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## ***Interactive comment on “Hydrochemical analysis of stream water in a tropical, mountainous headwater catchment in northern Thailand” by C. Hugenschmidt et al.***

**Anonymous Referee #4**

Received and published: 12 May 2010

Posting the fourth referee comment, I generally agree with the concerns raised by the three reviewers before, which makes the paper non-publishable in its present form and with the present data and methods. These are mainly: (i) a very small data base (missing spatial variability of endmembers, only three events with limited temporal resolution, missing hydrometric information), (ii) problems in the application of EMMA due to unclear endmember definition, (iii) a missing link to pesticides, (iv) a misleading title. Only if the data base is substantially increased including more events and maybe hydrometric information (e.g. soil moisture / gwa levels and gw chemistry) and a better definition of the kind of endmembers and their variability in space and time the results and discussion are backed up by the data and a publication could EVENTUALLY be possible.

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Although this will be very difficult. I include some additional recommendations which might help along.

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(1)Introduction: I doubt whether temperate and tropical regions can so generally be distinguished in their hydrological characteristics as one might infer from the introduction. The introduction also lacks structure and would benefit from a distinction between permanently wet “inner tropics” and “outer tropics” with distinct dry and wet seasons.

(2)Though already stated I would like to comment again on the way EMMA was used and how endmembers were defined. There is no clear distinction on the basic concept to distinguish between endmembers: was it residence time based (pre-event / event), was it dynamic (baseflow / quickflow), source area oriented (groundwater, hillslope-water) or process oriented (surface / subsurface). This has a bearing on the definition of the endmembers and on the interpretation of the data.

(3)Looking at table 2 (apart from silica and sodium) surface runoff is more enriched in all other components compared to interflow which also causes higher EC values. How can this be explained? And could this information be used for additional process insights?

(4)It is stated that: “the lower concentrations of interflow may have occurred due to dilution by quickly infiltrating rainfall water” and later: “surface runoff is most likely a mixture of HOF, return flow and SOF”. So is then a separation of interflow and surface runoff meaningful?

(5)The use of EC as a non-conservative summary tracer could be backed up by a calculation from the measured ion concentration.

(6)Give more details on the annual water balance maybe by presenting a figure of the entire season and compare the obtained annual value for ET and runoff coefficients with other studies in tropical regions.

(7)Include information on landuse (forest / agricultural areas, etc.) in a map (e.g. figure

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(8) More information on data collection is needed: e.g.: how (by collection method) could you guarantee that pure surface runoff was collected in a trench?, how that you measured only “interflow” (whatever this is in your case?)

(9) Provide information on the numbers of samples and on the accuracy of the measurements.

(10) If hydrograph separation is used, uncertainty bounds can easily be incorporated including the variability of the endmembers and the analytical errors.

(11) In general there are too many and sometimes redundant numbers in the text, check carefully, maybe produce tables?

(12) Fig 6 is not discussed, hence not needed.

(13) Fig 8 check the “cake diagram” this seems to be wrong

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 2187, 2010.

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