Responses to comments of Referee #2 to the paper "Integrated response and transit time distributions of watersheds by combining hydrograph separation and long-term transit time modeling"

Comment:
The paper "Integrated response and transit time distributions of watersheds by combining hydrograph separation and long-term transit time modeling" presents an interesting and potentially very useful comparative analysis of RTD and TTD for three contrasting catchments. The combined approach applied at different time scales allows a better representation of the hydrological response, taking into account the non-steady state boundary conditions of the systems as compared to commonly used lumped convolution integral models, which in most cases assume time invariant system states.
The paper is generally well structured, though some paragraphs seem to give redundant information and could be condensed. The methods are well explained and sound. The results are presented in a clear way, however a bit more illustrative detail could help the reader to follow the story more easily.
On balance I think that this is a very good scientific paper which could be considered for publication however I would encourage the authors to address some minor comments which are detailed below.

1) The total absence of uncertainty estimation is rather surprising. As already highlighted in the original TRANSEP paper by Weiler et al. (2003), parameter identifiability can be a major concern when using convolution integral models or other conceptual models in hydrology. Although the results seem absolutely plausible, it would be good to include an estimate of parameter uncertainty (e.g. by Monte Carlo sampling). As for example the differences of the best fit MRT of the base flow module are quite low this will help to illustrate the significance of the results.

Response:
As already pointed out to the response of reviewer #1, uncertainty is certainly an important point, and we this would add some interesting points for discussion. But adding a complete parameter uncertainty part would in our opinion add a complete new story line to the paper. We wanted to focus in this paper on combining event based hydrograph separation with baseflow transit time modeling to derive a complete TTD. This has never been done before and should be the focus of this paper. If we would add uncertainty methods, we believe that the main points are being watered down. However, we have already started with a new study to implement uncertainty estimation to this approach and we hope to publish this in near future.

2) No details of the O-18 values and of precipitation or stream flow distributions are given. This might help the reader to better understand the hydrological system of the three catchments. I would therefore encourage the authors to include time series plots of the observed and modeled variables (O-18, precipitation, runoff) for the three catchments and highlight the base flow and event samples in these plots, especially for the events used in the subsequent analysis.

This point has also been risen by reviewer #1 and was answered in detail in the corresponding response. To summarize, we are happy to include 2 new figures showing details of the 18O of precipitation and streamflow for basefloe and events.

3) I am not convinced by the discussion of the gamma distribution (p.16, l.11-21). The authors explain that they compared a single, **time-invariant** gamma distribution to the combined model. This does not seem to be correct as they compare a static with a quasidynamic model. As a gamma distribution assumes steady-state boundary conditions, such as any other TTD in a common lumped convolution integral model, and therefore would show different shapes for different boundary conditions it should be rather compared to a single **time-invariant TPLR** distribution rather than to a time variant, or non-steady state, combined model, which takes into account changing system states. Or in other words, a time invariant gamma distribution (exactly as a time invariant TPLR distribution) could be an appropriate descriptor of the system, if the boundary conditions (i.e. wetness of the catchment, or averaged over long time periods) remain constant. If the boundary conditions change (i.e. wetness of the catchment changes when looking at individual events) then the gamma distribution (or any other TTD, such as TPLR) changes as well.

We completely agree that we compare a time-invariant distribution (gamma) with out quasi time-variant approach. As stressed by the reviewer, the time-invariant model can only work if the boundary conditions remain more or less constant. We assume this for our long-term baseflow model, knowing that this is not necessarily the case, and assumed a time variant approach for the short-term, event time scale TTD. The idea of comparing our approach to the often applied time-invariant Gamma model was only to highlight the potential of our approach, but also to show that our combined TTD are not necessarily completely different from the Gamma distribution. We will highlight the effects of time-variant and invariant models in the revised paper in more detail in the discussion.

4) p.2, l.14-16: should be rephrased as "higher", "longer" and "lower" distributions sounds awkward.

Agreed. Revised.

5) p.3, l.8: it might be worth including this reference:
Soulsby, C., Tetzlaff, D., and M. Hrachowitz (2009), Tracers and transit times: windows for viewing catchment scale storage?, *Hydrological Processes, 23*:3503-3507.

Agreed. Included.

6) p.6, l.14: Two weeks seem quite a long sampling interval. It may be worth mentioning the general climate in the region and acknowledging associated potential fractionation in the sample.

Fractionation was prevented through the use of the rain sampler proposed by the IAEA. The water is collected in a bottle that is stored below the soil surface and isolated to prevent temperature variations and that has no direct exchange with the atmosphere. We will provide a reference in the revised MS

7) p.6, l.17: not sure about the sample size n=332. This does not add up with the preceding explanation of three sampling sites which are sampled every two weeks for a year.

By mistake we included the samples taken at the wetland scale, which were not included in this paper. Correct N=74.

8) p.6, l.18-21: the definition of an event fits better in here instead of in the results section (p.12, l.3-4)

Agreed. Revised.

9) p.6, l.27: how was base flow defined here? Pre-event runoff? Or within which limits of pre-event runoff? Please specify.

It is defined as discharge in the stream previous to the beginning of the storm event with a pre-event water fraction of 100%. We have changed the word "baseflow" to avoid the confusion.

10) p.6, l.27ff: description of sampling is too detailed. Maybe some of this information could go into one of the tables.

Agreed. Revised.

11) p.8, l.8: maybe replace "designed" by "defined"?

Agreed. Revised.

12) p.8, l.20: "bi-weekly" not entirely clear. Is it 2 times per week or is it rather fortnightly, i.e. once every two weeks?

Once every two weeks. Revised.

13) p.8, l.28-29: which climate data were correlated? I suppose temperature and precipitation amounts, but please specify.

See response to reviewer #1.

14) p.12, l.22: maybe replace "larger" by "more" or something similar.

Agreed. Revised.

15) p.14, l.16: not sure what "cumulative linear" means.

The correct way of saying it would be just cumulative.

16) p.15, l.5-32: there is a lot of redundant information in this paragraph, Figs 4 & 5 (especially when showing both, PDFs and CDFs) as well as Tab.3. Could be shortened.

Agreed. We will merge Figures 4 and 5. And the section you mentioned has been changed to:

The comparison of RDT for the three catchments for different events, shown in the left column of Fig. 6, illustrates similarities between catchments B2 and BB in terms of the pdf at short times

(revolving around 0.1) in contrast to the pdf of B1 with values below around 0.01. These pdfs highlight the impact of forests (B1) on water holding capacity and the dampening of storm flows. For the larger times, a difference can be seen between B2 (the grass land catchment) and the other two catchments that show a higher frequency of longer response suggesting the influence of forests and wetlands as a long term water storage.
The comparison of the TTD for the individual catchments in the right column of Fig. 6 shows a similar picture as for the RDT, but with much less variability between events within one catchment. The variability of cumulative event water contribution among rainfall events is much smaller than the variability of runoff response and runoff coefficients among events.

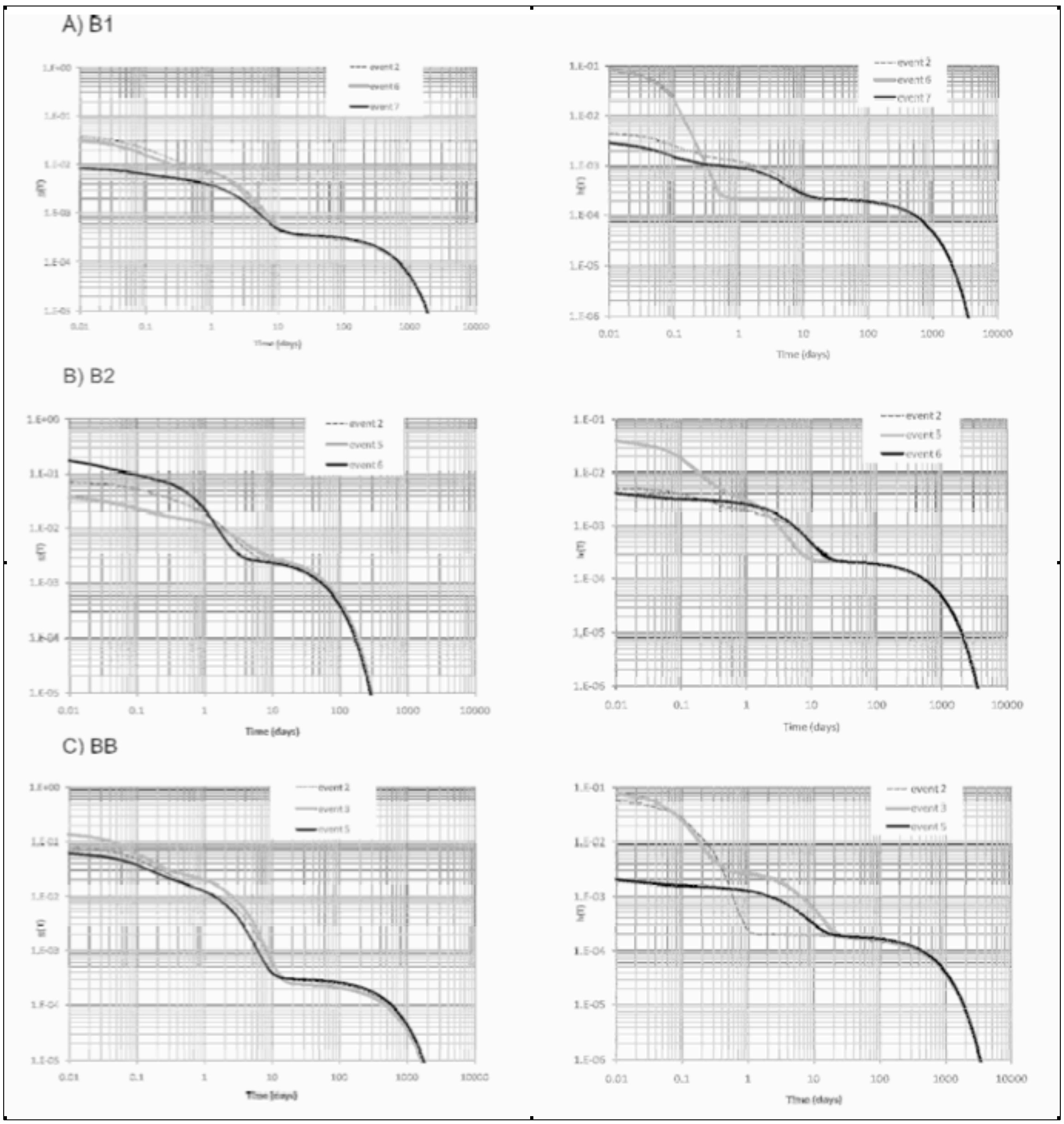

Figure 6. Response Time Distribution (RTD – g(T)) and Transit Time Distribution (TTD – h(T)) in the left and right column respectively. (Quality will be improved for the revised paper=

17) p.16, l.11: this reference might be more suitable:
Hrachowitz, M., Soulsby, C., Tetzlaff, D., Dawson, J.J.C., and I.A. Malcolm (2009b), Regionalization of transit time estimates in montane catchments by integrating landscape controls, *Water Resources Research, 45*, W05421, doi:10.1029/2008WR007496.

Agreed. Revised.

18) Although the importance of wetlands is perfectly plausible, I think it might be worth toning its significance down a bit throughout the paper, as it only covers 6 % of the catchment.

Agreed. Revised.

19) p.15, l.5, l.25: should "RDT" be "RTD"?

Yes. Revised.

20) Table 1: please provide a bit more information about the catchments e.g. elevation, slope, mean annual runoff, mean annual precipitation, mean annual evapotranspiration,…

Additional information requested has been added in the text describing the catchments, since Table 1 refers to land use in the catchments.

21) Figure 1: Please provide a bit more information, e.g. elevation or land cover and the location of the rain gauges

Additional information added to the text in the methods section.

22) Figure 4: please plot all 3 PDFs with the same Y-axis scale to make comparison easier.

Agreed. Revised.