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Discussions

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Interactive comment

Interactive comment on "Sensitivity of young water fractions to hydro-climatic forcing and landscape properties across 22 Swiss catchments" *by* Jana von Freyberg et al.

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This manuscript analysis stable water isotope signals in a range of contrasting catchments in the Swiss Alps to better understand what controls catchment storage and release dynamics. Based on a recently developed metric, the young water fraction (Fyw), the analysis provides a highly interesting and new perspective on the topic: the sensitivity of Fyw to stream flow. From my point of view, this topic alone would already merit publication. In fact, I would even argue that much of the additional analysis provided in the manuscript, specifically the comparison of the interpolation methods and the snow storage considerations, do not really add much value and actually somewhat dilute the really interesting story. I thus think these parts could easily be removed or at least be considerably shortened, but I leave this decision open to the authors.

Notwithstanding the well-designed experiments and in-depth analysis, the manuscript would benefit from some restructuring and, in places, from more precise and detailed explanations (see detailed comments below). My only major comment is the rather superficial discussion of the relationships between young water fractions and catchment characteristics (section 5). There were quite a lot of studies over the last 10-15 years (e.g. that looked into the relationships of the very same variables, e.g. soil types, L/G, drainage densities, area, TWI, precipitation intensity, etc. , with mean transit times (e.g. McGlynn et al., 2003, HP; McGuire et al., 2005, WRR; Laudon et al., 2007, JoH; Broxton et al., 2009, WRR; Tetzlaff et al., 2009, HP; Hrachowitz et al., 2010, WRR, 2010, HP; Soulsby et al., 2010, HP; Speed et al., 2010, HP; Asano and Uchida, 2012, WRR; Hale and McDonnell, 2016, WRR; and many others). Although the Fyw is an arguably more stable and thus reliable metric, it would be interesting to see and understand how the results and interpretations of the analysis presented here compares to these earlier studies. Can similar conclusion be drawn for Fyw than previously for MTTs? If yes, what does that mean? If no, why? Such a more detailed discussion would lend an additional, interesting edge to the manuscript.

In any case, I would be glad to see this work eventually published and I hope that the authors find my comments helpful.

Detailed comments: (1)P.2, l.8-9: “usually” is a quite unfortunate term here. Clearly, while there are quite some studies using “lumped-parameter” models (I suppose the authors referred to convolution integral approaches), there many(!) other studies that go far beyond that with many different types of models ranging from fully coupled 3D models to more conceptual models based on suites of storage tanks and the associated mixing coefficients/SAS functions. Please rephrase.

(2)P.2, l.10: “catchment storage” is inaccurate. It rather expresses some (essentially

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unknown) storage that is significantly affected by exchange processes. For many systems, there may well be significant additional storage below that, which remains essentially undetectable with stable isotope data due to potentially very long time scales of these exchange processes at depth (mostly molecular diffusion?). Please rephrase.

(3)P.2, l.12-13: is this generally true or is it not mostly due to the assumption of time-invariance? Again, please note that most model approaches, except lumped-parameter convolution integral approaches, do *not* rely on time-invariance of TTDs.

(4)P.2,l.16: perhaps better to use "estimated" than "obtained"

(5)P.2,l.13: to be precise, it should read as:"...from the differences in the amplitudes..."

(6)P.3,l.3-21: this is quite lengthy and written in an unnecessarily complicated way. The bottom line is, in my opinion, if only liquid water input to/storage in the system is considered or the total water input/storage.

(7)P.3,l.26: please clarify what is meant by "coefficients" of the seasonal cycles.

(8)P.4,l.5-17: some of the above references, analysing the relationships of catchment characteristics with MTTs would fit in nicely here and would place your manuscript into a somewhat wider context.

(9)P.4,l.32ff: also here, sine-wave fitting has been used already quite long time ago to understand transit times. Please add some references (e.g. DeWalle et al., 1997, HP; Soulsby et al., 2006, JoH)

(10)P.5,l.13: see comment (3)

(11)P.5,l.17ff, eqs.(3) and (4): redundant with eqs.(1) and (2). Instead of amplitude and phase eqs.(3) and (4) give the same information only expressed in sine and cosine components. I think eqs. (1) and (2) can be removed.

(12)P.6,l.26: what does "i. Br." mean?

(13)P.6,l.28: “accuracy” or “precision”?

(14)P.7, section 3.3: also here, some references to earlier papers that used similar and partly the same predictor variables would be good

(15)P.7,l.32: do flow path length and gradient refer to subsurface or total length and gradient to the outlet? Please be more specific.

(16)P.8,l.20-22: was the use of multiple linear regressions considered to better identify potentially spurious correlations? If not, why?

(17)P.8,l.25ff, section 4.1: does this section actually add value to the manuscript? I think, the section can at least be considerably shortened if not condensed altogether.

(18)P.8,l.25-P.9,l.14: this would fit much better into the methods section

(19)P.9,l.28-29: although this term is widely used in our community, I do not think that in any environmental system application we can actually “validate” a model in the actual sense of the word. The best we can do is to rigorously test our models.

(20)P.10,l.1ff, section 4.2: see comment (17). If you decide to keep the section, more detailed descriptions of the model used for the snow dynamics (including parameters, calibration procedure, uncertainties involved, etc.) is needed and can be placed in the supplementary material. In addition, I may have missed it, but it is unclear what PREVAH stands for.

(21)P.11,l.1: not clear what is meant by “…shifts the seasonal isotope pattern toward later in the season.” Does this refer to the amplitudes? If yes, please say so.

(22)P.11,l.2-3,fig.4: it would be easier for the reader to appreciate the information content of figure 4, if the phase would be given in days (or months) rather than in radians.

(23)P.11,l.19-23: “...young water fractions…that are larger…because high flows generally contain more young water…”. This seems a bit of circular reasoning to me.

(24)P.12,l.3-4: repetition of what was said earlier. Can be omitted.

(25)P.12,l.1ff,section 5: again please see comment (8)

(26)P.12,l.31-34: sure, a few studies could identify area as potential control on MTTs, but others clearly could not (see in the given references above). Thus please rephrase this statement.

(27)P.13,l.28: this interpretation is of course possible, but it surprisingly seems to not consider the potentially important influence of fast, lateral preferential flow pathways (e.g. macropores), which can be abundant in particular at (steep) forest sites. It may be worth reflecting on this a bit more.

(28)P.14,l.4: what is meant by “bigger” cycles?

(29)P.14,l.17: the description of how this was in detail done remains quite vague. Please provide a more detailed description in the methods section. Were samples from time periods outside the individual quartiles simply removed and the sine wave refitted on the remaining samples? How many samples on average were the individual fits then based on? The information content of the 4th quartile and the top 20% is very similar. One can be removed.

(30)P.15,l.1-12: it is not entirely clear in how this is different to what was done in 6.1. Please also here, provide a more detailed description in the methods section of what was done and how.

(31)P.17,l.23-24: which, in turn, would imply (to maintain the fraction of young water in spite of increasingly more young water in the system) an increasingly preferential sampling of older water as the system gets wetter.

(32)P.17,l.26ff: this is a very interesting analysis, but it remains unclear, which parts of it are actually supported by the available data/results and which are mere speculation. Please try to make it clearer, which evidence supports these interpretations.

Best regards, Markus Hrachowitz

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