

Response to comments on “Using geochemical tracers to distinguish groundwater and parafluvial inflows in rivers (the Avon Catchment, SE Australia)” by Cartwright and Hofmann.

We thank the two reviewers for their constructive comments on the paper and consider that we can use these constructively to improve the clarity of the paper. Our responses to the specific comments are outlined below (in **blue**, references are those in the original paper or comments except where indicated).

Reviewer #1

This reviewer is thanked for a very comprehensive and detailed review. Part of the substantive comments raised by the reviewer relate to the paper organisation. Overall the reviewer seems to be requesting that the paper is written in the format that they would have used. While the suggested format is logical, it should be noted that other papers in the literature (including in HESS) follow a variety of formats and the second reviewer was happy with the structure of the paper.

Some of the suggested reorganisation, specifically aggregating the equations into a separate section, is reasonably straightforward. Since we do not develop the equations, having them in a single place would probably make the introduction and discussion easier to follow as these sections are currently rather long.

We presented the parafluvial flow as a conclusion that arose from not otherwise being able to reconcile the Rn and streamflow data. The reason that we chose that approach is that it reflects the way that the study developed. The alternative is (as suggested by this reviewer) to start off from the standpoint that parafluvial flow is likely in in this environment, incorporate it into the Rn mass balance from the outset, and then to show that the Rn mass balance is less viable without it. This follows the logic that parafluvial flow is a known process but it has been little-studied in terms of its impact on the Rn mass balance, which is similar to the approach used in other studies (e.g., the discussion of hyporheic flow by Cook et al., 2006). We can certainly recast the paper in this format if it better explains the importance of parafluvial flow, in which case Fig. 10 would become the primary figure that presented the initial results with parafluvial flow and a modified version of Fig. 6 could be used to illustrate the situation with no parafluvial flow.

The paper has 2 objectives, of which the second is arguably the most novel, but the least well treated within the manuscript. Perhaps part of the issue is that the objectives are loosely related rather than following linearly one from the other.

The second objective (that major flooding events which alter the geometry of the floodplain result in changing locations of groundwater inflows) was part of the original motivation of the study.

However, the question of the extent of parafluvial flow is also important and probably more generally relevant. Most groundwater-surface water studies that have utilized Rn have not explicitly dealt with the impact of parafluvial flow on the Rn activities in the river. To our knowledge only the study by Bourke et al. (2014) has attempted to separate smaller scale hyporheic from larger-scale parafluvial flow in these types of studies,

although others (such as Cartwright et al., 2014) essentially combined the effects parafluvial and hyporheic flow. Parafluvial flow is likely to be important in rivers with coarse-grained alluvial sediments such as the Avon and thus is a process that must be taken into account when utilising Rn.

The paper is probably the first to use Rn to understand the changes to groundwater surface-water interaction resulting from changes to the streambed and provides us with a methodology to understand those changes. Additionally, it will be one of only a few to directly address the impacts of parafluvial flow on the Rn mass balance and that too is important. Finally, comparatively few studies have attempted to carry out Rn mass balances at baseflow conditions when the water in the stream will largely be provided by groundwater inflows. While this seems somewhat redundant as the groundwater contribution can be measured by differential gauging, it turns out to be important assessing whether Rn provides a reasonable estimate of groundwater inflows or whether there are problems in the (many) assumptions in the Rn mass balance. Where studies are carried out only at higher flows where the water in the stream comprises groundwater and surface runoff, Rn may imply groundwater inflows that seem reasonable but which are difficult to test. While we have made this point before (Cartwright et al., 2014), it is certainly worth emphasising in this paper as it improves the application of Rn to understanding groundwater-surface water interaction.

Significant parafluvial fluxes have previously been found in other streams with coarse sediments (eg. Holmes et al 1994, Goosef et al 2003, Bourke et al 2014). Further clarification around the novelty of this work should be provided.

We referenced the Bourke et al. (2014) study which in terms of reported parafluvial flows is probably the most similar to the Avon (in terms of stream characteristics and the implied scale of the parafluvial flow), albeit in a losing stream. That study also used Rn as a tracer and so the approach and results are comparable and address the point made above that this is a process that we need to consider in applying Rn in these types of environments. The Dry Valleys study of Goosef et al. (2003) envisages hyporheic / parafluvial exchange on a smaller scale (e.g. their Fig. 3 shows it to extend a few 10's of cm to possibly a metre or so from the stream edge). The scale of parafluvial flow envisaged by Holmes et al. (1994) is more comparable to that in the Avon and we thank the reviewer for bringing it to our attention. We again emphasise the point that the paper was not trying to prove that parafluvial flows occur, but that it needs to be accounted for in utilising Rn (the former is well established, but the latter is not).

Is this perhaps the first estimate of the influence of parafluvial fluxes on radon mass balance in gaining stream (or alternating gaining/losing)?

To our knowledge that is the case and actually it is one of only a few studies to explicitly discuss the impacts of parafluvial flow on Rn activities. Rivers with broad coarse-grained alluvial floodplains which contain features such as point bars and pool and riffle section are relatively common at mountain fronts and it is in these that parafluvial flow is likely to be most important. However, the use of Rn mass balance is probably influenced by the work on lowland rivers with finer-grained bank and bed sediments and incised water courses where parafluvial flow may be more limited. A parallel may be drawn here with hyporheic flow; while hyporheic exchange had been documented for many years, its

impact on Rn activities was largely ignored until some studies explicitly addressed it (e.g., Cook et al., 2006).

The group of comments addressed above deal with the reasons for the study and how it was framed. We thought that we had addressed these objectives in the paper, but we can be more explicit at the outset by emphasising them in the aims and more clearly focussing on them throughout the discussion.

The inference of spatial variation in groundwater inflows over time is an interesting application of this method (longitudinal radon mass balance), but it is unclear if this approach is valid using data measured under different flow regimes, some of which were non-baseflow conditions.

The conclusion that the spatial pattern of groundwater inflows has changed is robust. High Rn activities in rivers almost invariably correlates to zones of high groundwater inflows (see Cook et al., 2013 and references therein). While we agree that estimating the groundwater inflows at different flow conditions is more difficult (as we discuss in the paper), understanding where groundwater inflows occur is simpler. Additionally the February 2009 and February 2015 sampling rounds were both at baseflow conditions, and these occurred before and after the floods which rearranged the floodplain. This was noted but we will make the latter point more clearly in the revised paper.

Further support for the validity of the steady state assumption implicit in the method should be provided.

The reviewer is correct that the steady-state assumption is implicit in these calculations. This is mentioned in Cook (2013), although it is very rarely discussed in Rn studies. In terms of the Avon study, the flows did not change significantly during the sampling rounds (i.e. we did not sample during times of rapidly increasing or decreasing river flow) which implies that the assumption of steady state is reasonable. For reference, the variation in the flows at Stratford during day of the sampling and the couple of days either side (which would account for the time taken for water to transit the river) the rounds are <5%. We will add these details to the revised paper.

Major comments:

It is more common to simultaneously fit the water, radon and solute mass balances, rather than fitting them individually as was done in this paper. Simultaneous fitting of multiple tracers reduces the uncertainty in the groundwater inflow estimate (McCallum et al. 2012). The approach taken in this manuscript should be justified, and possibly reconsidered.

There are several ways in which the water mass balance has been addressed in the literature. Mullinger et al. (2007, 2009) and Cartwright et al. (2011, 2014) amongst others calculated groundwater inflows from the Rn data using Eq. (1) rearranged to make I the subject. This approach yields the same results as forward modelling if it is assumed that the inflows are uniform in the reach. Frei & Gilfedder (2015, Water Resources Research, 51, 6776-6786) use a PEST approach based on the radon data alone (the Finiflux program). While it is correct that multiple tracers can be used simultaneously, in this case the errors that arise from the use of major ions such as Cl are large due to the variability of Cl concentrations in the groundwater and the relatively small difference in Cl concentrations between the groundwater and river water. We discuss that the Cl

concentrations only broadly constrain groundwater inflows in the text. There are only three points where the streamflow is measured on the Avon, and while that provides an indication of the overall groundwater inflows, it cannot be used to constrain the reach-by-reach inflows. The use of streamflow in the calculations also implicitly assumes that gaining reaches do not contain any smaller losing sections, which (as discussed below) is not likely to be the case in the Avon and possibly elsewhere. From a pragmatic viewpoint fitting the fluxes to the Rn data alone is readily achievable in Excel and allows the effect of varying parameters in the Rn mass balance (such as k) to be readily assessed. The methodology is similar to that of Cook et al. (2006) or Cartwright et al. (2014) where Rn (& SF₆) was used to calculate the groundwater inflows and then these were used to construct predicted trends in EC or Cl. We will explain this more in the revised paper.

The first of the two objectives is to test the hypothesis that “large scale parafluvial flow is an important contributor of ²²²Rn to the river”. It was then somewhat surprising that the authors didn’t introduce a parafluvial flow component to their analysis until section 5.4 of the discussion. Given that this is stated as one of the main objectives, I suggest that the simulation of stream radon concentrations should be presented as a function of varying amounts of parafluvial flux. This would allow the author to demonstrate that a fit with zero parafluvial flux is not plausible while keeping the focus on the stated objectives.

The structure of the paper reflected the development of our understanding of the system. Our realisation that parafluvial flow was an important contributor to the Rn mass balance came when we examined the data and found it difficult (impossible) to reconcile the Rn data without inclusion of the parafluvial flows. Thus, we envisaged the parafluvial flow as a conclusion to the study and wrote it in that format (i.e., the paper builds to that point). The alternative as suggested by the reviewer that we would expect parafluvial flow and this need to understand its impact on the Rn mass balance is also valid. As discussed above we would be happy to reorganise the paper in that way if it results in a clearer explanation of the importance of parafluvial flow.

Further consideration should also be given to the effects of “losing” reaches on the water balance. The study river is said to contain “alternating gaining and losing reaches”. Could not accounting for water loss along losing reaches result in the discrepancy observed between simulated and measured streamflow? The authors acknowledge this on p9208L4, but do not appear to discuss it further. The influence of these losing sections on the water, chloride and radon balances should be quantified and discussed to fully justify the estimate of parafluvial flow.

It is true that a river with a large number of net-losing reaches might account for the discrepancies between the calculated and observed streamflows. However, in the case of the Avon, it is unlikely to be the only explanation. Between Wombat Flat and Stratford (the first two gauges), only the reaches around 25 to 30 km are net losing (ie it is not the majority of reaches in the stream). To account for the discrepancies in flows, the losses in the reaches at 25 to 30 km would have to be a significant portion of the groundwater inflows in the preceding reaches. There is no indication from field observations that this is the case. While we did not measure streamflows, a reduction in streamflow of 50% or more over such a short distance would be readily observable. Additionally, there are no indications from reports from state agencies or anecdotal evidence from local landowners that these or other reaches in the river dry up even during prolonged drought, and all

reaches of the river were flowing during the 2009 sampling campaign (which had the lowest flows).

Our interpretation that parafluvial flows are important is consistent with the nature of the Avon River (coarse-grained alluvials and numerous gravel banks and point bars on the floodplains). There also pool and riffle sections, and many of the riffles have steep longitudinal gradients that are likely to result in river water outflows at their upper section and inflows in the lower sections. Thus it is likely that the net gaining reaches have some sections which lose water that then reinfilters the river.

It is a common but unstated assumption in papers using geochemistry that individual reaches designated as gaining are gaining throughout. This is apparent in the numerous Rn papers where groundwater inflows have been calculated – the net increase in streamflow is calculated from the values of I in Eq. (1) and the reach lengths. If streamflow is to be used in the fitting of data (comment above), this is a necessary assumption. In reality many streams may contain reaches that are dominantly gaining but locally losing, so all calculated inflows must be maxima.

The distinction between parafluvial flow and a river that loses water into underlying aquifer systems is a matter of detail. Both scenarios involve water loss from portions of the stream which then flows through the underlying and adjacent sediments before returning to the river. The impact on Rn will be the same (i.e. Rn activities will increase along the flowpaths between the outflow and the inflow points). Given that the coarse-grained sediments on the floodplain are several metres thick and the scale of parafluvial flow is likely on the tens to hundreds of metre scale (by analogy with other studies), we think that it is more likely that much of the stream water interacts with the alluvial sediments rather than penetrating into the upper section of the regional aquifers.

In our treatment of parafluvial flow, we have assigned a portion of the inflows as being these returning waters which reduces the discrepancy in streamflows and the calculations in Fig. 10 include the impacts on Rn and Cl.

We can better explain these aspects in the paper as our current discussion may not capture all of these points. In particular the point that calculated increases in streamflow must be maximum estimates as it is difficult to account for small losing sections in an otherwise gaining reach is one that has general applicability and it would be well worth mentioning. What would be useful in this explanation is to add a schematic figure to illustrate how we conceptualise the parafluvial flow.

The treatment of chloride in the parafluvial zone requires further justification. It appears that the Cl⁻ in the parafluvial zone is assumed to remain constant at the concentration from the river at the point of exfiltration. However, given that EC readings at distances of 1-2m from the river were consistent with groundwater concentrations (section 4.5), it seems likely that after mixing with this water, the Cl⁻ concentration in parafluvial water may be more similar to groundwater than the river.

Our interpretation of the Cl data is that it represents a mixture of water that is derived from the river mixed with regional groundwater. Given that the Avon is a gaining system, mixing of water from these two sources in the gravels is likely.

The second of the two main objectives is to test the hypothesis that “major flooding events which alter the geometry of the floodplain result in changing locations of groundwater inflows”. However, in reading the remainder of the manuscript, this point does not stand out as a major part of the paper. This is an interesting point, arguably the most novel idea in the paper, and should be further addressed throughout the results, discussion and conclusion. Satellite imagery or mapping of the geomorphic changes along the river channel may be helpful. A major question that arises is what are the hydrogeological conditions that have allowed for this change in the location of groundwater discharge zones. Are there particularly lithologies that are more susceptible to erosion and movement?

As noted above, we considered that this was an important point of the paper but were more focussed on discussing parafluvial flows as the impacts of these on the Rn mass balance has hitherto been little considered. However, we can certainly highlight it further. As to the specific points

- **There is insufficient detailed imagery to show the changes to the floodplain which occur on the tens to hundreds of metre scale.**
- **The floodplain sediments are unconsolidated and there is little vegetation on the floodplain that can stabilise the point bars and gravels. During large floods, the gravels migrate along the river which alters the geometry and position of the floodplain landforms.**

One apparent shortcoming of the work is that the authors compare groundwater inflows at multiple times with differing streamflows to address this objective. However, a conclusion of both this work and previous studies seems to be that the method works poorly except at low-flow (baseflow), which appears to undermine this approach.

This is not what we meant to imply. Our point (which we also discussed above) was that undertaking studies at baseflow conditions allows a degree of verification of the parameters (as the net groundwater inflows should match the measured increase in streamflow, given that groundwater is likely to represent the only / main source of water at those times). This is valuable as it allows checking of the parameters in the Rn mass balance. At higher flows, as long as the calculated groundwater inflows are less than the measured increase in streamflow, the results are plausible but there are less cross-checks. While that will always be the case, demonstrating that the adopted parameters produce acceptable results under baseflow conditions gives some confidence to the calculations at higher flows. We will clarify this in the revised version.

Was the river at steady state during the non baseflow sampling campaigns as required by the method (Cook 2013)? Further discussion and justification of this approach for estimating groundwater inflows under non-baseflow conditions is required.

Yes it was and we will quote the variation in flows around the sampling times (<5% variation at Stratford). This is an important point but one that is hardly ever discussed in geochemistry papers (either by our group or others) and it would be well worth noting.

The introduction is quite long and would benefit from significant editing. The authors may consider implementing a theory section that contains the theoretical background and all equations, separate to the introduction. This would allow for the scope and objectives of the paper to be more clearly presented to the reader in the first instance and remove the need for sub-headings within the introduction.

Throughout the manuscript it seems that information is not presented in the appropriate section. Results are presented in discussion, equations in discussion, and methods in results and discussion. These will be outlined in more detail in minor comments.

Some of this is a question of preference and papers on this topic in general have ordered the material in a variety of ways. The papers by our group and also by others (e.g. Mullinger et al., 2007, 2009) have introduced equations in the sections where they were utilised with the main equations in the introduction. It is also not uncommon to have equations presented in the methodology. Other papers (e.g., Cook 2006) have a Theory section following the Introduction. Given that we do not develop new equations here, there is no reason not to group them together and this would shorten the Introduction and Discussion sections, which are already long.

Minor comments:

9) Consider changing units to Bq/L instead of Bq/m³ as this removes the need for large concentration values (10000 becomes 10).

Using Bq/m³ is logical from the point of view of the dimensions of the terms in Eq. 1. The terms in Eq. 1 have units of Bq/m/day which for the hyporheic or parafluvial flux is relatively easy to envisage. Using Bq/L, these terms become Bq/L.m²/day which is not as elegant or as easy to envisage (although the calculations are the same). Since m³ is an SI unit, we would propose to keep it as is.

10) Consider changing the title to something more specific - as written it is quite general and doesn't suggest anything novel.

We consider that the title reflects the study but will modify it to include reference to the changing loci of groundwater inflows and replace geochemistry with Rn.

11) The authors may wish to reconsider abbreviations such as ~for approximately, and i.e., e.g. or c.f. within references.

Changing ~ to c. is relatively straightforward. We are not clear on the problems with i.e. etc but will follow HESS house style.

12) P9207 L11, more references required for methods to assess gw inflow to rivers.

13) P9207 L114. Specify baseflow separation rather than "numerical techniques"

14) P9207 L114. Are the authors referring to the type of water balance models used in this paper? Clarification required.

We did not want to turn this part of the introduction into a review. Considering the length of the paper, we propose to omit these sections (P9207 L10-22) as they are too brief to add much of importance and they detract from the discussion of geochemical tracers (P9207 L23 et seq.)

15) P9209 L20 Other methods of estimating k should also be mentioned; k can be directly measured using an artificial tracer release while the authors use an observed decrease to estimate k .

We do discuss other methods of estimating k later in the paper (the empirical equations that relate k to velocity and width) and compare them with the approach that we used to estimate k (which is similar to that of Mullinger 2007, 2009 and some of our other studies such as Cartwright et al., 2011, 2014). The degassing coefficient is a difficult parameter to constrain with confidence and we have done far more to understand k than in many other Rn papers, which is appropriate given its importance. k can be measured directly using artificial tracers; however, this is sometimes only attempted at a specific flow condition or in a small portion of the river, and that leads to questions about how representative the values are. We will add a comment regarding the issues around k and note there are other methods. In the revised paper, if the equations are grouped into a single section then that discussion would logically belong there.

16) P9210 L7 Use of exfiltrate and infiltrate somewhat confusing, given that “infiltration” is commonly used to refer to water percolating into the subsurface.

We agree that the terminology is not totally clear. We will use “water outflows” and “water inflows” to remove the ambiguities.

17) P9210 L21 suggest: increases with increasing residence time until secular equilibrium is reached.

Will reword as suggested (makes it clearer).

18) P9211, 9212. The difference between numerical approaches for hyporheic zone and parafluvial zone is fundamentally because in the hyporheic zone it is reasonable to assume it is well mixed with one concentration, whereas in the parafluvial zone, with longer flow paths, this assumption may not be valid. This should be clarified.

We can add those details, although in many ways it is a matter of scale. The hyporheic zone probably has gradients in Rn activities, especially if there is mixing between high Rn groundwater at the base of the zone or variable length flow paths; however those would be difficult to resolve these during sampling whereas conceivably sampling within the parafluvial zone would be able to resolve the differences in Rn along the flow paths.

19) P9213. Section 2 contains information other than local geology and hydrogeology. Consider renaming as Site Description.

We will change this to something more suitable (e.g., Study Area or Site Description).

20) P9214 L1 Clarify that streamflow was measured at fixed gauging stations, rather than using velocity meter.

We will add these details. Using a velocity meter to estimate streamflow is feasible in some circumstances but in wide shallow rivers with irregular beds (such as the Avon) it would be difficult to get reliable results.

21) P9214 Some discussion of whether the characteristics of the site described in section 2 make it a unique study site, or one that is representative of a large number of river catchments would be helpful.

This is a good suggestion. The Avon is similar to many streams that occur at mountain fronts both in Australia (e.g. many of the streams draining the Australian Alps) and elsewhere (e.g., New Zealand) and so the results of this study will be generally applicable. We will mention this in the introduction and echo it in the conclusions.

22) P9215 L1-11 Not required, consider deleting.

We can shorten this section, but some of these details are needed. The comments regarding the paucity of monitoring bores is required to address comments by Reviewer #2 (below) and the prohibition of river water use is made use of in the discussion where it is noted that sampling occurred during periods when there was little water abstraction from the river.

23) P9215 L12-17 Consider moving to introduction.

We agree that it would sit better in the introduction where the reasons for carrying out the study are explained.

24) P9216 L22 Reference?

The precisions are ones that we have determined in-house by repeated measurement over a short period (a couple of days) of water samples with a range of Rn activities on our RAD-7 meters. We will add those details to the paper.

25) P9217 Eqn7: Suggest presenting all equations in one section.

As discussed above given that we do not develop the equations as part of the study, we could easily do this. It would also help facilitate the discussion of estimating k (comment 15).

26) P9217 Streamflow results description is confusing, suggest a table. These data are important context for the comparison of data that the paper purports to undertake and subsequent conclusions.

We agree that this paragraph is very dense and difficult to wade through and that a table would present the data better.

27) P9218 Chloride concentrations are reported for the river and groundwater but the alluvium, while EC is reported for the groundwater and alluvium but not the river. At least one of either EC or chloride should be reported for all three end-members.

We can readily report EC for all the end members. We do have some Cl data for the alluvial waters and given the good correlation ($r^2 \sim 0.97$) between EC and Cl in the waters as a whole, we can also report Cl in the alluvial waters; although for some of these, it would obviously be a calculated value.

28) P9219-20 Suggest swapping order of S4.4 and S4.5

We agree that would make more sense as section 4.5 describes water geochemistry that is more akin to the data in sections 4.3 and 4.2.

29) P9220 L18 Chloride increase could also relate to evaporation along river.

Evaporation would probably occur relatively uniformly along the river, which would produce a steady increase in Cl even in reaches that were losing, whereas the observed pattern has discrete zones of increasing Cl that correlate with the zones of high Rn. This makes it more likely that the vast majority of the Cl increase is due to groundwater inflows. The reaches interpreted as losing have little or no increase in Cl concentrations, which would not be the case if significant evaporation had occurred. At the evaporation rate of $5 \times 10^{-3} \text{ m}^{-1}$ that we quote in the study, the increase in Cl concentration over a 10 km reach due to evaporation (calculated by rearranging Eq (1) and using the measured discharge values and widths) is $<0.1 \text{ mg/L}$.

We also have stable isotope data (not reported) and most of the river samples lie close to the local meteoric water line rather than defining distinct evaporation trends. Since a relatively modest degree of open-surface evaporation produces a displacement in stable isotope ratios (10% evaporation $\sim 1\%$ change in $\delta^2\text{H}$), this also points to relatively minor evaporation.

We can readily discuss the first points, but are reluctant to add the stable isotope data as the paper is already quite long and the data does not inform about other processes.

30) P9220 L23 Specify river distance that you're referring to here.

We can add the distances to the text here and elsewhere. The names make for easier reading but are probably not as informative.

31) P9221 L19-28, P9222 Suggest moving to methods.

See comments above regarding aggregation of equations.

32) P9222 L7 Chloride concentrations along a losing reach will still increase due to evaporation

That is true and the observation that there are regions where the Cl does not increase is consistent with the points made above that evaporation is not that important in increasing the Cl concentrations.

33) P9222 L14 Specifically, mixing is the only mechanism that will increase the EC of water in the hyporheic or parafluvial zones

As was pointed out to us in a review of a previous paper (Cartwright et al., 2014), evapotranspiration can occur from river gravels. However, on the timescales that are involved in parafluvial flow or hyporheic exchange, ET is a minor process and mixing is the main player. We will clarify this sentence.

34) P9222 L22 What is the variance on this mean? And therefore the associated uncertainty?

This was discussed in Section 4.4. The standard error is ~180 Bq/m³/day (or ~8%). In section 5.3, we did not propagate this error as even using the 95% confidence interval of ~16%, the impact of this error is small compared with the assumptions around estimating the dimensions of the hyporheic zone (which in most systems is only broadly constrained). We will note this in the revised version.

35) P9222 L24 I think you mean hyporheic here, not parafluvial

Yes, should be hyporheic.

36) P9223 Estimates quantities of groundwater inflow should be reported in the results section.

We disagree with this comment as this is an interpretation of the data and as such belongs in the discussion.

37) P9224 Heading 5.3 Not sure what you mean by variability here?

It is clearer if we just call this “uncertainties and sensitivity”

38) P9225 L15 The gas transfer term also includes w and d , is it possible that your k is underestimated but these other parameters are underestimated?

It is correct that there is a combination of terms in the gas transfer term. The approach that we used, which was to match the observed decrease in R_n in the losing reaches, estimates the whole $kdwc$ term and then k is derived from the d , w , and c estimates / measurements. If we assign different values to w or d , then our k value will be different but the $kdwc$ term remains the same (which is what is important for the R_n mass balance). We will clarify this in the text.

39) P9225 Consider moving eqns 8 and 9 to theory or methods sections.

See comments above.

40) P9226, Fig 8&9. Adjusting these parameters individually does not account for the fact that there are multiple parameters in a given term, ie gas transfer contains both k and w .

That is true, although as discussed above what we estimated in this case was the net $kdwc$ term. What we tried to show in this section is that there are limits to how the parameters can be varied independently. So in a losing reach, there will be combinations of F_h and k (or strictly $kwdc$) that will produce the observed R_n profiles. This is important as parameterisation of Eq (1) is difficult, but showing that there is not freedom to change all the parameters independently of each other helps with reducing the overall uncertainty on the calculations. This is a little-reported point in R_n papers and we will clarify this in the revised version.

41) P9229 L11 What are the difficulties? Which of these were known prior to this study and which are new based on this study?

This was not a very specific or informative start of the conclusions. What we were trying to convey was that the Rn mass balance could not be achieved without considering parafluvial flow.

42) P9229 L26, P230 L6. Suggest this belongs in introduction.

We agree that as a statement it reads like introductory material and a similar statement appears at the end of section 1.2. We can remove it from this section.

43) Fig 1b What is C_r in this calculation?

C_r is the same as C_{in} (i.e. 1000 Bq/m^3). This is noted in the text and we can add it to the figure caption for clarity.

44) Fig2 What are the arrows on the map?

They are the generalised directions of groundwater flow. We will add this to the legend.

45) Fig 4 Suggest adding streamflow

We can add this as a small third panel or as a dataset with a second Y-axis on the lower panel.

46) Fig 5 Suggest adding distributions of Rn and EC in river and groundwater to demonstrate presence/absence of distinct end-members.

Agreed that this would be useful and we can add the endmembers.

47) Fig 8 Suggest this fig not required.

If the paper is reformatted then this Figure will probably disappear.

48) Fig 10 Radon fit identical to Fig 6, consider that panel may not be required.

Since the Rn fit is that produced by this specific calculation it is better to retain it.

49) Ensure font sizes are adequate and consistent across all tables and figs

We will check the figures

Reviewer #2

We also thank this reviewer for their helpful comments and provide the following responses.

The experimental design focuses mainly on the use of radon-222 as a hydrogeologic tracer of groundwater and/or parafluvial inflows. The main critical point of the applied approach is the definition/measurement of an average groundwater value for radon and major ions, especially chloride. In the study, the authors have measured radon specific activities and major ions concentrations in 8 boreholes, finding big discrepancies among values likely due to the sampling in the riverbank. It would have been very important for such kind of studies to enlarge

the sampling network to boreholes surely unaffected by river water and also to reconstruct the morphology of the water table to identify gaining river reaches.

We chose to install bores close to the river to sample the groundwater that directly interacts with the river. Utilising regional bores for Rn data is commonly done; however, since Rn activities in groundwater are a function of the mineralogy of the aquifer it is always questionable how representative the Rn activities are when measured in a bore several km distant from the river. Only the closest bore at Pierces Lane is actively exchanging with the stream. The geochemical variation in the other bores both temporally and spatially is similar to that observed elsewhere in bores further from the stream (e.g., Yu et al., 2013; Atkinson et al., 2015). Given the scale of the catchment it would be implausible to install a whole network of bores to map the water table (desirable though that would be).

The abstract should include also a brief description of the applied methodological approach.

We will add a sentence on methodology to the abstract.

The description of the methodology is clear and thorough and well evidences the critical points. The discussion of the shifting inflow reaches (paragraph 5.1) has to be improved and reorganized, since the reader has poor and fragmented information about that.

As with our responses above, we agree that the changes to the loci of groundwater inflows over time should be better emphasised as it is a key result of the study.

Specific Comments (on the uploaded supplement)

Page 9212 L16. Veracity vs. Accuracy

Plausibility is probably a better term

Page 9212 L20. Hypothesis vs. Hypotheses

Agreed, should be hypotheses (plural)

Page 9214 L9-10. It would be better writing: "both are instrumented with discharge gauges" or similar. Nevertheless, it seems that non data about the tributaries' discharge have been reported.

Clarification of the streamflow data (that it is from established gauges) was also requested by Reviewer #1 and we will add that. The streamflow data from Valencia and Freestone Creeks is mentioned in Section 5.2 but probably should have been also be reported in Section 4.1.

Page 9216 L1. No mention in the text on discharge data from tributaries. In Figure 2 also the river gauges on the tributaries are not reported. This information is useless if incomplete as it is.

We agree that we should report the data in this section and we can add the gauge locations to Fig. 2.

P9216 L19. The expression of radon specific activities in Bq m^{-3} are quite common, nevertheless this way to present data can confound the reader since it is not very clear if the cubic meters are referred to a volume of water or air. Commonly radon is reported as Bq L^{-1} . The authors are requested to put a conversion expression between Bq m^{-3} and Bq L^{-1} (i.e., $1 \text{ Bq m}^{-3} = 1000 \text{ Bq L}^{-1}$).

We had not considered the possible ambiguity. As discussed in the responses to Reviewer #1, the choice of units gives the terms in Eq. (1) more useful dimensions. We will note the equivalence and specify that it is m^3 of water.

Section 4.3.

We will correct the table numbering to make it consistent.

Section 4.3

Units of Rn activity discussed above

Section 5.1. This paragraph could be better organized in order to discuss also the temporal shifts of the gw inflows, which are only mentioned in the text (lines 7-9, page 9221).

As with our responses to Reviewer #1, we agree that the changes to the loci of groundwater inflows over time should be better emphasised as it is a key result of the study. We will better explain this point here.

P9221 L10-11. It would be desirable to explain why the authors have not carried out a water table surface reconstruction, using the agricultural wells in the floodplain, besides those sampled.

Unfortunately, this was not possible. Groundwater levels in these wells were generally (but not always) measured during their construction, but not thereafter and so it is not possible to construct the water table at any given time. There is a water table elevation map for the region but it is constructed from a digital elevation model and estimates of depth to water that were made with numerous assumptions and little verification. This combination of data indicates the general direction of groundwater flow but is not suited for more detailed analysis and could not be used to determine the distribution of gaining vs. losing reaches. We will note the limitation in the revised manuscript and emphasise that in many river systems there is not sufficient hydraulic data to understand the details of groundwater-surface water interaction making geochemical tracers a more viable option.

P9221 L21-22. Please, explain better this sentence.

We meant that using a smaller distance step in the calculations did not change the results. We will clarify this in the revised paper.

P9224 L13. Delete “need”

Agreed, this is a typo.

P9226 L11. Insignificant vs negligible

Agreed, negligible is a better term

P9229 L5-9. The clarity of this period needs to be improved.

The point that we were trying to convey here is that we can produce plausible estimates of groundwater inflows at the higher streamflow conditions even if we ignore parafluvial flows but the analysis of Rn at low streamflow conditions makes it likely that there is parafluvial flow at all times. This also goes back to the point that we made above that carrying out studies at baseflow conditions is important in testing the parameterisation. We will clarify this section.

P9229 L18-21. This sentence is in contrast with the statement of the first working hypothesis. In other words, from this sentence the reader understands that in a natural process of trial and error the authors came to the conclusion that parafluvial flow was occurring, while from the reading of the working hypothesis the parafluvial flow seems already foreseen. There is a subtle mismatch between the two sentences. If the authors agree with this suggestion, they could adjust the text accordingly.

This comment is similar to those of Reviewer #1 as to how we framed the paper. In reality, the process was one of trial and error (or realisation). As discussed above, the paper would probably be clearer if we followed the format of stating that parafluvial flow is likely and then going on to understand it's impact on Rn. This would also agree better with how the hypotheses are expressed.

P9229 L27. As pointed out in the general comments, the choice of the groundwater samples has not been addressed properly due to the great measured spatial and temporal variability likely caused by the sampling of river bank water.

As noted above, the logic behind the location of the groundwater bores is that we wanted to sample the water that directly interacts with the stream. The Cl concentrations of the regional groundwater that is sporadically reported from the bores on the floodplain are similar to those in our bores. Importantly, the spatial variability of Cl concentrations are similar and this restricts the use of Cl in the mass balance equations. This is true in most of the catchments in SE Australia in which we have worked so is an important general point. We will note this in the results section.

P9230 L7-9. This consideration is meaningful. It would be desirable that in the discussion session a major emphasis is given to the comparison of the results during baseflow and high flow conditions, also to better introduce and discuss the second working hypothesis that changing flow conditions alter the location of groundwater inflows.

We agree that the importance of the baseflow sampling needs to be emphasized throughout as it is an important point. The second hypothesis isn't that the changing flow conditions alter the location of groundwater inflows but that major floods rearrange the sediments and landforms on the floodplain and that this results in the changes over time. We will ensure that this is clear in the revised manuscript.

Table 1.

Will correct typo in “Stream” and add units for velocity

Table 2. In the table the sampling date has not been reported. The discrepancies found in the sediment emanation rate could be due to the sampling carried out in different times at the same location (e.g. before and after a flood which could move the sediments, as the authors have reported).

All the sediments were sampled post the flood (which we will clarify). We are not sure that these are discrepancies but rather the natural variability in emanation rates; the variability is similar to that in other studies (e.g. Bourke et al., 2014; Cartwright et al., 2014) which we will also mention.

Fig. 2. For the sake of clarity and to help the reader, the names of the sampling sites could be reported following the sequence along the river, starting from Browns (BR) and ending to Chinns Bridge (CB), instead of being reported in alphabetical order.

We agree that this would probably be more useful.

Fig. 3. It would be desirable to clearly evidence the sampling campaigns on the streamflow diagram (e.g., tracing a line intersecting the discharge curve for each sampling campaign) to give immediately the information on the flow regime at the time of sampling.

We agree that this would be a better illustration of the sampling times.

Moreover, the fact that the major floods changed the geometry of the floodplain could be deleted from the figure caption and kept in the text only.

We consider that it is useful to have this in the caption so the reader can relate this to the Figure.

Fig. 8. From the text it not clear if the predicted and observed ^{222}Rn activities match also in the case of the isolated change of parameters.

This statement is incorrect in the figure caption. It is the streamflows that match and the Rn activities that are not predicted correctly. We will ensure that this is correctly expressed in text and figures.

Fig. 10. The range of variation (95% confidence interval) of the calculate streamflow should be reported on the plot (as a shaded field in the d) plot).

We can add the confidence intervals to the streamflow on the Figures that are discussed in Section 5.