

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

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We appreciate the numerous and helpful comments of Anonymous Referee #2.

General comment

[. . .]. 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

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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.

Reply

After a similar remark of Anonymous Referee #1, we came to the conclusion that the reasons we had to include the results of normalised transfer functions (see our response to Anonymous Referee #1) were insufficient to justify such a detailed consideration of those results. In the revised version of the paper, we will focus on the comparison of the unnormalised, mathematically correct transfer functions and we will reassess our use of terminology.

There are a lot of helpful detailed comments. Many of them will be adopted into the revised version of the paper without further mention in this document. In the following section we will concentrate on all of the detailed comments which contain questions and on those comments which demand responses. Please understand that for this response we will stick to the terminology in the original paper.

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 correlation between topographic indices and mean response times.

Reply

We will think of a more appropriate title for the revised version of the paper.

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- 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).

Reply

We agree to reassess our use of terminology and will take care to be consistent with its use in the revised version of the paper. We will seek to comply to the terminology used by McGuire and McDonnell (2006) and McDonnell et al. (2010).

- 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 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

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these reasons, I would suggest to completely delete any references to normalisation and normalised distributions throughout the manuscript.

Reply

The use of normalised transfer functions results from an earlier improper implementation of the convolution integral. We thought it might be interesting to compare the results of normalised and unnormalised transfer functions and to show that while the tailings were distorted by normalisation, this did not affect the evaluation of the simulations at all. We do admit that there is no real justification for considering an arbitrary, improper implementation variant and will remove the normalised transfer functions from the revised version of the paper.

- 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.

Reply

With the full sentence "However, the collinearity of those [geomorphological] indices, which were also correlated to mean annual precipitation sums [...]" (page 6754, lines 19–20), we intended to convey that we observed a correlation between geomorphological characteristics and mean annual precipitation sums. When rewriting the abstract for the revised version of the paper, we will make sure to clarify this.

- 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

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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".

Reply

We will adapt the introduction and include the proposed references.

- 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.

Reply

For the revised version of this paper we will adapt the introduction accordingly.

- Page 6759, line 16: What type of initiation threshold did the authors use? Drainage area threshold or slope-area threshold?

Reply

We used a drainage area threshold and will add this information in the revised version of the paper.

- 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

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results would possibly be obtained if DD was computed as $1/\langle D \rangle$?

Reply

When we computed DD the way described in the manuscript, we also had its traditional definition as L/A in mind. Under the assumption of a sufficiently highly resolved DTM and the further assumption that different catchments' channels' directions are similarly distributed with regard to the raster orientation, we supposed our metric should be a sufficiently good approximation to L/A . However, now that we computed DD based on your suggestion, the two kinds of DD notably differ (R^2 of 0.46) and the correlations to our MTT estimates decreased further. In the revised version of the paper we will make sure to use correctly computed DD values.

- Page 6760, Eq. 1: What are the implications of assuming a constant vertical gradient g of isotopic content? Is this a reasonable assumption that is supported by previous studies?

Reply

Siegenthaler and Oeschger (1980) have clearly shown that there is a vertical gradient of isotopic content in precipitation for the study area and we also saw this gradient in the site data. Most of this gradient is linked to the vertical temperature gradient, as there is a clear influence of the condensation temperature on the isotopic content of the resulting precipitation (Dansgaard, 1964). Apart from seasonally varying atmospheric conditions, temperature differences in the study area are predominantly caused altitudes differences.

To account for the seasonality, we did not assume one constant vertical gradient of isotopic content over the whole year, but we computed average gradients for each month of the year. We are aware of the fact, that the assumption of constant height gradients for each month of the year will certainly not hold for each and every specific month contained in our study, as atmospheric conditions do not strictly align with

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calendrical dates and we are sure that more sophisticated ways to estimate the vertical height gradient are conceivable. Within the scope of this study we decided on the described procedure and we were content with its results (see Appendix B and Fig. B1 in the discussion paper).

- 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^* .

Reply

Our input time series of isotopes in precipitation exhibit various gaps (see the attached Fig. 2 of this reply). In months with few available site data, a direct interpolation of the available monthly site data will inevitably fail to reproduce the real spatial heterogeneity. By basing the estimation on average monthly values, we may retain at least the average component of spatial heterogeneity.

- 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.

Reply

Since we directly referred to the ITTP defined by Tetzlaff et al. (2009), we chose to repeat their definition. We agree that this is not the best way to define the TTP we used in the study and will seek to reformulate the section in question and add a more precise explanation why we decided to include the into the study.

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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?

Reply

Except for the modifications listed on page 6762 (lines 21–24), the snow model basically is ESCIMO (Strasser2010), which is a point based energy balance model. Therefore, it has no spatial resolution and we did not account for shading effects regarding incoming shortwave radiation or snow drift. In accordance with the available meteorological input data (see page 6758, lines 5–8) the model was computed for 100 m elevation bands of each catchment. We will add this information to the revised version of the paper.

- Page 6765, line 9: Please change "pareto" to "Pareto". Also, briefly explain the meaning of Pareto-optimal parameters sets, for non specialised readers.

- Page 6767, line 21: Please explain the meaning of Pareto-fronts for non specialised readers.

Reply

We will make sure to explain the concepts of *Pareto optimality* and *Pareto front* in the revised version of the paper.

- 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.

Reply

Assuming that anyone interested in the details of the NSGAll algorithm would resort to the given reference (Deb et al., 2002), we avoided to give more information on the algorithm specific meaning of *population size* (which is the number of parameter sets)

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and *generations* (which is the number of iterations of the algorithm). In consideration of your comment, we understand that the details given and the details withheld might hamper the understanding of the paragraph in question and we will improve this paragraph in the revised version of the paper.

- 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.

Reply

We will introduce the abbreviation earlier in the paper and pay attention to a more consistent use of it in the revised version of the paper.

- 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?

Reply

We intended to choose catchments which represent all occurring types of distributions encountered in our study. As shown in Fig. 6, which shows the RTDs and TTDs of all catchments, the five selected catchments' distributions (coloured lines, belonging to the same five selected catchments whose results are depicted in Fig. 5) encompass the other catchments' distributions and also contain some intermediate cases. In the revised version of the paper, we will seek to make our intentions regarding the selection clearer.

- 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

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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 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.

Reply

In that point we disagree. Our simulation period encompassed 20 years and we used an equally long time series of precipitation isotope data. The increased damping of the input signal towards the tailing of a transfer function with heavy tailing has nothing to do with the length of the output validation data time series. There is no measurable difference between a seasonal oscillation signal damped over 10 or 100 years: both will lie around the average value and both will be overlain by short term variation and noise. Longer stream discharge isotope data time series may be beneficial to decrease short term climatic influences on time-invariant transit time estimations or enable time-variant transit time modelling, but as long as the only considered input signal are annually recurring stable water isotope concentrations, they will not help to identify transit time distributions' tailings beyond a few years.

- 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

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$$C(t_i) = \frac{\sum_{j=0}^i \bar{g}(t_i) p_{eff}(t_i - t_0) C_P(t_i - t_j \Delta t)}{\sum_{j=0}^i \bar{g}(t_i) p_{eff}(t_i - t_j) \Delta t} \quad (1)$$

where

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

Reply

Yes, the two equations you provided mostly correspond to the way we implemented the convolution, and we will include them into the revised version of the paper. We would argue against deleting this part of the appendix, as our intention was to give more insight into the way we implemented the convolution integral:

$$C(t) = \frac{\int_0^t g(\tau) p_{eff}(t - \tau) C_P(t - \tau) d\tau}{\int_0^t g(\tau) p_{eff}(t - \tau) d\tau} \quad (3)$$

While many papers state a convolution integral like in Eq. 3 or a similar form, none of them explicitly convey how they implemented them into their code. For ? we know that the applied implementation worked with point based computations of transfer function values. In most cases the differences between the methodologically flawed point based computation and the mathematically correct time averaged computation of transfer function values should remain negligible. We would argue that not even normalisation will lessen the validity of transit time studies working with stable water isotopes. Both of these practices cause imprecisions, one at the shortest time scales, the other at longer time scales. As long as the temporal resolution of the input data is the limiting factor, those parts of the transit time distributions are under-determined anyway.

This might cause problems, as soon as other kinds of isotopic input data (higher temporal resolution, or additional ³H data) are available. Therefore it might be beneficial

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to insists on the use of a mathematically correct implementations and to increase awareness towards possible (as yet non-effective)flaws in existing implementations. We will point this out more clearly in the revised version of Appendix A.

- 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".

Reply

Thanks to this comment, we realised that the explicit depiction of the five meteorological input variables does not help to convey the essential information and we merged them into one box (see first appended Figure). Apart from that, we would refrain from further simplifications of the model scheme.

- 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.

Reply

We will remove the normalised distributions and hope that this step will make the right column of the figure easier to comprehend. The bold lines in the right column of subfigures of Fig. 5 do not show the results of one particular parameter set, but represent the median value of 30 to 100 Pareto-optimal parameter sets, while the thinner lines indicate the upper and lower ranges of those Pareto-optimal parameter sets. We will clarify this in the figure's caption.

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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.

Reply

We agree and will remove the figure, which loses its purpose when the normalised transfer functions are removed from the paper.

References

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

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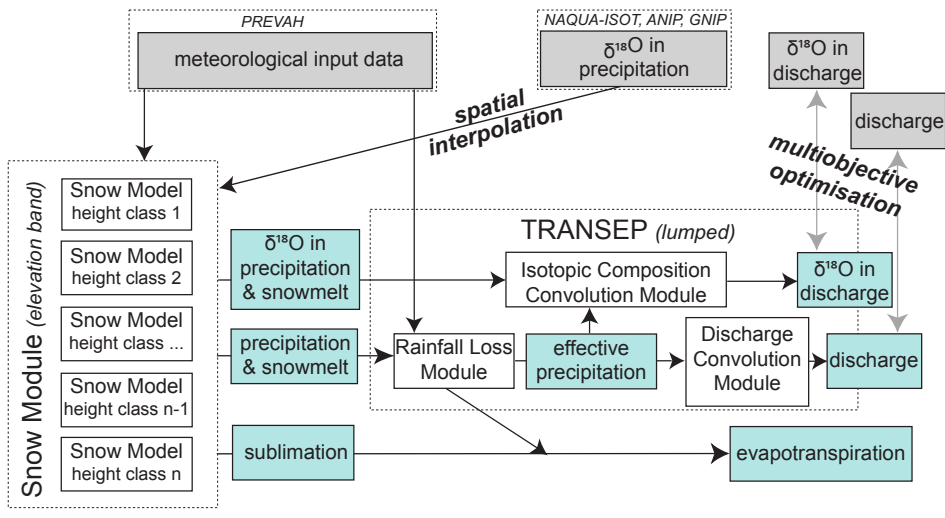


Fig. 1.

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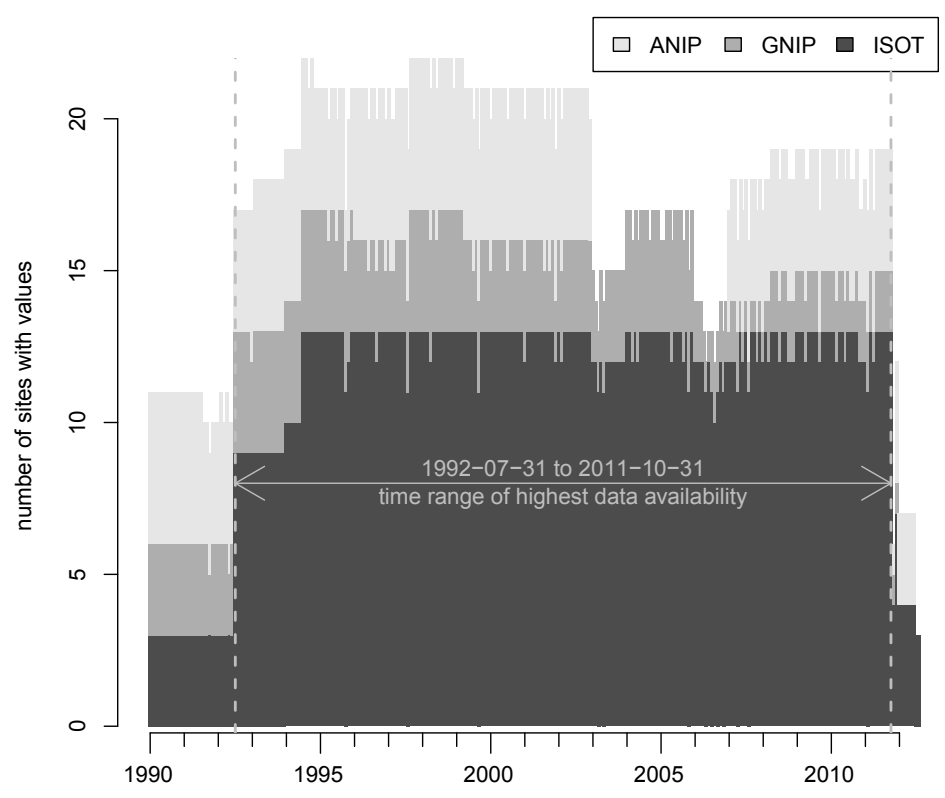


Fig. 2.

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