

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
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Review on hess-2021-558

Anonymous Referee #1

Referee comment on "Soil water sources in permafrost active layer of Three-River Headwater Region, China" by Li Zongxing et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-558-RC1>, 2021

General comments

In this manuscript, the authors analyzed the spatial variability of the isotopic composition of soil water at different depths in the Three-River Headwater Region (China). The isotopic composition of soil water was compared to the isotopic signatures of precipitation and ground ice, and it was related to elevation, soil moisture and soil temperature. In addition, the authors quantified the contribution of precipitation and ground ice to soil water.

The topic of this manuscript is potentially interesting for the readers of Hydrology and Earth System Sciences. I think the authors presented a valuable and interesting dataset that it is difficult to collect at high-elevation catchments. Nonetheless, several important methodological details were not presented in the manuscript, and I have major concerns about the approach adopted for the data analyses.

Firstly, it seems that the authors have not considered how the likely heterogeneous soil properties, the local topography, the climate and other characteristics could have affected the main results of the study. Indeed, the authors carried out their study in a very wide study area, but besides grouping the data based on soil depth, main land use or aspect, they have not analyzed the dataset based on other characteristics (e.g., data could have been grouped by subcatchment, homogeneous soil properties, presence of permafrost etc.).

Secondly, the authors have not described the soil type and in general, the main soil properties in the study area, as well as the spatial distribution of permafrost and the ground ice (details about how the presence of permafrost was determined and thickness of these layers should be added). Furthermore, given the large study area, I recommend a

better characterization of the climate and estimates of potential evapotranspiration, as well as an analysis of the temporal variability of the meteorological variables and the isotopic composition of the water sources.

Thirdly, in the quantification of the contribution of precipitation and ground ice to soil water, the authors should estimate the uncertainty due to the measurement errors and the high spatial and temporal variability in the isotopic composition of the various water sources. In addition, I think these contributions should be related to the specific subcatchment and sampling period (particularly, if the samples were collected during different sampling times, from June 2019 to July 2020).

Finally, I think some parts of the results and the discussions should be revised to avoid a mixture of the two sections (please see my technical corrections).

Specific comments

- In section 2.1, the authors should present (by also using a table) quantitatively the land use in the study region, as well as information about the main soil types, their average depths, and other available details on soil properties. Furthermore, details about the spatial distribution of discontinuous permafrost and how the presence of permafrost was assessed should be added in this section.
- Lines 148-152: I suggest providing mean annual precipitation and temperature data here, and adding a table with the monthly characteristics of the meteorological variables during the sampling period.
- In sections 3.1 and 3.2, the isotopic composition of soil water is not presented in relation to the isotopic signature of precipitation determined in the same sampling period.
- Lines 581-592: Figure 8 alone cannot support this discussion because the dual isotope plot depicts the isotopic composition of various water sources, from very distinct sampling sites distributed in a very large study area. My suggestion is providing an analysis based on small subcatchments where there are homogeneous soil characteristics.
- Lines 629-631: This sentence needs to be supported by the results of a statistical test.
- Lines 632-635: Figure 9 is not meaningful because it presents a regression line fitted to only three data points.
- Lines 637-647: Uncertainty should be added when presenting the contribution of precipitation and ground ice to soil water.
- Lines 692-693: Based on Figure 11, I disagree with this sentence because all regressions seem to have very low coefficients of determination. Are the regressions significant?
- Lines 698-703: The regressions should be tested to assess whether they are statistically significant or not.
- Lines 698-736: Most of the sentences report results, which should be moved to the

previous sections.

- Lines 766-770: The relations shown in Figure 12 are very weak, and thus they cannot support the statement that the isotopic composition is related to the soil moisture.
- Lines 805-812: The soil temperatures reported in Figure 12 and Table 3 are very high for soils where there is a permafrost layer. Are there soil temperature data measured very close to the permafrost layer? I suggest providing clear details about the spatial distribution of permafrost (and its depth) for the sampling sites where soil water was collected.
- Tables 1, 2 and 3: Sample size per each group of data should be provided in the tables. Furthermore, information about the sampling times should be added in the captions of the tables.
- Figure 1: I suggest adding the spatial distribution of permafrost in the study area, and showing the land use. The size of the labels is too small.
- Figures 5 and 6: I suggest using the same colour scale for all depths, and showing where bare rocks and glaciers are located (it does not make sense having interpolated isotopic values where there is no soil). Furthermore, details about how the interpolated maps were obtained are not present in the Data and methods section.
- Figure 9: Error bars representing the spatio-temporal variability should be added but, given that the figure is not meaningful, I suggest removing the figure.
- Figure 11: The figure is unreadable because the labels are too small, there are too many regression lines (probably the regressions are not significant), and the legend is missing. I suggest moving the equations in a new table.

Technical corrections

- Lines 167-171: Please add a reference to support this sentence.
- Line 183: It is unclear whether it is snowmelt water, glacier melt water or snowmelt from a glacierized area. I suggest rewording.
- Line 236: Based on the description this seems to be glacier melt water, but it cannot be assessed whether it consists more of snowmelt or ice melt water.
- Lines 315-327: This explanation belongs to the discussion. I suggest moving the sentences to the proper section.
- Lines 338-345: This explanation also belongs to the discussion. I suggest moving the sentences to the proper section.
- Lines 348-354: This explanation also belongs to the discussion. I suggest moving the sentences to the proper section.
- Line 526: I suggest replacing 'there were high variations' with 'there was a high variability'.
- Lines 560-568: This explanation belongs to the discussion. I suggest moving the sentences to the proper section.
- Line 579: It should be 'indicating'.
- Lines 582-594: This explanation belongs to the discussion. I suggest moving the sentences to the proper section.
- Lines 832-838: These results should be moved to the previous sections.
- Lines 647:655: This explanation belongs to the discussion. I suggest moving the sentences to the proper section.
- Lines 685-687: This explanation belongs to the discussion. I suggest moving the

sentences to the proper section.

- Line 692: The citation should be (Sprenger et al., 2017).