We thank both reviewers for their extensive and thoughtful comments on this paper. Our responses to these are below in **blue**.

Response to Reviewer #1

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.

We will revise the Objectives and Conclusions sections of the manuscript to more specifically address which of our conclusions are of relevance globally, and which apply specifically to the Otways region of Australia. Notwithstanding numerous studies over recent years, there is still not a complete understanding of the range of mean transit times (MTT) in headwater catchments nor what controls these. The realisation that MTTs in some Australian catchments are long (years to decades) is significant in their management, which is of local importance. That the MTTs are longer in these catchments than is perhaps commonly recorded elsewhere is important for understanding catchment behaviour more generally and we can emphasise this.

Demonstrating a new method to estimate groundwater recharge was not a specific goal of this study. Nonetheless, through examination of groundwater volumes across different times of the year, we realised that our data could be used to estimate groundwater recharge to the regional aquifer. We are keen to retain this section as it is novel and potentially of interest to researchers elsewhere. However, it is a relatively minor part of the paper and requires testing in other regions. We chose to include this topic in our manuscript to demonstrate a broader use of MTT estimates. Given that, we consider that this paper constitutes a regular research paper. Further work in this field may be suitable for a "cutting-edge case study" type of publication.

2. Model Selection

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.

It is true that using the approach where MTTs are estimated from individual ^{3}H activities that one has to assume an appropriate LPM. The potential advantage of using ^{3}H as a tracer in the southern hemisphere is that it may be used in a similar way to other radioisotope tracers (e.g. ^{14}C or ^{36}Cl), whereby an age or mean transit time estimate can be derived from individual measurements. In turn, this permits estimation of MTTs at a range of flow conditions. In the northern hemisphere, the use of ^{3}H as a tracer requires time series data collected over several years to estimate MTTs (due to the much larger bomb-pulse ^{3}H signal). Where samples are collected at similar flow conditions (e.g. summer low flows), this permits an independent assessment of the LPM via comparison of the measured and predicted ^{3}H activities. It is questionable, however, whether one could still apply the same time series approach in the southern hemisphere due to the diminution of the relic bomb-pulse ^{3}H activities. For example, the calculated decrease of ^{3}H activities for a water with a mean transit time of 10 years between 2016 and 2026, as predicted by the EPM and DM models used in the paper with the Melbourne ^{3}H record, is only 0.2 TU. Additionally, the time vs. ^{3}H trends produced by the LPMs in the southern hemisphere are similar within analytical uncertainty. Further, given the long MTTs in many southeast Australian catchments, it is not feasible to use other tracers (such as the stable isotopes) to better constrain the LPMs due to initial geochemical variations being attenuated when MTTs are more than a few years (e.g., Stewart et al., 2010, Hydrol. Process., 24, 1646-1659). We addressed these points on lines 96 to 118 of the paper and we will provide a few more details based on the above discussion to clarify our approach.

Our approach was to utilise different LPMs to bracket the estimates of MTTs. These LPMs have been used in many other studies, and where time-series ^{3}H data are available, they do reproduce the observed variation in ^{3}H activities. The LPMs are always a simplification; however, their geometries do agree with the likely form of the flow system and thus the approach is defensible. Estimating precise MTTs is difficult and the not knowing the best LPM to apply represents an uncertainty in these calculations (as we discuss in Section 6.3). However, the conclusions that the water in these streams has MTTs of several years to decades is independent of the LPM that is employed (as was emphasised throughout the paper).

As a general point, with the diminishing of the bomb pulse tritium signal, MTTs in the southern hemisphere are not overly sensitive to the models. Because of this, a change in the age distribution that occurs when different LPMs are used do not change the MTT dramatically. In the northern hemisphere, with significant bomb tritium still present, a change in the age distribution significantly changes the fraction of bomb-pulse tritium in the sample and therefore result in a different MTT. This was noted in the comparison of the MTTs from the different LPMs in section 6.2.

3. System Understanding

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?

The 200 year value is an absolute maximum and is subject to considerable uncertainty (as we discuss in section 6.3). However, as outlined in the response to other comments below, the conclusion that mean transit times are years to decades is robust. The long mean transit times do imply slow recharge rates. There are only sparse measurements of hydraulic conductivities in these aquifers and, consequently, it is difficult to corroborate the recharge rates using the aquifer properties. The recharge rates are consistent with those generally proposed for eucalyptus forest areas in SE Australia. For example, Allison & Hughes (1983. Journal of Hydrology 60, 157–173), Allison et al. (1990. Journal of Hydrology 119, 1-20), Herczeg et al. (2000. Marine and Freshwater Research 52, 41-52), and Cartwright et al. (2006, Journal of Hydrology 332, 69-92) estimate that recharge rates in areas dominated by native forest are at most a few mm per year and often less. These low recharge rates are due to the high transpiration rates in eucalypt dominated catchments. Further, because hydraulic conductivities are poorly known in most areas, it is important to find other means to estimate groundwater recharge. However, we recognise that there is potential for groundwater discharge from the catchment via deeper groundwater flow pathways. If this is the case, our estimates would underestimate the true recharge rate because the proposed method accounts only for the discharge at the stream gauge, not total discharge. We will discuss these uncertainties in more detail within the revised paper.

Additionally, the lack of significant near-river alluvial sediments may be a reason why the estimated MTTs are so long. The lack of near-river alluvial sediments precludes the possibility of significant bank storage and return flow contributing to total river discharge and, thus, probably influence the MTTs. We will also discuss this in the revised paper.

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 behaviour 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 %?

The calculation of the runoff coefficient in a region with well measured rainfall and long streamflow records is relatively straightforward. What is less clear are the reasons for the variation. The high runoff coefficients for Upper Lardners, Lardners Gauge and James Access may be because these three rivers drain steeper catchments and are underlain almost entirely by low hydraulic conductivity Otway Group basement rocks.

The runoff coefficients do correlate well with ^{3}H activities and the reason that we included them in this study is that they are useful in providing a first-order estimate of MTTs (in as much as they indicate whether the water is likely to be relatively young or old). The variation in the runoff coefficients is probably controlled by similar factors that control the variation in the MTTs. Catchments with low recharge rates may lose water to the atmosphere by evapotranspiration and consequently have both long MTTs and low runoff coefficients. However, as noted in Section 5.4, it is unclear whether and how catchment attributes such as slope, drainage density control the MTTs (and so by extension the runoff coefficients). A lack of a single catchment attribute controlling MTTs was also noted by Cartwright & Morgenstern (2015. Hydrol. Earth Syst. Sci., 20, 4757-4773) in the Ovens catchment of NE Victoria. In that catchment, there was also a good correlation between the ^{3}H activities and the runoff coefficient. We will explain the importance of the correlation in terms of providing first order estimates of MTTs in the revised manuscript.

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 3H 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.

The causes of the variations of the MTTs between the catchments remains an open question. In studies elsewhere, catchment attributes such as slope and drainage density were shown to correlate with MTTs. Given the multiple interacting processes that control the transmission of water through catchments (e.g., as discussed by McGuire and McDonnell, 2006; Hrachowitz et al., 2009; Stewart & Fahey, 2010), it is probably not surprising that no single catchment attribute controls mean transit times. Moreover, the lack of correlation confirms that multiple processes control water flux, and that these processes and their interaction are still poorly understood. Similar variations in MTTs between streams are also apparent in the Ovens Catchment (Cartwright & Morgenstern, 2015) and, in that case, the reasons are also not clear. While it is a negative outcome, it is worth us emphasising the lack of correlation between ^{3}H and the catchment attributes in the Discussion section, as it is important.

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.

We agree that we can better integrate the results. As we discuss in response to later comments, while the calculation of MTTs has uncertainties (many of which we discuss in Section 6.3), the observation that the ^{3}H activities are far below those of modern rainfall means that the MTTs must be several years to decades. The reasons for the variations in MTTs are not clear, but making that observation is also of general importance.

4. The basic assumptions of your 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).*"

We agree the LPM models are an approximation of real-world situations. Nevertheless, they are commonly used and have successfully predicted variation in tracer concentrations / activities in many catchments. It is generally not possible to constrain all the variations in hydraulic properties in a catchment and all modelling approaches contain some elements of generalisation.

The assumption regarding time-invariance is only correct where mean transit times are calculated from time series measurements (of ^3H or other tracers). Because ^3H is radioactive, it will yield a mean transit time regardless of whether the catchment is time invariant as long as the flow path geometry remains relatively constant. Further, there is no requirement that water from the entire catchment reaches the stream. The much-used exponential-piston flow model, for example, is applicable to catchments that have both confined and unconfined portions.

Regardless of the uncertainties in the LPM calculations, one can get a general idea of timescales from the ^3H activities. If a water with a ^3H activity of modern rainfall (~2.7 TU) were collected and isolated, it would take 30 to 40 years for the ^3H to decay to the lowest ^3H activities recorded in the streams (0.2 to 0.5 TU). Given that the ^3H activity of rainfall in the past 50 years was considerably higher, the timescales would be even longer. This is not a real calculation of water age or MTT; however, it highlights that ^3H is an important qualitative or semi-quantitative tracer over and above its use in the calculations (i.e. waters with low ^3H activities are relatively old).

Some of this discussion is in the current version of the paper and we can expand on these points as they are important and perhaps not clear to a broad readership.

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.

As noted above, the use of ^3H does not assume time-invariance. Also, because the ^3H activities in the streams are much lower than those of rainfall, the conclusions that the MTTs must be years to decades are robust. We have been clear throughout the paper that there are considerable uncertainties in the calculated MTTs and have sought to address these where possible. However, the data allows an understanding of the broad mean transit times between and within the catchments, which was the main objective.

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?

The conclusion comes from a variety of observations. Firstly, although we sampled throughout the year and at different flows, we avoided sampling immediately after heavy rainfall when new or event water may be important. Secondly, at the time of sampling, the major ion concentrations in the river do not suggest that there has been dilution between low salinity recent rainfall and older water from within the catchment. The observation that the ^{3}H activities appear to plateau at values that are less than those of rainfall also implies that, during our sampling, the rivers were not dominated by recent rainfall. Finally, during the sampling rounds, there was no overland flow observed in the catchments. Our conceptualisation is that the catchment contains several stores of water ranging from deeper groundwater to shallower soil water that progressively become more important as the catchment "wets up" during the winter months. The observation that the highest ^{3}H activities are similar to those recorded in soil / regolith water in this catchment is also consistent with that idea. Section 6.1 discusses this and we can add some of the above details to explain our reasons more fully.

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 you 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.

The comments were valuable as many papers are written from a point of assuming a high level of background knowledge. Without turning the paper into a review article, we will broaden the explanation of these issues.

5. Minor or technical comments

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.*"

That is what we meant to say and we will clarify this in the revised paper. The long MTTs mean that the base flows in the streams are buffered against short-term variations in rainfall (and indeed many of these streams continued to flow through the Millennium drought between 1996 and 2009) but that longer-term climate change will probably impact the catchments.

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

This sentence notes that if the system does contain more than a single store of water (e.g. old baseflow and young event water), then the calculated MTT gives the minimum age of the baseflow component. We will rephrase it.

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?

The catchments have a broadly similar geometry to those in New Zealand. The flow system here and in those examples comprises an unsaturated zone overlying the aquifers which is the basis for the choice of the EPM model (piston flow through the unsaturated zone followed by exponential flow through the aquifer). In recognition that we cannot constrain the suitable LPM, we utilised a range of values for the EPM ratio. An EPM ratio of 3 is a system with 75% piston flow. This may be too high in reality, but it does help limit the calculated range of MTTs.

Line 443: July 2015 instead of 2014?

The date should be July 2014; we will correct this.

Line 442 until 449: Belongs to the method section?

Respectfully, we disagree. Lines 442-449 discuss the MTT results, not how the MTTs were derived. Consequently, we will keep this in the discussion.

Line 455 until 464: Again method section?

As above, this paragraph does not discuss the methodology, but is a discussion of the MTT results. Again, we will keep in this 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.

As discussed earlier, this was an unintended but nevertheless important finding of this study. Estimating groundwater recharge is difficult and we have proposed a way to estimate it from the MTTs. Estimating groundwater recharge from groundwater MTTs is common; however, we are unaware of anyone attempting it from the MTTs of river water.

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.

It is acknowledged that this section is a minor component of the study. Nonetheless, the data suggest that anthropogenic impacts to several of the streams have occurred and, for this reason alone, is worth mentioning. On a more global scale, these data demonstrate the usefulness of using ^{3}H in water quality studies, much in the way that Morgenstern and Daughney (2012) used ^{3}H activities to assess baseline groundwater quality in New Zealand. We will add more detail in the revised paper.

Response to Reviewer #2

The paper estimates mean transit times in 6 headwater catchments in southeast Australia using two methods and radioactive ^3H tracers. The study is very interesting and provides with an initial overview that stable isotope tracers cannot provide. However, I think the discussion could be more thorough and the structure of the paper be reorganized. Following I write my suggestions to improve this paper and hopefully the authors take them in the best way possible.

We are grateful for the suggestions.

General comments:

1. My first comment is a general concern since it was not mentioned anywhere in the document. Are all 3H activities used on the study normalized? If it was mentioned I missed it.

The ^3H activities are absolute values measured against the NIST standard. This is described by Morgenstern and Taylor (2009), which we cite in section 4.2, but we can add this detail to the paper.

2. I mentioned that the results from this study are a good initial overview because the authors are ignoring the seasonal variation of tritium concentration in precipitation. In Varlam et al. (2016) and Tadros et al. (2014) is shown that seasonal variation is noticeable where autumn-winter precipitation has activities half or lower than spring season precipitation. From Tadros et al. (2014): "Within the annual cycle, a clear maximum is observed in early spring between August and September and extends into summer, with the minimum concentration occurring in March/April." The values ranging between 2.4 and 3.2TU are the annual average activities, but if the actual precipitation in March/April was at least half of the measured during those 78 days in July-September, the MTT would increase so much as it did. I understand that resources are not raining and analyzing samples for 3H are expensive, but this should be acknowledged as a flaw of the study and probably causing overestimation of MTT in March 2015.

It is true that ^3H activities in rainfall have seasonal variation. However, for waters with long mean transit times, this has little impact on the calculated MTTs unless recharge occurs dominantly during periods when rainfall either has high or low ^3H (see discussion in Morgenstern et al., 2010 doi: 10.5194/hess-14-2289-2010). The Otways have high rainfall distributed through the year and consequently there is not a distinct recharge season. The seasonal variation of ^3H activities in SE Australia is ~1 TU, which is similar to the range of ^3H activities that we calculated MTTs for (2.4 to 3.2 TU) and so any potential impact of seasonal recharge is likely to be a similar order of magnitude or less. It does not alter the overall conclusions that MTTs are years to decades. We will include some discussion pertaining to this issue in our revised manuscript.

3. The document lacks structure. Even when there are subtitles stating "Methodology", "Study Area", "Results" and "Discussion" there are results and methods in the discussion section, as well as study area information in the results section. I will point out in more detail in the specific comments.

We will ensure that the material in in the correct section. We address the reviewer's specific comments that relate to the structure of the paper below. However, in many cases, we consider that we have the material in the correct sections. Specifically, our results section presented the data and the discussion section interpreted it (which is why the MTTs, catchment attributes, and uncertainties appear here). This is a common, albeit not universal, way of organising papers and is our preference. Perhaps the editor can comment as to their preferred structure for HESS.

4. There are a lot of regressions were the only measurement for curve fit is the r2, using the p-value would also add information on the data that is correlated.

Agreed. P-values will be presented in the revised manuscript. While these are useful, they do not change the overall conclusions.

Specific comments:

1. P1 L18: 2.4 to 3.2 TU is the annual average value, not the real range of activities, big difference, which might explain partially the low values obtained on the stream water.

As discussed above, there is little evidence for a strong seasonal variation of recharge in this catchment. For catchments with long MTTs, the annual variation in the ^3H activities of rainfall has little impact where the MTTs are in excess of a few years. As also noted above, we will add a sentence or two to the discussion (Section 6.3) to explain this. The lowest ^3H values of <1 TU are much lower than any recorded in rainfall (either annual averages or seasonal measurements) and consequently, the water must be at least several years old.

2. Page 6 Line 148: "agricultural" I think the authors meant "agriculture" or rephrase.

Correct "agricultural" should be "agriculture". This will be corrected in the revised manuscript.

3. P7 L169: Is the forest cover in Table 1 only eucalyptus? If so, reference table 1.

No, the catchment percentages of forest cover presented in Table 1 include native eucalyptus as well as production forestry (much of which is eucalyptus). Here, we are providing a general description of the catchments but can add a few more words to clarify this.

4. P15 L390 to P16 L410: should be in "Study Area" section.

We disagree. Most of this material was derived as part of this study. While the WMIS website does provide estimates of catchment areas for most of the gauges, the values quoted here are from out GIS analyses. The other catchment attributes discussed here were calculated specifically for this project. The Study Area (section 3) summarises material from previous studies rather than results that arose from this study.

5. P16 L411-412: What about the correlation with geology? As well as a multiple regression or a PCA?

Noted. There is likely a correlation between runoff coefficient and geology (and/or possibly slope) for three of the catchments (Lardners Gauge, Upper Lardners, and James Access), as these catchments are relatively steep compared to the other catchments and are underlain almost entirely by the low-permeability Otway Group basement rocks. The variation in MTTs is probably controlled by multiple factors and while multiple regression analysis could be carried out, the small number of data points and the large number of potential controlling catchment attributes make it difficult to derive a unique solution (the same holds for other approaches such as PCA).

6. P16 L413-Fig7: This is a good example where the p-value could give more information, it's easy to see there are two extreme points with higher runoff coefficient that create that "correlation", but if those two would not be there the slope of the correlation would be negative instead of positive.

Agreed that including P-values would be useful. However, as noted above, the conclusions do not change.

7. P17 L433-P18 L475: This should be in Results, not Discussion (with few exceptions of a couple of sentences that were discussion).

In the paper, we have made the distinction between Results (which reports what is measured) and Discussion (the interpretation of the data). Thus, the ^{3}H measurements are included in the results and the MTTs are included as discussion as these are the interpretation of the ^{3}H data. This is our preferred discussion, although we acknowledge that there is no standard way of doing this (either in HESS or other journals). Perhaps the editor can best advise which structure is the best fit for HESS.

8. P18 L 476: The authors could make a section called uncertainties in the "Methodology" section with a description of each of them so there is no need to explain them in the "Discussion" section.

Noted. We included a brief description of uncertainties in MTT determination in the Introduction section (Line 108) as they are part of the background to understanding MTTs. This follows a similar format to other papers of ours and other authors. However, we can move this material to the Methodology section.

9. P19 Eq 2: this equation goes in the "Methodology" section, not discussion. Additionally, the "d" is missing in the equation, which is correctly mentioned in the text afterwards.

This would be better in the Methodology section. The "d" is subscript and this will be corrected in the revised manuscript.

10. P20 L515-520: As mentioned before, 2.45 TU is probably the high end of activity on the annual precipitation.

There appears to be some confusion here as the ^{3}H activity of 2.4 TU is on the **low**-end rather than the high-end of ^{3}H activities of rainfall for this area (line 239). We agree that there is uncertainty in the ^{3}H activity for modern rainfall. This is why we re-calculated MTTs using a range of ^{3}H activities (2.4 TU to 3.2 TU) which encompasses the range given by Tadros et al. (2014). As we noted in the response to the other reviewer, the assumed ^{3}H activities of modern rainfall make little difference to the calculated MTTs in waters with long MTTs such as these.

11. P20 L523: If 2.45 TU is on the high end, for the March calculations the precipitation should be more on the 1-1.3 TU (being conservative).

We are unsure where the ^{3}H activities of 1-1.3 values come from. The measured ^{3}H activity (2.45 TU) in the single precipitation sample that we collected is the lowest recorded ^{3}H activity in rainfall for any area in Victoria, Australia (that we know of). The measured average annual ^{3}H activities (both from our studies and the IAEA datasets that Tadros et al., 2014 quote) are in the range 2.4 to 3.2 (lines 340 to 345). We will ensure that the ^{3}H activities in rainfall are clearly explained in the revised manuscript.

12. P20 L530-531: Yes, unimportant for the surveys taken in September, partially for those in November and July, I don't think it was unimportant in March.

The March samples have the lowest ^{3}H activities, so the impact of the uncertainties in modern rainfall ^{3}H activities will have the lowest relative impact on the estimated MTTs. This sentence could usefully be expanded to explain the relative impacts more clearly.

13. P21 L539-L544: This should be in the Results section.

We disagree as this is part of the interpretation of the results. We will, however, reword this section so that it better conveys the point that we are interpreting data not presenting new data (e.g., “Given that the analytical uncertainty of the ^3H are… …the resultant uncertainties in MTTs are…). The uncertainties were presented in the Methods (Section 4.2) and in Table 3 and we will refer to those sources here.

14. P21 L547-549: I agree that the intermediate flow rates are important, maybe even the

This comment is incomplete, so we are not entirely sure of its meaning. It appears that you are agreeing with our conclusion that the greatest uncertainty in MTT estimates are for waters with intermediate ^3H activities. We believe that this is an important conclusion.

15. P21 Eq 3: This equation belongs to “Methodology”, not discussion.

Agreed, we will move it to the Methods section

16. P22 Eq 4 and 5: These should be in results.

Agreed, we will move them to the Methods section

17. P22 L569-582: This belongs to results.

This is a section that interprets the results, thus we consider that it is in the correct section

18. P22 L588-P23 L592: Discuss why there is no increase on the sulphate concentration in the Ten Mile Creek, are the anthropogenic activities different in this catchment than in the others?

We are unsure as to why there is no correlation between sulphate concentrations and discharge at Ten Mile Creek, when such correlations (including nitrate) do appear to exist at Upper Lardners and James Access. Sulphate concentrations are much higher at Ten Mile Creek than they are at Upper Lardners and James Access (Figure 6), which probably reflects the fact that Upper Lardners and the Gellibrand River at James Access are more pristine streams than Ten Mile Creek. Clearly, more data would help elucidate whether such correlations are real. We will touch upon this in greater detail within our revised text.

Reference:

Carmen Varlam, Octavian G. Duliu, Ionut Faurescu, Irina Vagner Denisa Faurescu (2016) Tritium time series in precipitation of Rm. Valcea, Romania, Isotopes in Environmental and Health Studies, 52:4-5, 363-369, DOI: 10.1080/10256016.2015.1114932

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-219, 2017.