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Comment on hess-2022-258

Anonymous Referee #4

Referee comment on "Estimation of groundwater age distributions from hydrochemistry: comparison of two metamodelling algorithms in the Heretaunga Plains aquifer system, New Zealand" by Conny Tschritter et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-258-RC4, 2023

This manuscript aims at assessing the validity of using two machine leaning techniques to extrapolate beyond available groundwater age data and infer the lumped RTD from hydrochemistry.

This contribution is novel and appears quite appealing to complement tracers dataset which are costly and time consuming.

The manuscript is nicely written and easy to follow.

Major comments:

I have reservations about the choice of the LPM models as calibration targets. I understand that the study is closer to the reality in which the age distribution is unknown. Still, I consider that it would have been much stronger to test the validity of the methodology on a pure synthetic case controlling every aspect of the problem: data and associated uncertainty, full shape of the age distribution, etc. An important aspect as well is that, without a priori information about the age distribution, a few LPM differing in their hydrogeological conceptual representation can equally fit. My point is that it is difficult to evaluate the validity of a calibration or inference methodology on real largely under-constrained cases. One way to tackle this would be I think to highlight the fact that the system studied here is a "not so complex" system (a textbook system?) and have been widely studied so that the target LPM is a more than reasonable estimation (see my minor comment below).

I have reservations as well about the independence for the percentiles and the further chaining approach. It appears to me that it goes again physics and flow mechanics to consider percentiles as separate entities, and not the age distribution as a whole. My point is that a LPM or numerically-generated distribution lies on a hydrogeological conceptual representation which describes the functioning of the system. It has been shown (Leray et al, 2019: https://doi.org/10.1016/j.jhydrol.2019.04.032) that local modification of the system properties affects not only local flow lines and mass balance locally but the overall response and functioning of the system and consequently the age distribution. So it is confusing to me that the distribution is considered by part (even if the chaining approach intends to reconstruct the puzzle)

Minor comments:

Line 26: I would write the age as plural ("understanding the ages of water") to reinforce the fact that natural groundwater systems are made of a wide variety of flow paths and consequently of residence times (or ages). If it is correct grammatically of course.

Line 53: "most such previous studies". Revise

Line 218: I am not an expert but should it not be half of the detection limit?

Lines 252 to 254: It is argued that the EPM provided good matches for a wide range of New Zealand systems. A fault-bounded, local, relatively homogeneous and thick system with uniform recharge rate upstream and zero recharge rate downstream looks like an EPM to me. So, I think the validity of the EPM should be argued considering specific aspects of the system (that may be quite similar to other sites in New Zealand)

Line 278: to differentiate.