Answer to Reviewers regarded the paper: "Age-ranked hydrological budgets and a travel time description of catchment hydrology

Answers to Dr. Markus Hrachowitz

The authors did a commendable job in addressing my previous comments. The contribution of the manuscript to literature is now much clearer and by putting the analysis into a wider context I am sure it will reach a extended audience. I really enjoyed reading the clarifications and the revised manuscript and recommend publication as is.

We thank Dr. Hrachowitz for his comment. His review helped us to refocus our manuscript, and we are happy that he enjoyed reading it.

I have only one small comment concerning the authors' response (11) to my earlier comments: I maintain that in figure 9 of the mentioned article the forward TTDs are shown and, as can be seen, their CDF-proxy does *NOT* add up to 1. This highlights, in my understanding, exactly the authors' argument that at a given time t the full forward TTD (i.e. the distribution of times until a given input signal has completely left the system) is not known.
best regards, Markus Hrachowitz

If we properly understand the comment (note that there is no Figure 9), Dr. Hrachowitz is correct. The comment, however, does not imply any change in the manuscript.

Answers to Dr. Paolo Benettin

General Comments

As pointed out in my previous review, I think this is an interesting article that offers a complementary view on the theoretical foundation of hydrologic transport at catchment-scale. The theory is formulated using probability-theory formalism, which helps making the formulation more general. The paper still includes some little faults which should be polished upon publication. I tried to annotate them in the attached pdf. The English form should also be improved, especially in the new parts introduced after the first review. As one of the authors seems to be English mother tongue, I would suggest a final English proofread. I also write below some minor comments which I hope may help the authors to further improve the paper. I therefore recommend the paper for publication after technical and language corrections are done.

We thank Dr. Benettin for his review. His observations were very constructive and detailed, and we used all of them. We have edited the writing style and English grammar in the new manuscript, and provided answers to each of his new comments.

Minor Comments for the authors

I still believe that some more physical interpretation would help the reader. I report here an example from line 151, where $p_Q(t - t_{in}|t))$ is defined as the "pdf of travel time". For this particular example, the authors could e.g. add that it represents the probability that water in the output Q has entered the system in t_in

Added

Meaning of clock-time t: I personally find the separation between the "past" which is completely known and the "future" which is completely unknown a bit too extreme and distant from applications. If one has twenty years of hydrologic data from the year 1990 to 2009, all the forward and the backward distributions that can be estimated over that period will be truncated. However, many of them are likely to be determined up to a satisfying point.

True. However, this has to do with the need to use historical (past) data in papers. For real-time applications, the perspective changes, and our view is a necessity.

I think an important clarification should be included in the application to the IUH case (Section 10), as it's been fully clarified in the

literature that the IUH describes how a catchment reacts to a precipitation event, but it does not describe the actual time that water takes to travel across a catchment.

Good observation. The clarification is added in the new text. However, it occurs in section 5, not in section 10.

I think it should be made clearer that a linear relationship between the bulk discharge and the bulk storage $Q(t) = \frac{1}{\lambda}S(t)$ does not imply that each single rank storage is proportional to each single rank volume, as it only implies: $\int_0^{\min t,t_p} q(t,t_{in})dt_{in} = \frac{1}{\lambda}\int_0^{\min t,t_p} s(t,t_{in})dt_{in}$

Dr. Benettin is right that mathematically the linear relation of the whole system does not imply an equivalent linearity for the subsystems. In fact,

$$\int_0^{\min(t,t_p)} \left(q(t,t_{in}) - \frac{1}{\lambda}s(t,t_{in}) \right) dt_{in} = 0 \tag{1}$$

just implies that the integrand would be either null, or an oscillating function (in t_{in}) which integrates to zero in $[0, \min(t,t_p)]$. To obtain this, however, it is easy to show that the subsystems' lambdas should become time variant, and must be chosen accurately "ad hoc" to nullify the integrand for any clock time t. Therefore the linear bulk system as obtained by linear subsystems remains the simpler option. We added in the text: "assuming that the age-ranked storages behave linearly ...".

In section 9 the authors stress that the SAS functions "should be derived rather than arbitrarily imposed". This would be true if C_Q and C_J were analytical functions (and the system of equations were easy to solve), but in reality they are data collected at some (often coarse) frequency. It could be at least mentioned that a feasible alternative is that of testing possible shapes of the SAS functions against the available tracer data, which in the end is a $C_Q(t) = \int C_J(t_{in})\omega_Q(t-t_i)p_S(t-t_i|t)dt_{in}$ I think this testing is very different from "arbitrarily assuming" their shape.

We dropped the word "arbitrarily". However, equations involving concentrations, do not require knowledge of $C_Q(\,)$, but of the injection and storage concentrations, which are assumed to be known in all the approaches.

Notes in the paper

Page 2 - line 46 - There is something wrong in this phrase: "A more detailed history of these concepts can be found in Benettin et al. (2013) and Hrachowitz et al. (2016) and Appendix B, is more specifically related to this paper"

The phrase was modified: "A more detailed history of these concepts can be found in Benettin et al. (2013) and Hrachowitz et al. (2016). Appendix B includes a brief review more specifically related to the topic of this paper.

Page 5 - Equation 9. I think it would be useful to explain in words what the interpretation of eq (9) is (especially because the derivative d/dt is not developed into partial derivatives, so the interpretation is slightly different from van der Velde's and Harman's equations).

To the phrase: "These equations were introduced first by van der Velde et al. (2012) and named by Harman (2015a), even if similar ones were present in previous literature, as discussed in Appendix B" we added: "In our formulation, however, by using t and t_{in}, instead of t and T_r, as independent variables we do not need to transform the original ordinary differential equations into partial differential equations."

Page 6 -line 143 - eq. 10 - Yes, but what is this p ?

We changed it to p_S, as it should be.

Page 6 -line 187 - Yes, but what does this physically mean? I am afraid the reader would have a hard time understanding this.

What is to be understood is that this is not a pdf in t, but just in t_{in}. This fact remained kind of unclear in previous treatments of the matter, because they did not use conditional probabilities. This is actually explained in the lines below 187 (first revised manuscript).

Page 8 - Figure 2 - Why does the x-axis is "injection time"? Indeed, the y-axis label says that ps is a function of t-tin, i.e. the residence time.

The the domain of the pdfs (in this case backward residence time pdfs) is bidimensional: one dimension is the injection time (given by the time series of the inputs), the other one is the actual time (how each input evolves with the actual time). If we consider to have the injection times in rows and the actual times in columns, Figure 2 is obtained by plotting all the results in the same column (t_{in} varies and t is fixed) while Figure 4 is obtained by plotting all the results for the single row (t_{in} is fixed and t varies)

Page 8 - line 196 - I am not sure this new notation with the subscript S is appropriate here and in the following paragraphs. This variable does not describe a property of the storage. It is just the (cumulative) probability that a precipitation input entered at a past

time ti will exit the system at time t (either through Q or ET)

It is a property of the storage, since it is a function of the storage age-ranked functions. This applies in the whole paragraph.

Page 11- Caption of Figure 5 - Forward distribution of what?

Residence time forward distribution.

Page 12 - please mention which SAS is used for the simulation

We specified in the text that we were considering the complete mixing case ($\omega_Q(t, t_{in}) = \omega_{E_T}(t, t_{in}) = 1$).

Page 16 - I think this is the variable that should be defined as pS, for consistency with the backward framework (eq 10)

We changed with p_S everywhere.

Page 16 line 299 - this variable is not defined and does not appear in the notation appendix

It should be: $p_Q = p_{t_{ex}} * p_S$. However, because this passage is not very informative we deleted the whole sentence from line 297 to line 300.

Page 16 - line 308. Unclear

We changed the phrase as follows: The variable t_k, used for making compact equations above and below, is such that $t_0 = t_{ex}$ $(k = 0)$ and $t_1 = t$ $(k = 1)$.

Page 18 - please make it clearer that the perfect mixing just refers to the water sample (to make sure the reader cannot think the entire catchment is well mixed)

We changed with: "When a sample is taken, the action implies perfect mixing of all the age-ranked waters in the container where measurements are made."

Page 19 - this title is a bit unclear

We changed the title to: "A simple example where probabilities are assigned instead of derived."

Page 19 - this is prone to misunderstandings, as the "mean travel time" in the IUH framework actually refers to the response time of a catchment (which involves the displacement of old water) and not to the actual water travel times. If one estimates $\lambda$from hydrological measurements, the actual water travel time would be underestimated by orders of magnitude.

We changed "mean travel time" to "mean response time" and specified that it refers to forward distributions, not to backward ones.

Page 20 - this is not a results. You have assumed a uniform SAS function from the beginning!

Not true. We are not assuming SAS here. Instead, we are assigning the forward storage probabilities. Thus SAS values result from this assignment.

Page 21 - but can't $ae(t, t_{in})$, in general, depend on $s(t, t_{in})$? Then eq. (77) would not be linear anymore.

This discussion can be applied to eq. (76). Obviously the dependence on ET could be made non-linear, and it probably is at least slightly non-linear. In that case, solutions have to be found numerically. However, all the cases made by Dr. Benettin in his papers, to our knowledge, are linear.

Page 27 - I think your approach is "spatially integrated" rather than "coarse grained". You can actually derive the budget equation by integrating the equations of the local approach.

In the revised manuscript, we changed the text to "spatially integrated".

Page 29 - too vague as a sentence. Which "experimental results" do the authors talk about? I think the cited paper does the opposite: it shows how one single reservoir can be used to properly model discharge.

Dr. Benettin is correct about the citation to this specific Kirchner paper, but, since it is unessential to our arguments, the citation have been dropped. We rephrased it as follows: "Looking at the literature we cited in the main text, it is usually recognized that a single reservoir is not able to reproduce proper discharge and tracer behavior. Usually a few "embedded" reservoirs are deployed in models."