Review of "Mean Transit Times in Headwater Catchments: Insights from the Otway Ranges, Australia" by Howcroft et al. (2017).

Summary and Recommendation

In this paper the authors estimate the mean transit times (MTT) of six headwater catchments in south-east Australia. Their goal is to gain a better understanding of the catchment's function to enable an improved water resources management in these areas. To estimate the MTTs the authors use tritium (^3H) activities, major ion geochemistry and discharge data in combination with lumped parameter models (LPM). The authors discuss the uncertainties of their approach partly conclusive, try to identify possible proxies for ^3H as well as shortly compare water quality variables with their estimated MTTs. Based on these results and the unusual long MTTs, the authors conclude that changes like droughts, deforestation and other forms of land use changes would not be realized within the streams for at least a decade.

Overall, the manuscript is well-organized. The introduction, methodology and discussion sections are well written, although sometimes I feel that restructuring some parts would improve the overall structure of the manuscript (see technical details). The results are well presented using adequate figures and tables. However, I have some concerns regarding the possible generalization and the overall connection of the results to the system functioning (see the major comments below). Nevertheless, I suggest that this paper could be accepted for publication in HESS, although I believe some major revisions are needed beforehand. I am looking forward to receiving the replies of the authors.

Major comments

1) *Generalizability*

Line 158: "*It is expected that the results of this investigation will facilitate greater understanding of headwater streams not only within the Otway Ranges but in similar catchments worldwide*."

Line 626: "*This study demonstrates a new methodology for estimating groundwater recharge based upon 3H activities in river water.*"

I encourage the authors to reconsider why a potential reader from a different part of the world should read your manuscript? If as you propose in Line 158 your approach will facilitate greater understanding of headwater streams worldwide you should discuss which of your results are general and which are more specific to your landscape. Furthermore, I would reformulate your primary and secondary objective with a stronger focus on generalizability.

If your goal is to develop a novel methodology as written in Line 626 you should make that clear at the beginning of your manuscript, state a clear hypothesis and explain what is new compared to other approaches. However, if you prefer keeping your primary and secondary objectives as they are, which is perfectly valid, you should consider moving this paper to the "cutting-edge case studies", a relative new type of publication form in HESS.

2) <u>*Model selection*</u>

Line 117: "*As a consequence, LPMs must typically be assigned based upon knowledge of the geometry of the flow system and/or information from previous time-series studies.*"

It is interesting that you develop a perception of how you think the catchments are functioning to justify the basic assumptions of your general approach (see major comment 4) and upon which you chose your LPMs. However, in Line 464 you write that it is not possible to assess the most suitable LPM in your study which means that all chosen models are equally likely, doesn't it? Though, just in the following lines you discuss which LPMs results are more or less realistic. To avoid confusion, I think you should clarify this in your manuscript and clearly state if you can constrain your model results or not.

<u>*3) System understanding*</u>

Overall, I found the discussion of your MTTs results a little short with respect to your system understanding. I recommend that you discuss in more detail, if and which of your calculated MTTs are realistic in your systems. For instance, are MTTs of 200 years and an annual groundwater recharge rate of 1 % in a headwater catchment of your geology realistic, if considering the hydraulic conductivity, the mean depth and average gradient of the groundwater bodies?

Furthermore, your calculated runoff coefficients vary from 8.6 % to 39 %. This is a pretty large spectrum, especially because the catchments are within the same climate and share a similar land-use (1.3 m of mean annual rainfall / forest cover 78 -95%). Do you have an explanation for this rather strong difference in the hydrological response? Are you seeing these clear differences in the hydrological behavior also in your MTTs and what conclusions can be drawn from this? Do the basic assumptions you need to make to apply your approach (no significant dilution of groundwater inflow; see discussion point 4) also apply in the two catchments with a runoff coefficient of around 40 %?

As you treat all catchments in a similar fashion, why do you think your MTTs are so different in your catchments? Is it a result of the uncertainty in your models or are the catchments functioning differently and if so, could you identify catchment attributes which might be the reason for this dissimilarity? For instance, the Porcupine creek and the Yahoo share a similar runoff coefficient of 11.4 and 10.5. On the 20/03/2015, you took ^{3}H samples in both catchments. If you calculate the specific discharge in both catchments, it shows that they are not too dissimilar with respect to their runoff generation at that given day. However, your MTTs differ in both catchments from a maximum of 80 years (DM 0.5) to a minimum of 2 years (EPM 3.0) and this is not the only day with such high differences.

Overall, given the large differences of your results, I encourage you to connect your research results much stronger with your system architecture and check if these results fit with the knowledge you have from these landscapes. Showing that two systems act differently can be relevant, however, identifying why they act differently is much more interesting for potential readers.

4) *The basic assumption of you approach.*

Line 428: "*The flow system may therefore be viewed as a continuum that is dominated by older groundwater inflows at low flows and progressively shallower and younger stores of water (such as soil water or perched groundwater) that are mobilised during wetter periods.*"

Line 452: "Whether this reflects changes to the flow system or is due to uncertainties in the MTTs (discussed below) is not certain."

From McGuire and McDonnell (2006) which you cite in your manuscript: "*Most methods are based on early adaptations from the chemical engineering and groundwater fields (e.g., Danckwerts, 1953; Eriksson, 1958; Maloszewski and Zuber, 1982; Haas et al., 1997; Levenspiel, 1999) and may not apply in catchments where there are complex and important controlling processes like variable flow in space and time, spatially variable transmissivity, coupled vertical and lateral flow, immobile zones, and preferential flow, to name a few.*

…

These simplifications include one-dimensional transport, time-invariant transit time distributions, uniform recharge, linear and steady-state input and output relations, and contribution from the entire catchment area (Turner and Barnes, 1998)."

First of all, I would like to highlight that I am not an expert in isotope or tracer hydrology. I apologize for the following comments in advance. Nevertheless, I believe that the following questions, which came across my mind while reading your manuscript, could help readers apart from the tracer community, to better understand your approach.

Similar as it is the case in different unit hydrograph applications, your approach assumes a time invariant and linear input-output relationship of your tracers passing your catchments. However, it has been proven that catchment responses of different kinds are highly non-linear and time variant in several studies over the last 40 years. With respect to runoff predictions, it is nowadays widely accepted that concepts like the unit hydrograph will lead to unrealistic predictions on longer time scales. If we now consider your coarse sampling (3-6 observations in each catchment), the seemingly arbitrary choice of your LPMs and the corresponding parameters as well as the time frames you are working on (up to 233 year/sampling period 1.5 years), it comes to me as no surprise that your model results are so different and highlight how speculative they are.

Furthermore, in Line 428 you propose that the flow paths in your system are state dependent. You argue that you couldn't identify significant dilution of groundwater inflow by recent rainfall at the sampling time. However, you miss a detailed explanation how you came up with this fundamental conclusion. I believe, you need to have a rather good understanding of your systems to exclude that flow paths are interacting and especially when your system is switching between the two proposed states (groundwater or soil water dominated). If you have this knowledge why do you not use it to constrain your model results?

If I made some wrong conclusions here about the necessary assumptions you need to make (linearity (superposition principle) and time invariance (your filter shouldn't be time-varying on the scale your are working), I again apologize for these comments. Nevertheless, I suggest a much more comprehensive discussion of the assumptions you need to make to apply your approach in your systems and why you think they are valid on a time scale of decades.

<u>Minor or technical comments:</u>

Line 27 "*The MTT of this ^{3}H activity is approximately ten years, which implies that changes within the catchments, including drought, deforestation, land use and/or bush fire, would not be realised within the streams for at least a decade.*"

Line 604 to 607: "*The reason for the unusually long MTTs is uncertain but could be related to very low aquifer recharge rates and/or high transpiration rates associated with eucalyptus forests (Allison et al., 1990). The long MTTs suggest that short-term events such as drought or bushfire may not impact the streams.*"

How can you exclude that the direct reaction of the stream flow to rainfall (rise of the hydrograph) is not influenced by the named land-use changes as you only analyzed your systems at times where they produced baseflow (following your definition). I would reformulate your statement and make clear what you mean with: "*The long MTTs suggest that short-term events such as drought or bushfire may not impact the streams."*

Line 431: I do not understand this sentence. Please rephrase.

Line 436: Are the catchments in New Zealand of which you chose one of the EPM ratios of 0.33 similar to the catchments you are working in? Have you chosen the EPM ratio of 3.0 as the minimum exponential flow (25 %) on basis of a catchment property or did you just randomly pick this value?

Line 443: July 2015 instead of 2014?

Line 442 until 449: Belongs to the method section?

Line 455 until 464: Again method section?

Line 561 until 585: I recommend to rework or remove this entire section. First of all, method, result and discussion parts are entirely mixed. Furthermore, the calculations seem to be widely speculative especially because your estimated MTTs are highly uncertain (see your subsection 6.3.1). I believe a potential reader understands that properly calculated MTTs can be used to estimate the groundwater recharge.

Section 6.6: Either you discuss this section in more detail with references to other studies and with a relation to the processes and potential hazards or you remove this section from your manuscript.