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Hydrology and Earth System Sciences Discussions

Interactive comment on "Using stable isotope tracers to identify hydrological flow paths, residence times and landscape controls in a mesoscale catchment" *by* P. Rodgers et al.

Anonymous Referee #2

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This publication addresses a topic that is potentially of interest for both research and application communities. It discusses a relatively simple isotopic technique applied in nested catchments, and explores how this technique can be used in larger more complex catchments. I believe that this is a good and necessary way and that this manuscript can be therefore published. Before the publication, however, I strongly advocate for a substantial enrichment by new thoughts and ideas. Indeed the manuscript declares neat promising goals to move from too "over-sophisticated" methods to more universal techniques applicable in environments beyond the small scale and beyond our "developed" part of the world - but at the end it does not leave the standard schemas and vocabulary known from many small-scale studies. Therefore, unfortunately, much potential remains "behind the scene".

One or more working hypotheses can be formulated in the introduction. For example, testing the equivocal relation between res.times and catchment size (what implication this would have for the catchment management? There is a neat small-catchment study towards that in Boehlke, Denver, WRR 1995). With increasing catchment size it seems to be obvious that the isotopic composition of stremwater dominantly reflects the large-scale climate patterns (Kendall, McDonnell, 2001 or 2000, Hydrol. Proc.) and that “residence time” is a very unclear characteristics for such large systems. Gibson et al (2002, EOS 83) show isotopic variations of different origin in different rivers, while it is obvious that an extrapolation of a hypothetical res.time*area correlation does not work beyond small scales. If, for example, the authors estimate a res.time of +-0.5 y at Brocky Burn (1.3 km2) and +-2y at Powlair (61.1. km2), how far can this extrapolation go for catchments of a size beyond 100-200 km2? Are the residence times 10 or more years in these cases? Or is there a threshold in this extrapolation, along to another hypothesis (Haitjema, 1995, J. Hydrol.) that states that in an exponential-distribution-like system the flow-weighted mean travel time is independent on horizontal distance (i.e. independednt on scale)? In such idealized systems the flow-weighted mean travel time of water discharging as baseflow into a river will be the same in the upper and lower reaches of a watershed (Haitjema, 1995). Is the decrease of estimated res.time at Heugh Head (+- 1y with 233 km2) in comparison to Powlair a sign of such a threshold? - - I’d appreciate addressing of such and other interesting issues, at least in form of a brief literature review or an outlook commentary.

I believe that the selected method (sine-wave 18O -variations) is a suitable universal way for comparison of catchments, although the research community considers it as a rather simple procedure. In any case the O-18 variations (with or without estimation of residence time) could be a reasonable complementary indicator of the runoff generation (although I would be careful with statements such as (p.1., row 21) that those “insights” are unavailable by other methods). Reviewer #1 correctly pointed out some questionable technical points related to this method: it needs longer O18 records in rain and stream, it needs extrapolation of the O18 in rain prior to the analysed period, and, incase the main target is the baseflow, it needs also the O18 record in rain corrected for recharge and the O18 in stream considered only under baseflow conditions. These steps are most likely necessary in small catchments, while in more complex catchments they might become very difficult to perform and interpret. Here a challenging analysis could be done: how much (across scales) differ the results obtained by the sinus-wave method from those obtained by more sophisticated transport models? Maybe they wouldn't differ very much on a larger catchment scale, where several details are biased. I would advocate for a review/discussion of these methodical issues as well, as it would be a very useful contribution to practical studies in sparsely instrumented catchments. Those are the places where the sinus-wave method can be crucially important in a variety of practical problems, far beyond the purely research sites. Other "surrogate" res. time indicators might be discussed, such as the "contact time" (Wolock et al, 1998) or the runoff recession (Vitvar et al, 2002).

Some specific comments (in addition to those pointed out by reviewer Nr. 1)

p.3row 2 - catchment waters - maybe "natural waters" is better row 3 - hydrological source areas - maybe just water sources is enough row 9- Sklash,not Skalsh row 12 - residence time of runoff is an awkward term - runoff does not have any residence time, runoff is just a process/movement of water. Water itself has a residence time, therefore "residence time of streamwater" might be a better term here p4, row 18 - as above row 19 - catchment landscape controls are related to residence times - - this is a quite awkward formulation too. Maybe a simpler"relation of residence times to major runoff processes"is better chapter 3 - it might need more about the alkalinity separation method. Res. times are explained, while baseflow separation is not. P8, row 18 - "residence time models" is also an awkward term- res. time is not modelled, it is just estimated. Flowpaths in aquifers indeed are modelled (or simply a model setup of flowpaths is assumed) Chapter 4 - should be called Results and Discussion, and I recommend a simplification of subchapters: - 4.1. 18O variations in rain and streams (covers from 4.1. to 4.3) - 4.2Mean residence times of streamwaters (covers the rest) The reasons are the following: 1. a better balanced structure (now 4.1. and 4.2 are too short, and 4.3. is too long) 2. removed awkward formulations such as “inputs”, “outputs”, “hydrological controls”, “O18 patterns” and “preliminary estimations”. These formulations do not seem to be very clear to me, although they became for some reason a standard research “vocabulary”. Inputs and outputs probably originate from the batch reactor theory, that was the background of the flow and transport boxmodels. However, in the nature, there are just rain and stream, and there is no reason to talk about inputs and outputs. No one beyond the narrow research community knows what it means. “Hydrological controls” are also awkward and unnecessarily complicated, while talking about simple factors that might cause various O18 records (a form of valley or hillslope is actually not necessarily a “hydrological “ control).”O18 patterns”actually don’t exist; water flow patterns indeed exist, while O18 might trace these patterns. Estimations of res.times provided ni this manuscript are, I believe, not very preliminary, given they are calculated in an exact range of, for example, 0.41 to 0.74 years.

Tables 1,2,3 - an order of subcatchments according to the increasing size might be more transparent - athough the size might not be the key impact on the res.times.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 1, 2005.