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Hydrology and Earth System Sciences Discussions

Interactive comment on "Hydrograph separation and scale dependency of natural tracers in a semi-arid catchment" *by* R. Bohté et al.

Anonymous Referee #1

Received and published: 3 April 2010

This paper aims to analyze scale dependencies and hydrograph separations in semi-arid catchments. The title and abstract sound very promising, but the results and data of this paper do not support the expectation I had from the title. In general I have the feeling that this paper is an attempt to publish data that is from a location without too many hydrological studies, but without being able to provide new insights into the question of hydrograph separation and tracer methods and without providing new scientific insights that support a paper in HESS. I will list my concerns in detail below and I hope that the authors can provide an appropriate response and revisions.

1. The introduction is very short and is far form providing a good coverage of the state of research in hydrograph separation. In particular, there is not any focus on the topic of uncertainty and in particular on the proposed scale dependencies. There have been

many studies analyzing scale dependencies of hydrograph separation (HS)and using several tracers to perform HS. Similarly, in the following chapters the paper focuses on hydrograph separation than on the problems of different scales: only two different scales are investigated and the temporal scale of comparison is only event scale and even not the same events for the small (Mataini) and large catchments (Vudee and Ndolwa). This is not acceptable when the focus is on scale dependencies – I would at least expect 5-10 catchments with different scale and analyzing several events that resulted form the same rainfall events.

2. The paper concludes that the assumption of stable isotopic and tracer end-members was not met in this study. Considering preceding research, which already used this effect to determine the origin of water (Kendall et al., 2001; Scholl et al., 1998; Schotterer et al., 1993), these results are by far not new, they should rather have been expected. If this is the main message of the paper, I would suggest rejecting the paper. The additional point, that "at the smaller scale the spatial variability could be neglected" has not been proven in the MS.

3. Instead of focusing on the scale problem I recommend to focus more thoroughly on the hydrograph separation itself, for example to investigate further the propagation of uncertainty (which should be standard nowadays when applying simple HS methods) and to use more tracers (sometimes the manuscript refers to Mg2+, Ca2+ and HCO3- observations, which unfortunately can't be found in the figures and tables). By doing so, a valuable understanding of the regional hydrology can be provided and recommendations for further observation campaigns in semi-arid areas can be given.

But in any case the manuscript needs a substantial revision: it was frustrating to look for data referred to in the text or figure descriptions which was not there or to find out that the content of other papers was cited wrongly. Additionally, the nomenclature should be revised: there should be only one name for the seasons and the catchments etc. Likewise, the design of the figures is not appropriate for HESSD.

General comments:

1) the authors use r instead of r^2, which is rather unusual as it gives a wrong impression of the degree of correlation (surprisingly, in fig. 7 they use r^2, but only once)

2) there is no uncertainty analysis (Genereux, 1998) - especially in this case it would be really interesting to see how input uncertainty propagates through the hydrograph separation and how the HS of the different tracers match considering uncertainty

3) the figures are really difficult to read - the tick label font size as well as the axes' description are by far too small; in particular, the study site map if very unclear: the catchments are not shown, the labels are too small and have confusing names and there is no description in the legend about the different stream colors (and what is this black bar in the upper right corner?)

4) Why is dD used for hydrograph separation instead of d18O (which is less affected by evaporation, which could be certainly a concern in this environment)? Or even better, why not using 18O and D.

Specific Comments: 1) P. 1345 L. 14: the statement is very general, but the citations don't give an overview about the research which was done in this field

2) P. 1346 L. 14: site description does not belong into the chapter "Materials and Methods"

3) P. 1347, L. 11: were these devices installed at the nested Mataini catchment or somewhere else inside the Vudee catchment (this not clear neither in the next nor in figure 1)

4) P. 1347, L. 25: Is the small forested catchment the Mataini catchment? Please use clear descriptions

5) P. 1348, L. 15-20: If springs are investigated, too, there must be some information about the geology/hydrogeology of the study site

6) P. 1349, L. 22-25: there are also studies which show different results (eg. Brown et al., 1999)

7) P. 1349, L.22-26; P. 1350, L.1-5: most of these processes describe the mobilization of pre-event water, which is not groundwater in every case - better use the "pre-event water" instead of "groundwater"

8) P. 1350, L. 10: Three? Is this really all?

9) P. 1351, L. 11: The term "collinear" should be chosen instead of "correlated" (as it was also done by Hoeg et al. (2000))

10) P. 1352, L. 10-22: EC and SiO2 are not conservative. So they can adapt to the pre-event concentrations, especially EC (and SiO2 depending on the type of rock, which is not mentioned in the site description)

11) P. 1353, L. 9: Only three events are listed in the table, not four.

12) P. 1353, L. 16, Figure 5: use a,b,c,... to distinguish figures, not "upper", "middle", etc.

13) P. 1354, L. 12-15: references for graph colors, etc. belong in the figures description, not in the text

14) P. 1355, L. 1: if the upper edge of the Vudee season range would be true, no significant difference would exist - please clarify this statement

15) P. 1355, L. 19: what about the altitude effect on precipitation isotope composition? The d180 ratios of both stations are really similar despite of 500 m of altitude difference.

16) P. 1355, L. 23-25: this sentence is absolutely not clear.

17) P. 1355, L. 25: the note added by "1" is confusing since it begins at the same page but goes on on the next page. It should be placed into the text.

18) P. 1356, L. 12, 16-17: where are the other hydrochemical parameters? There is no

figure or table.

19) P. 1356, L. 17-18, P. 1357, L1: clarify this sentence

20) P. 1357, L. 9: The effect of evaporation should be mentioned much earlier and it should be discussed not only concerning the springs

21) P. 1358, L. 1-2: if this conclusion should hold comment point 14) has to be elaborated

22) P. 1366, Fig. 2: the figures should have the same size and the rain-arrows in the dD plot for the March 25-28 2008 event are quite confusing (what do they mean? Either add in figure description or discard them)

23) P. 1368, Fig. 3: using triangles and quarters of different colors to depict the contributions calculated by different tracers is confusing, too - there are other ways to show this data in a clearer way – the current figure is very difficult to read

24) P. 1368, Fig. 4: in order to refer to the different subplots in this figure, they should be given a name (a,b,c,d,...)

25) P. 1369, Fig. 5: in the description, the authors refer to precipitation, discharge, EC, SiO2 and dD measurement, but the figure shows only Q, P, GW and EC.

Grammar/repetition of words (P. 1345, L. 20-21 “catchment”; P. 1350, L. 17-18 “groundwater”, P. 1354, L. 29 “, respectively” to end of sentence; P. 1356, L. 4-5 either use “November event” or “21 November event”;)

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 1343, 2010.