

Interactive comment on “Lumped convolution integral models revisited: on the meaningfulness of inter catchment comparisons” by S. Seeger and M. Weiler

Anonymous Referee #2

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1 General comment

The paper investigates the response functions of 24 Alpine catchments through a history matching of measured and computed signals of streamflow isotopic composition. The computation is performed using lumped convolution integral models, assuming the response function to be linear and time invariant. Three different types of distributions are tested, i.e. the exponential model, the gamma function and the two parallel linear reservoir. Once the parameters of each distribution are optimised through calibration, the corresponding mean values are calculated and compared. Finally, the authors seek

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correlations between the estimated mean values and the geomorphologic characteristics of the study catchments.

The paper is well organised and the research topic should be of general interest to HESS readers. The analysis is very well supported by quantitative results and the problematics involved in finding a topography-driven regionalisation method are clearly discussed. Even though stationary response functions have progressively lost scientific relevance, owing to recent theoretical and experimental advances in the field of time-variant transit time distributions, many simple models still perform discharge simulations through calibration of stationary response functions. Hence, the presented results in terms of mean transit times do not strictly provide physical information on the underlying hydrological processes, for which the stationarity assumption cannot hold, yet provide a useful reference for model calibration in other ungauged catchments, having similar meteorological and geomorphological characteristics.

However, the methods applied are not always convincing, in particular the normalisation of the response functions. This practice, which is presented as necessary for a numerical implementation of the convolution model, seems to me confusing and unnecessary. Moreover, the relevance of the work is never clearly stated in the manuscript. A more consistent use of the terms "transfer function", "response function" and "transit time distribution", to which I would give very different meanings, would also help the reading and the understanding. Accordingly, I think the paper should be published under major revisions.

2 Detailed comments

- Title: I don't think the title conveys the essential information about the paper. I would suggest to change it, focusing more on the core of the work, i.e. the determination of the catchment response functions from isotope data and the

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correlation between topographic indices and mean response times.

- Page 6754, line 1: Please rephrase to "The transit time distribution of water particles in a catchment is linked to..."
- Page 6754, line 2: Please rephrase to "This distribution generally depends on the physiographic and hydro-geological conditions of the catchment and is therefore not easily determinable".
- Page 6754, line 9: Here the term "transfer function" is mentioned for the first time, apparently with the same meaning of transit time distribution. I would suggest to be more cautious and consistent in the use of the terminology throughout the paper: transfer function (or, as it is sometimes called in the manuscript, response function) and transit time distribution are conceptually different. The transfer function, in fact, describes the causality between input pulses and output signals at the outlet, without requiring that the water that flows out is exactly the same water that was injected. The transit time distribution, instead, implies this link. Evidently, the work does not investigate transit time distributions, for which the stationarity assumption cannot hold, but rather transfer functions. I would therefore avoid using the term "transit time distribution" and replace it with "hydraulic response function" and "tracer response function", respectively for discharge and isotopic composition. Accordingly also the MTT should become MRT (mean response time).
- Page 6754, line 10: It was not clear to me why you decided to introduce the normalised response function. I do not see the necessity, neither from a mathematical nor from a numerical point of view. Mathematically, the normalisation introduces errors, because you are constraining the mass of the distribution in a finite range, which is given by the length of your record and is thus arbitrary. In fact, there is no physical reason to assign an upper limit to the random variable "response time". From a numerical perspective, the convolution is effectively

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computed by calculating the mean value of the distribution in the different time steps, without normalisation. This procedure is sufficient to conserve the mass, as it is correctly explained at the end of Appendix A. For these reasons, I would suggest to completely delete any references to normalisation and normalised distributions throughout the manuscript.

- Page 6754, line 20: "which were also correlated to the mean annual precipitation sum". It is not clear if the authors observed a correlation among the geomorphological and meteorological characteristics of the study catchments. Please clarify.
- Page 6754, Abstract: In the abstract a sentence regarding the scientific relevance of the work is missing. I would say something at the end of the abstract.
- Page 6755, introduction: In the introduction there are no references to a whole line of research that sought a more in-depth theoretical understanding of the non stationarity of the hydrologic response, the water age mixing and the old water paradox. I would suggest considering the relevant work of Botter et al. 2010 "Transport in the hydrologic response: Travel time distributions, soil moisture dynamics, and the old water paradox", Botter et al. 2011 "Catchment residence and travel time distributions: The master equation", Botter et al. 2005 "On the Lagrangian formulations of reactive solute transport in the hydrologic response", Rinaldo et al. 2011 "Catchment travel time distributions and water flow in soils".
- Page 6756, line 22: "...assumed time invariant transfer functions". Here, I would briefly discuss the implications of this assumption. The resulting mean values of the response functions (mean response times) are not mean transit times (because the stationarity assumption cannot hold). Though, they can still give useful information on the catchment behaviour. A sentence discussing the relevance of the work would also be appreciated here.

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- Page 6759, line 16: What type of initiation threshold did the authors use? drainage area threshold or slope-area threshold?
- Page 6760, line 3: The way DD is defined in the manuscript does not correspond to the traditional drainage density, which is $L/A [m^{-1}]$ (length of the streams over catchment area). It could be calculated, using a DTM, as the inverse of the mean distance from the stream $1/ \langle D \rangle$, where D is calculated for each non-stream pixel along the steepest descent direction. Why did the authors choose this definition? Different results would possibly be obtained if DD was computed as $1/ \langle D \rangle$?
- Page 6760, line 15: Please correct to "...every measurement sites, as follows:"
- Page 6760, Eq. 1: What are the implications of assuming a constant vertical gradient \bar{g} of isotopic content? Is this a reasonable assumption that is supported by previous studies?
- Page 6761, Eq. 5: It is not clear to me why only \bar{i}_s was interpolated and not i_s . If both were interpolated, there would be no need of identifying the closest measurement point s^* .
- Page 6761, line 16: It is not clear what is the purpose of considering the transit time proxy. "To complement the lumped convolution modelling" is too vague. It becomes clearer later in the manuscript but for the reader would be useful to have a more precise explanation here.
- Page 6761, Eq 6: I would suggest giving directly the definition of TTP, instead of defining ITTP and then saying that you preferred using TTP.
- Page 6762, line 8: Please change "alpine" to "Alpine*."

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- Page 6762, Section 3.4.1: Additional information about the snow model would be useful. What is the resolution of the model? Do you account for the shading effect in the computation of incoming shortwave radiation? Do you account for snow drift?
- Page 6764, Eq. 8: Please change dt to $d\tau$ at the numerator.
- Page 6765, line 9: Please change "pareto" to "Pareto". Also, briefly explain the meaning of Pareto-optimal parameters sets, for non specialised readers.
- Page 6765, line 24: "...with a population size of 1500 and 20 generations". I could not understand this. Are the generations the number of parameter sets that you extract? what is the population then? Please clarify.
- Page 6766, Eq. 9: I would not replace KGE' with E and $KGE-$ with E_r . Please assign only one symbol (possibly just a single letter) to each variable.
- Page 6766, line 24: Please rephrase "...models under two aspects:".
- Page 6767, line 21: Please explain the meaning of Pareto-fronts for non specialised readers.
- Page 6768, line 3: If you decide to abbreviate transfer function as TF , please start doing it since the beginning of the manuscript. At this point of the text you have already mentioned this term several times and it seems a bit too late for an abbreviation. I would anyway suggest using the term response function and the abbreviation RF .
- Page 6771, line 17: On which basis did you select the five catchments? Do they show any particular features or are they representative for all the other catchments?
- Page 6772, line 5: Please correct "TWI, G and L/G were..".

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- Page 6772, line 19: Please correct "...the drainage density DD ".
- Page 6755, lines 21-26: I am not sure I understand or agree with the explanation. I think the reason why the tails of the distributions were not influential in the computation of the objective function is rather caused by the length of the record, which is less than 3 years. Accordingly, when the convolution is computed over such a relatively small time period, the tail of the distribution (which in some cases extends far beyond the length of the record) does not play any important role.
- Page 6776, line 10: Please correct "assess".
- Page 6776, line 23: Since the main problem involved in the estimation of the MTT (that I would call MRT "mean response time") is the poor influence of the tail of the distributions, I would add here that reliable MRT estimates are not possible without a longer data set, because of the aforementioned reasons.
- Page 6777, line 5: Please correct "...seems suited to...".
- Page 6777, line 12-18: "McGuire et al. (2005) ... in the Scottish Highlands" all this information was already given in the introduction, so here it is enough to provide the references without any further detail. I would rephrase as "This correlation was also observed by McGuire et al. (2005) and Tetzlaff et al. (2009b). Hrachowitz et al. (2009), on the other hand, did not reach similar conclusions, possibly due to the choice of a fixed initiation threshold for the stream network delineation in all the 20 study catchments".
- Page 6778, line 8: "...to normalise MTTs by their respective mean annual precipitation". This procedure does not seem reasonable to me, unless it is actually shown that the mean response time scales linearly with the mean annual precipitation (which seems to me unrealistic given the strong non linear relation between soil moisture and response time). I would rephrase as "...first eliminate the influence of such first order climatic controls."

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- Page 6780, Appendix A: I would suggest to delete this section of the manuscript. Otherwise, the authors may decide to keep it and give additional details on how they computed the numerical convolution. The first bullet point explains why it is necessary to consider the mean values of the distributions in the time steps to correctly compute the numerical convolution. After this short explanation, I would give the formula used for computing the convolution, which I imagine is something of the type

$$C(t_i) = \frac{\sum_{j=0}^i \bar{g}(t_i) p_{eff}(t_i - t_j) C_P(t_i - t_j) \Delta t}{\sum_{j=0}^i \bar{g}(t_i) p_{eff}(t_i - t_j) \Delta t}, \quad (1)$$

where

$$\bar{g}(t_i) = \frac{1}{\Delta t} \int_{t_i - \Delta t/2}^{t_i + \Delta t/2} g(\tau) d\tau. \quad (2)$$

- Table 5: In the caption, please correct "Minimum".
- Fig. 2: Maybe the authors can find a way to convey the information with a simpler scheme. E.g. I would use only one arrow connecting the box "input variables from PREVAH" to the box "snow module".
- Fig. 5: The plots on the right are very confusing. I could not really understand why there are so many lines having the same colour but different thickness. "Thinner lines indicate ranges of the best solutions" is not really clear. Range of what? Why don't you show ONLY the ones giving the best solution? I imagine that after removing the lines of the normalised distributions the plots may be more clear, but I would still suggest to explain it better.
- Fig. 8: If I understand correctly TTP is the inverse of the damping ratio. So in the vertical axis I would replace "1/damping ratio" with TTP .

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- Fig. A1: The Figure on the left may be useful to understand the numerical computation of the convolution. The Figure on the right should be removed.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 6753, 2014.

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