Hydrol. Earth Syst. Sci. Discuss., 12, C4313–C4319, 2015
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Hydrology and Earth System Sciences
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Discussions

HESSD
12, C4313–C4319, 2015

Interactive comment on "Using geochemical tracers to distinguish groundwater and parafluvial inflows in rivers (the Avon Catchment, SE Australia)" *by* I. Cartwright and H. Hofmann

Anonymous Referee #1

Received and published: 16 October 2015

General comments

This manuscript uses Rn, Cl and water balances to examine the contribution of parafluvial flow to a river, and spatiotemporal variation in groundwater inflow to that river. The paper presents snapshots of river Rn at 6 different times over a period of six years. Repeated radon surveys of this kind are not common and this constitutes an interesting data set. However, significant changes are required to produce a work of suitable quality for publication in HESS.

The focus and novelty of this work should be emphasised more consistently throughout the manuscript. 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.

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

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. Further support for the validity of the steady state assumption implicit in the method should be provided. The manuscript would also benefit from significant editing and restructuring as outlined below.

Major comments:

1) 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.

2) 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 paraluvial flux is not plausible while keeping the focus on the

stated objectives.

3) 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.

4) 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.

5) 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. As 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?

6) 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. 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.

7) 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.

8) 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.

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).

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

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

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.

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.

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

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

18) P9211, 9212. The difference between numerical approaches for hyproheic 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.

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

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

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.

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

23) P9215 L12-17 Consider moving to introduction.

24) P9216 L22 Reference?

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

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.

27) P9218 Chloride concentrations are reported for the river and groundwater but not

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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.

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

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

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

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

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

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

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

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

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

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

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?

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

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.

41) P9229 L11 What are the difficulties? Which of these were known prior to this study

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and which are new based on this study?

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

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

44) Fig2 What are the arrows on the map?

45) Fig 4 Suggest adding streamflow

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

47)Fig 8 Suggest this fig not required.

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

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

REFERENCES

Bourke et al 2014 Characterisation of hyporheic exchange in a losing stream using radon-222, J. Hydrology 519, 94-105

Cook 2013 Estimating groundwater discharge to rivers from chemistry surveys, Hydrological Processes, 27,3694-3707

Goosef et al. 2003, Determining long time-scale hyporheic zone flow paths in Antarctic streams, Hydrological Processes 17, 1691-1710

Holmes et al 1994 Parafluvial nitrogen dynamics in a desert stream ecosystem, J Nth American Benthological Society, 13(4), 468-478

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 9205, 2015.