Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-283-RC2, 2018

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Isotopic reconnaissance of urban water supply system dynamics" *by* Yusuf Jameel et al.

Anonymous Referee #2

Received and published: 9 August 2018

General comment This manuscript presents the application of an isotopic (2H and 18O) approach used to quantify the contribution of different water sources in a complex public water supply system in Utah, identifying areas supplied chiefly by one source and those supplied by multiple sources), as well reconstructing basic flow patterns. This research is highly relevant for practical purposes and has operational implications in urban and rural environments, and will surely be of interest to the readers of HESS.

The manuscript is very well written, logically organized and nicely illustrated. The dataset is well explored and analysed in a robust way, and the results and their interpretations are solidly supported by data. I have only some minor comments and clarifications requests for the Authors before recommending this manuscript for publication.

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Specific comments The manuscript contains an excessive number of abbreviations (e.g., PWSS, SIW, SLV, BIMM, JVD, WTP, MGD, SEPT, SWTP etc) that sometimes make the reading difficult. Please, use acronyms sparingly, only when strictly necessary.

Introduction: although the results are clear, the Introduction misses a clear definition of the working hypothesis upon which to establish specific objectives. The objective should stem from the analysis of the identification of research gaps in the current literature and/or from a practical management issue in the study PWSS. Pease, re-structure the introduction taking this into consideration.

L8. Figure 2 is introduced quite abruptly in the text, before the M&M section, even though it seems to me to present some results of this study. I suggest redefining its position in the structure of the manuscript.

In the M&M section, please specify in a clearer way the concept of prior and posterior fractional contributions (see also captions of Fig. 3, 4 and 5).

L208. These assumptions should be discussed.

L290. The Hotteling multivariate t-test assumes a multivariate normal distribution: is the same distribution assumed at L209?

L296-301. I suggest reporting d-excess values as indicators of evaporative effect, to corroborate these statements.

L314. Please, be more specific: in what sense this work goes beyond previous work? This is part of the discussion on the originality and novelty of this research.

L419. I suggest moving this paragraph to section 3.5.

Minor comments and technical corrections

L45. Please, specify what these particular threats are.

L91. The reference to Gorski et al., 2015 applies to vapour. Is still relevant?

L122. Please, specify that the study was carried out in the USA for the benefit of the international readers.

L129. Is this especially true for irrigation purposes?

L135. Why? To avoid overexploitation? Please, specify.

L138. Please use SI units.

L146. Why do these well have so high salt concentrations?

L163: Combine with L174.

L184. Specify the sample size.

L199. The acronym has been already defined at L115

L201. Information, such as?

L202-204. Please, clarify.

L345. Typo: to our model.

L355. Typo: lines.

L380. Can you explain why you used credible intervals and not confidence intervals?

L396. Replace “credible intervals” with “CI”.

L510. Give references for these two models.

L513. I doubt cities in developing countries have to fund to perform costly isotope analysis over a long time span or large areas. Perhaps add a comment here.

Fig. 1. In the caption define WTP and SWTP.

Fig. 7, L780. Replace panel a with (a).

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