Hydrology and
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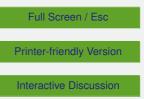Sciences

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Discussions

Interactive comment on "Understanding mean transit times in Andean tropical montane cloud forest catchments: combining tracer data, lumped parameter models and uncertainty analysis" *by* E. Timbe et al.

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The manuscript "Understanding mean transit times in Andean tropical montane cloud forest catchments: combining tracer data, lumped parameter models and uncertainty analysis" by Timbe et al. explores differences in transport processes in different parts of the hydrological system. Let me upfront say that in spite of the comparatively simplistic (i.e. time-invariant) modelling approach I do really like the approach taken by the authors in the manuscript under review as it provides and analyses an interesting

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tracer data set from different hydrological components such as soil, springs, etc. and provides some level of experimental insight on the differences in transport processes between these components. I do have, however, some comments that I would encourage the authors to address in detail. The major points being that firstly I think that the manuscript is too much focussed on the choice of models themselves rather than on what these models can tell us about the underlying processes and the way water is routed through the system. Secondly, I found that the methods section is kept very superficial and requires some more attention and detail.

Please find detailed comments below:

1) p.15872, l.26ff: this is quite an unusual definition of what a TTD is. And I am not entirely sure it is correct. Please check.

2) p.15873, l.15ff: where do the 12 years come from? This seems a bit too specific. In addition, there is a wide range of other methods than carbon dating for older waters. Thus maybe rather say "[. . .], while, for example, carbon isotopes are employed [. . .]"

3) p.15873, l.20: I do not think that these methods can be called "traditional" when it comes to tracer routing. These are quite recent developments, really, compared to the use of the convolution integral technique used here. The first and for a long time only ones who did it were to my knowledge Barnes and Bonell (1996).

4) p.15873, l.22-28: Good point! But not the MTT itself is of primary importance here (and elsewhere in the manuscript) as it is just a very reductive metric. It is rather the shape of the TTD that is of interest as it gives information about the underlying mixing processes and the way water is routed through the system.

5) p.15874, l.3: the terms "more recently" and "new" seem a bit out of place for a paper that has been published almost one and a half decades ago.

6) p.15874, l.3-8: it would be good if this could be put more into context of actual hydrological function. Why are TPLR and GM more flexible? What can they do better?

For example: they allow the representation of different mixing processes in different system components, such as soil and groundwater. In contrast, EM-based models assume instantaneous and complete mixing over the entire model domain, which is only likely in few, if any, surface water systems (see e.g. Hrachowitz et al., 2013)

7) p.15874, l.17-21: seems to better fit into the methods section

8) p.15875, l.7: In science, except mathematics, it is close to impossible to verify or confirm hypotheses (Popper, 1959). In addition, as hydrology is an inherently inexact science it may frequently also prove difficult to reject hypotheses simply due to inadequate, i.e. scarce or erroneous data (e.g. Beven et al., 2012). I would thus suggest to replace "confirm or reject" by the more neutral "test"

9) p.15875, l.9-20: not sure this is correct. How did you test if tracers are conservative?? How did you test that there are no stagnant waters? How did you test that stationary conditions are dominant? The use of lumped equations does tell you very little about that. They can also be fit to a non-stationary system, trying to get the best average fit. It seems to me as if in this paragraph the authors mixed assumptions with hypotheses they wanted to test.

10) p.15877, l.5: should read as "Major"

11) p.15877, l.16ff: there is an entire paragraph about streamflow observations. It is however not clear what it is needed for in this study except to define base flow conditions. Can be largely condensed.

12) p.15878, l.21-23: how were the rain event samples obtained? Automatic sampler? An eager student who changed the sampled bottle after each event? Does this also mean that there were samples that spanned for example 2 hour periods, 13 hr periods or, whatever, 4.32 day periods?? What did you do if there were 2 or more events during one day? Conversely, what did you do when there was a 2 day rain event? Please provide details.

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13) p.15879, l.1-12: as tracer input "concentrations" always need to be flux weighted, I was wondering which precipitation amount was used for each elevation zone. Did you use the catchment average rain for each elevation zone? Or did you rather use some kind of elevation corrected precipitation for each zone? That could make quite a difference in the catchment averaged input signal!

14) p.15880, l.1-2: please justify in a bit more detail.

15) p.15881, l.18ff: this is the greatest mystery for me in this manuscript. Why would the authors choose to dismiss an interesting high-resolution data set to aggregate the available observed daily input data to weekly values??? That is quite an amazing waste of valuable information. Even if rainfall events spread over two or more days, a uniform input distribution over this period can be assumed. I am pretty sure that the uncertainty introduced by that assumption is easily more than compensated for by the gain of additional information.

16) p.15881, l.22-23: it does not matter which signatures are used in precipitation free periods as the input tracer signal needs to be weighed by the respective precipitation volume which per definition of precipitation free periods equals 0.

18) p.15881, l.16ff and table 3: The methods are not described concretely enough. The equations are fine, but how exactly was the stream concentration computed? Concentrations are measured weekly (and instantaneously in the stream but as a volume-weighted average in precipitation and soil(?)) but water fluxes are measured more frequently, so what was done to distribute concentrations over time? In addition, how was the gamma function integrated (since it goes to infinity at t=0 when alpha is less than one)?

17) p.15882, l.1: I would be glad if you could add the reference Hrachowitz et al. (2011) as an overview of different methods was provided therein.

18) p.15882, l.9ff: Methods again. It was stated that the model performance was evalu-

ated on basis of 10000 MC realizations within the GLUE framework. That is fine. Quite some important information is missing however. What were the prior parameter distributions for the models under consideration (ranges, uniform or informed,...)? GLUE requires the definition of some sort of likelihood measure to weight the solutions and to construct the uncertainty bounds (see e.g. Freer et al., 1996). Yet, no mention of that is made. What likelihood measure was used? Nash-Sutcliffe efficiency? Were the solutions not likelihood weighted at all (implying that ALL solutions retained as feasible, i.e. NSE > 0.45 (?), were assumed as equally likely)? Please specify, justify and reference this part in more detail.

19) sections 3 and 4: As mentioned above, I really like the general set-up of the study. The result that MTT in soils is by 1 order of magnitude below that of streams and springs is extremely interesting and in addition it lends considerable experimental support to the hypothesis put up by Hrachowitz et al. (2013), that different system components can exhibit substantially similar transport patterns, i.e. TTDs. However, apart from that the results section (but also the discussion) is too much centred on the models themselves. Bear in mind that models are to be seen as mere tools. Thus the tools themselves are discussed. However, it would be much more instructive if more emphasis was given to "what" the use of these different tools can actually tell us about how the different catchments and (more importantly) the different compartments of the system (soils, springs, streams) function. As a very first step I would thus recommend the authors to largely condense the results and discussions of which model performs best, in favour of showing what is the difference between them. In other words, it would be highly interesting to see the actual TTDs of say the best 2 or 3 models for each compartment. In how far are the shapes of the TTDs similar or dissimilar when comparing one compartment to the other. Do for example the TTDs in the soil show different general shapes than in the stream (e.g. delayed peaks as in EPM, DM or GM models with alpha >1)? No matter if the answer is yes or no, it would tell us something quite fundamental about the characteristics of transport and water routing processes in the different components. I would therefore be very glad if the authors would consider

adding such an aspect in the results and discussion by showing the shapes of different TTDs in streams, soils and springs and carefully interpret the shapes and discuss in respect with amongst others the results of Botter et al. (2011), Hrachowitz et al. (2013) and Stewart et al. (2010). Please also note that the results concerning soils are fundamentally different from springs and streams, as the soil data characterize the age of resident water stored in the catchment (often termed "residence time distribution") and spring and stream data give the age of water in fluxes (often termed: "transit time distribution). See Hrachowitz et al. (2013) and Botter et al. (2011) for detailed characterizations.

20) section 3: please provide complete a table with results including information such as the optimum model performances and the 5/95% of model performances of the retained models and the same for all parameter(s) for all sites and components

21) p.15888, l.1-4: maybe include Roa-Garcia and Weiler (2010) as reference here

22) p.15889, l.5 and elsewhere in the manuscript: this should not come as a surprise. Rule of thumb: more parameters = more uncertainty, simply by the additional degrees of freedom in a model, allowing for different parameter combinations giving the same results (equifinality)

23) Table 1: not sure that the SI units for "site code" is [m a.s.l.] and for "altitude" [weeks]. Just saying. . . ;-)

24) Table 3: symbols need to be defined somewhere in the manuscript

25) Table 4: it would be nice to also provide and discuss a figure with the transects and the respective MTT and/or TTD depth profiles therein.

26) Figure 1: please add a zoom-in to better show the transects

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port in catchments, Hydrol. Process., 10, 793-802, 1996.

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