

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

### **Anonymous Referee #1**

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The topic of the paper is within the scope of HESS. It presents new data on water chemistry (ions) for a headwater tropical catchment in Thailand where this type of information is rare. The paper is not however acceptable in the current form: MAJOR REVISION is required.

1) One main flaw with the study is that the data come from only three events, sampled over two seasons. This has to represent the bare minimum acceptable for a journal of this caliber – especially given the number of coauthors. In fairness, it is not quite clear to me if these events were the only ones when all investigated stormflow components were active. This latter point should be clarified. Another problem with sampling is

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that there do not seem to be data for rising limbs—a common problem with synoptic sampling in flashy systems. This missing rising limb data suggest to me the sampling campaigns were insufficient.

2) The other major flaw of the paper is the excessive reference to pesticide transport via various flow pathways (done in prior work by other researchers). This paper should focus on the ion chemistry patterns and the stream hydrograph separation. In the discussion at the very end of the paper, it would then be acceptable to say the observed trends support the notion that pesticide movement could be through interflow processes, as demonstrated by others at the site. Also, at this point the threshold rainfall depth at which this occurs becomes particularly relevant and important—this potentially the most important result of the study.

3) The paper is too long (and has too many figures), given the small amount of data collected. It would be better presented as a short note focusing on the ions, hydrograph separation, and threshold for activating the interflow component. For the separation, only the best result (either two- or three-component separation) needs to be shown. With regard to interflow, it must be made clear that the site was monitored continuously over the two seasons so that it is known for sure return flow did not occur before the threshold was found.

4) One cannot help wondering if the return flow and interflow observed is representative of the preferential flow alluded to in prior pesticides studies. Is it possible that the processes observed in this study were simply localized near-stream processes, rather than hillslope-scale processes? This needs to be clarified; and the seep should be explained or even shown.

5) I am not sure all readers would agree the identified “baseflow” component is indeed only baseflow. The authors might want to think about this and convince themselves this really is the best explanation of the process.

6) In the map it is difficult to see the catchment boundary. Also, there do not appear to

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be any rain gauges inside the basin. If this is true, one questions the value of any of the reported rainfall patterns for the storms, because rainfall is highly variable, even at small spatial scales.

7) I don't think one can comment on how close soil layers are to saturation if it wasn't measured—although this is probably relatively true for most tropical areas with distinct wet seasons.

8) Abstract: (a) The term “vulnerable mountainous” is vague; (b) Is it true that pesticides are “mainly lost by interflow” or is it just an important process; (c) keep in mind that the timing of pesticide application relative to a rain event is also important determining how toxins will move to the stream system.

9) Introduction: (a) Surely there are some supporting studies for the land cover change in the region; (b) Isn't there a more recent reference that the 1991 Weischet paper? (c) Is it really (generally) true that tropical and temperate soils differ in porosity and hydraulic conductivity? (d) Be careful using the term “region” if you are referring to your catchment—your region would include parts of Thailand, Laos, Cambodia, Myanmar, and China.

10) Methods: (a) It is probably ok to say mangos, teak, and pine rather than using their latin names; (b) what is the source of the climate data? (c) it is not really necessary to mention the ISCO sampler for collecting pesticide samples because this was not done in the study (same for weather station); (d) event-based field campaigns are mentioned, but the presentation of data from only three makes one wonder if there are more data that are not shown; (e) report where the ion chromatograph is located (Thailand or Germany)?

11) Results: (a) is all the information on the top of page 8 needed? (b) make sure sample numbers are listed for means/medians.

12) Discussion: This all comes across as pure speculation that is not sufficiently sup-

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ported by the data or observations.

13) Conclusions: Be careful about presenting the pesticide information in way that makes it appear it was determined in this study.

14) Table 1. Make sure EC data agree with Figure 7.

15) Table 2. Is this needed?

16) Figure 1. Clearly show catchment boundary and measurement sites,

17) Figure 2. The caption does not agree with the legend.

18) Figure 9. Data at beginning of storm on 28 Sept are not particularly believable. What extent of the variability in these data could be related to experimental error?

19) Figure 10. Is this needed?

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