Figure 7 (the observed spatial variations may not be significant).

* I see several major flaws to the isotopic mass balance and mixing methods developed by the authors. First, equation (7) does not introduce weighting factors for each component, so the equation is incorrect. Second, the mass balance presented in equations (9) and (10) has two unknown terms, i.e. groundwater inflow Gi and groundwater outflow Go (or it is unclear to me whether one of the two terms is known, and how it is estimated); therefore the mass balance cannot be resolved using only one equation. Third, the mixing approach assumes that the two end-members are (1) lake water and (2) groundwater. Surprisingly, the mixed component to be determined also seems to be lake water. How can a single term be both the mixed component and an end-member in the calculation?

Results and Discussion

* I would suggest revising the structure of the Results and Discussion section. In particular, it would be more convenient for the reader to first be presented with the hydrochemical facies of each component. Please consider moving subsection 3.1.2 at the beginning of section 3 (also note that for some reason subsection 3.1.1 is missing in the current manuscript).

* The Piper diagrams shown in Figures 4 and 5 may be dispensable, unless they are used to convey further information such as the delineation of hydrochemical processes, mixing between different components, etc. The observed changes from dry to wet are not discussed in the text. All other figures are not sufficiently described, some being not even referred to in the text (e.g. Figure 2).

* Generally, it is not clear how the seasonality factor is addressed. Can the authors elaborate on the effect of seasonality on major ion chemistry? And on the water balance? Also, the dry sampling round was carried out in January–February (L.83), however the authors mentioned that rainfall in the region occurs from October–November to May. Please elaborate on the weather conditions for that specific year.

* The way the end-member mixing analysis has been undertaken and the way the results are reported are both extremely confusing to me. It is unclear how the end-members were defined, and a proper characterisation of each end-member in terms of water types and isotopic ratios is clearly missing. This should be a prerequisite to carrying out any mixing analysis. Importantly, how do the seasonal changes affect your mixing analysis? How was this handled? Furthermore, the indication that “the fraction of groundwater is 0.73” (L.293) comes out of the blue and comes without any form of justification. How was this calculated?

* As per a previous comment, it is unclear to me what the unknowns in the mass balance equation are. I suspect that the authors had to constrain either Gi or Go? If so, how was this done? How did you obtain the values 2,430,960 m3/y (L.279) and 2,902,620 m3/y (L.280)? Where does the “80%” value (L.295) come from? Also, on which basis can you state that 73% and 80% are significantly different?

* The authors could have used the isotopically-enriched, highly evaporated lake water signature as a tracer of lake water inflow into the underlying aquifer. Do the major ion and isotopic values match your hypothesis of water loss from the lake to the aquifer? Are there different signatures upgradient vs downgradient the aquifer? Once again too many pieces of information are missing.

Technical corrections

I will not go through technical corrections because they are too many at this stage, and the paper first requires proofreading by the authors themselves. However, I would agree to assess a revised version of the manuscript if necessary.

Cited references

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