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.

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.

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 quasi-dynamic 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.

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

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.

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.

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.

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)

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

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

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

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?

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

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

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

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.

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.

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.

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

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,…

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

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