Hydrol. Earth Syst. Sci. Discuss., 10, C2582–C2586, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C2582/2013/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Examining the spatial and temporal variation of groundwater inflows to a valley-to-floodplain river using ^{222}Rn, geochemistry and river discharge: the Ovens River, southeast Australia" *by* M. C. L. Yu et al.

Anonymous Referee #2

Received and published: 15 June 2013

GENERAL COMMENTS

The manuscript "Examining the spatial and temporal variation of groundwater inflows to a valley-to-floodplain river using 222Rn, geochemistry and river discharge: the Ovens River, southeast Australia" by Yu et al. focuses on quantifying the flux of groundwater to an Australian River, primarily through using Radon as a tracer of groundwater. The authors divide the river into three geomorphology segments (upper, middle and lower) and compare how using radon as a groundwater tracer compares to using a frequency

analysis based hydrograph separation technique and using a chloride mass balance mixing approach.

Overall the manuscript is well written and easy to follow. The authors have collected an impressive robust multi-year dataset across the entire Ovens River watershed, which provides many opportunities for comparing groundwater discharge methods. My major concerns with the manuscript focus on the use of Cl for the geochemical mixing model and in the interpretation of the results. Following are my major comments, with minor corrections afterwards.

SPECIFIC COMMENTS

From looking at the results, there is a fairly poor correlation between the radon and Cl results. I understand very well the issues with the lack of range in the Cl concentrations. If the Cl method worked so poorly, why was it used as a solute tracer? The authors state in the introduction that "The requirements for using geochemical tracers to study GW-SW interactions are that the concentration of the tracer in groundwater is significantly different to that in river water…" It would seem that based on this important criterion and the poor results using the method, that a different solute tracer would be more appropriate. In my opinion, it would be misleading to conclude from the manuscript that the geochemical approach does not work if it is being incorrectly applied.

An obvious solution to the Cl issue would be to choose a different tracer. In the acknowledgments the author's mention stable isotope results. Assuming that they are referring to 18O and deuterium (which are not reported in the manuscript) this might be a better tracer to use for a geochemical mixing model. A more advanced method would be to use a more statistical approach such as End Member Mixing Analysis (EMMA).

In the comparison section (Section 5.5) of the manuscript the authors compare the results from the three baseflow estimation methods. From my reading, it seems that the underpinning idea is that the radon results are the 'correct' answer, and the comparison is in actuality explaining why the other two methods do not produce the same results. While the radon result are very interesting, I am not sure that there is sufficient evidence to say that they are the true value of baseflow contributions. I think this section should be reworked to avoid this issue.

Related to the previous comments about comparison of data, I think that a more robust statistical comparison of the three methods is needed. While a qualitative comparison is instructive, given the range of values it seems difficult to make these comparisons. Even a scatter plot of radon vs Cl groundwater values for all of the data points (distance along stream and time) would be valuable in seeing where the two methods converge and diverge.

From looking at Figures 7 and 8, is the statement 'Relatively higher groundwater inflows occur in the upper and middle catchment (Page 5240, Line 14-15)' actually true? From the figures it looks like the locations and timing of the highest baseflow values are very variable both spatially and temporally. And, from the cumulative baseflow results the greatest increase in baseflow is clearly in the middle section of the river.

Further related to the cumulative radon results, the biggest increases in baseflow appear to be two temporally persistent step-increases in the middle section of river. Is this a result of sampling density, or are there actually two locations where there is a very large groundwater input to the river? Is this evidence of focused groundwater discharge (e.g. along a fault or fracture network) that is having a large control on the total flux of groundwater to the river? Or is this a result of sampling density and how the results are plotted?

Some of the results show a very strong saw-tooth pattern (ie. based flow fluctuates between very high and low values along the river). For example, on Figure 7b the Oct-11 data are notably variable from point to point compared to data from other sampling events. Is this a real phenomenon or is it a result of the poor data and/or data processing or something else?

I would suggest that some of the results and discussion be rephrased to clarify the interpretation of the results. The authors often write about the range of results, which while accurate, are sometimes difficult to interpret. For example, on Page 5240, Line 23 the authors refer to '4-22% of the total flow' which is a fairly wide range, or two line later they refer to the minimum baseflow contribution as being 18-70% which covers almost the entire spectrum of possibility. I would suggest that these sorts of quantitative ranges could use additional discussion. Maybe using the range and the mean and/or the standard deviation may provide more information for a reader.

While the manuscript is focused on the inflow of groundwater to the Ovens River, the methods used do not quantify the amount of river water that enters the groundwater system. Do the authors expect there to be losing reaches of the Ovens River? Should zero values of baseflow be taken as zones across which the river is actually losing water?

Does the amount of water flowing into the river via baseflow equal the observed increase in discharge or baseflow across the Ovens River? (Can this sort of calculation even be made?) Or, by a different approach, could a simple Darcy calculation be made (given the hydraulic gradients that are presented and permeability data that must exist for the region) to calculate the flux of groundwater to the river, and then compare that value to the study results?

My final comment is with regards to the conclusions of the study. The final take-home message of the manuscript is very focused on the Ovens River and the methods used in the study. I would urge the authors to put their results into a broader context. What is the transferability of these results to other river systems? Are their findings from this study that change how we understand groundwater-river interactions to behave? Or are there new methodological ideas for how to approach such a study?

TECHNICAL CORRECTIONS

The first introductory paragraph requires additional citations. Currently there is only one citation for a lot of information regarding the complexities of surface water - groundwater interactions.

The term ‘hydraulic loading’ should be defined in the manuscript.

Line 7 - ‘water in unsaturated zone’ should be ‘water in the unsaturated zone’

Line 21 - ‘calculate water budgets’ would be better as ‘calculate hydrologic budgets’

Page 5229, Line 16 - Something is missing - ‘the temporal of GW-SW exchange’

Page 5230, Line 8 - Change ‘pass’ to ‘past’

Page 5231, Line 3 - Change ‘slit’ to ‘silt’

Page 5233, Line 8 – What is TPS?

Page 5237, Line 10 – Change ‘decrease’ to ‘decreasing’

Page 5237, Line 25+ - What is source of Na?

Page 5243, Line 5 – Change ‘for individual reach’ to ‘for individual reaches’

Page 5243, Line 17 – ‘assuming’ should be ‘assumed’

Page 5243, Line 16 – I would argue that there are many different hydrograph separation techniques, one of which is ‘Hydrograph separation employs a low

Page 5247, Line 1 – Unfinished sentence

Page 5247, Line 24 – Delete ‘do’

Supplementary data – ‘nm’ must be defined. Assume it stands for ‘not measured’

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 5225, 2013.