

Interactive comment on “Application of pore water stable isotope method to characterise a wetland system” by Katarina David et al.

Anonymous Referee #3

Received and published: 21 June 2018

This review follows the assessment of Anonymous Reviewers 1 and 2 – this reviewer concurs with their suggestions and adds only the following comments.

I must reject this paper due to serious concerns about the accuracy of porewater isotope analytical methods. If there is no clear confidence in the analytical isotopic results the subsequent modeling does not matter.

This paper, if revised and resubmitted, further requires hard editing and a lot of trimming. Please conduct a thorough review for basic grammar and sentence structure. Check for imprecise or vague terminology usage. Please consider reducing the length – in many places there is unnecessary “filler text” (i.e. “International publications/example . . . why not just say “research has shown. . . (refs). Remove a lot of the ancillary information (detailed lithology) that is not explicitly needed for your objectives

C1

of using pore water isotopes.

The entire Methods section, upon which this work hinges entirely, is insufficiently described or referenced. For example page 7 lines 13-17 – no citations are give for this sampling methodology. Section 3.2 needs to be entirely re-written – the analytical descriptions are incoherent. You need to give the delta values of all calibration standards. There is insufficient detail given to give confidence in the results.

I have grave doubts about the results for porewater stable isotopes and suspect the trends (or differences) between samplings may be due to evaporation artifacts. What was neglected to mention is the time lapsed between core sample collection (coring, stored in Ziplok) and the sample preparation (ie inflation) for isotope analysis. Was this storage hours, weeks, days? Ziplok bags are only good for a couple of days before evaporative loss occurs. If variable periods of times elapsed for the samplings, the samples could have been subjected to differential evaporative loss (ie why is the groundwater isotopic composition constant). There were no gravimetrics controls used, nor isotopic field controls to give confidence in this method (at least as it is described).

The Los Gatos “standards” used are not certified RMs, and should never be used for calibration. They have been revised at least 5 times due to improper storage (at LGR) in the past years.

SMOW / VSMOW do not exist – VSMOW2 does. Which was it?

The introduction is too long – suggest deleting lines Page 1, lines 12-18 (unrelated to wetlands)

Continuous line numbering would have been useful for reviewers.

Many places have a “the” or “a” added or missing. (i.e. title, Page 3 line 1, etc.).

Page 1 line 4, not climate change (aka CO₂)-> rather, paleoclimate.

Page 4, line 8 “Following such extreme settings. . . (what does that mean?)

C2

Page 4, line 16-20 – please rewrite the objectives in a clearer manner. A hypothesis would be a good place to start.

Page 8 line 25-26 - IRMS cannot be an LGR analyser!

Figure 6 caption error and use of $d^{18}O/d^2H$ – the slash suggests 'or' when you mean 'and'. Superscripts missing. Suggest using the same Y-axis scaling on all figures. Why are the symbols for the same thing different in each panel? Very confusing to look at and compare!

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2018-237>, 2018.