Responses to comments of Referee#1 on HESS-D paper hess-2010-65:
"Hydrochemical analysis of stream water in a tropical, mountainous headwater catchment in northern Thailand"

First, we would like to express our gratitude to the Referee. We appreciate the comments and will address the comments adequately in a revised version of the paper. The referee does not accept the paper in the current version and asks for MAJOR REVISIONS. We are prepared to carry out significant revisions for a manuscript to be considered for publication in HESS, if invited to do so.

In the following we give a short summary of the comments and explain for each of the comments, how we would like to address the mentioned issues.

A technical note: References not yet included in the reference list of the manuscript are listed in Appendix 2 – References. New figures (A1 to A7) and tables (Table A1) are shown in Appendix 1.

Response to Referee #1

Comment 1a) Insufficient/limited database, **1b)** the referee asks if the events shown were the only ones, when the investigated stormflow components were active, **1c)** no data for the rising limb of the hydrograph.
Reply 1a) We are aware of the limitations of our database. Nevertheless, it should be considered that we carried out the study under difficult field conditions in a rarely investigated area, and we are convinced that the study helps to improve the knowledge and understanding of hydrological processes in such data-scarce regions. We would like to include two more events in a revised paper version, which were sampled in 2009 (electrical conductivity and silica). This will strengthen our database.
Reply 1b) No, the regular weekly visual observation of this leakage during the rainy season of the last four years shows that at some time in the course of the rainy season the leakage (in our understanding interflow) is activated and discharges continuously to the stream (see Figure A1 and A2, Appendix 1).
Reply 1c) The hydrograph in this catchment shows a very rapid response and reaches maximum flow rates within 20-30 minutes. However, there are data from the rising limbs (2-3 samples, depending on the duration of the rising limb).

Comment 2) Focus is too strong on pesticides.
Reply 2) We agree, however, to change the perspective of this paper. The revised manuscript will focus on the runoff generation processes.

Comment 3) Paper is too long, includes too many figures and only best results should be shown.
Reply 3) We will shorten the paper, focusing on the hydrochemical data, and remove some of the graphs (see as well Comment 10-13). The results will focus on the results of

the hydrograph separation including the (new) results gained in 2009. This will help to strengthen our conclusions.

Comment 4) Interflow/Return flow rather near-stream processess than hillslope processes? It should be clarified and shown.
Reply 4) The work of Kahl et al. (2007 and 2008) revealed that pesticides applied at the hillslope at a significant distance to the stream (50 m) were measured in the stream during the recession limbs of events, when no surface runoff was present. We take this as a strong indication for lateral flow along the hillslope other than surface runoff (see Figure A1 and A2, Appendix 1).

Comment 5) Definition/Identification of baseflow
Reply 5) We are assuming that the referee is referring to the definition given in line 155 ff, which might indeed be misleading, because base flow is a part of stream flow at all times: sustaining the stream flow during rainless periods, and generating storm flow peaks as well. We will carefully rewrite the definition in the revised manuscript and mention that additional subsurface flow components can be activated during floods.

Comment 6a) The catchment boundary in Figure 1 is critizised to be not clearly visible and **6b)** it is asked, if there is no rain gauge in the catchment, which questions the value of any reported rainfall patterns.
Reply 6a) We agree that it is not really necessary to show the whole area. Therefore we will only show the sub-catchment in Figure 1 including the rain gauges that were used to measure the rainfall in the catchment.
Reply 6b) The referee is right: no rain gauge was located within the catchment. However, the distances of the lower two rain gauges to the stream gauge is 500-700 m and the upper two gauges are within 1000 m distance to the catchment. We calculated the cross-correlation coefficients (CCR) between the rainfall stations and between rainfall of the single stations and runoff before using the rain gauges for this work. The CCR were computed for each month (September and October) based on time series with a 10-minutes resolution (144 records per day, and 4464 records per month). The CCR show very good correlations, and therefore we assume that these gauges can be regarded as representative. CCR between the lower two gauges and the stream gauge is 0.6 with a lag time of 60 minutes in September and 0.7 after 60 minutes for October. The upper two gauges result in CCR between rainfall and runoff in September 0.5 with lag time of 80 minutes and in October 0.3 with a lag of 50 minutes. This latter data were not included due to the low CCR. These facts and discussion of representativeness will be included in the revised manuscript.

Comment 7) The degree of saturation of soils is questioned as there are no measurements on soil moisture, etc.
Reply 7) This is correct. We can not justify a quantitative statement on the degree of saturation of the soil and this will be revised. Nevertheless, there must be a significant change in the moisture content, because the leakage is not active from the first day of the

rainy season and also does not end suddenly when rainfall intermits. In qualitative terms, the change in soil moisture could be clearly observed in the field.

Comment 8a) The terminology 'vulnerable mountainous' is vague**, 8b)** questions, if interflow is a main pathway of pesticide loss or just an important process, **8c)** timing of application of pesticides in terms of rain event is important as well regarding its transport to the stream.
Reply 8a) The expression 'vulnerable mountainous' will be eliminated and simply changed to 'mountainous region'
Reply 8b) Kahl et al. (2007) concluded that preferential interflow besides surface runoff is an important contributor to pesticide losses to stream to explain the delayed pesticide peaks after discharge peaks. It was imprecise to write that pesticides are 'mainly lost by interflow' but at the hillslope under study it is a very important process.
Reply 8c) We agree and are aware of it. This point will be discussed in the revised manuscript.

Comment 9a) studies on land cover change, **9b)** more recent publication, **9c)** is the difference of tropical soils and temperate soils really given? **9d)** 'region' might include wider areas, other than the watershed.
Reply 9a) The studies on land use change in Mae Sa watershed (were the study area is located in) are rare to non-existing, however the "*Agricultural statistics of the Mae Sa watershed area, Thailand, 2006*" (Schreinemachers et al., 2008) gives a comparison of land use change between data obtained in 1974 (Irwin, 1976) and data collected in 2006. The result show an increase of annual crops from 8.0% (1974) to 37.8% in 2006, and a reduction of rainfed rice from 20.7% in 1974 to 0.5% in 2006, for example.
This will be clarified in the revised manuscript.
Reply 9b) We can include a more recent publication (Schuler, 2008, for example) and/or give the coordinates of the catchment, which will clearify the location of the catchment.
Reply 9c) This statement is based on Bonell (1993) and can also be found in Hodnett and Tomasella (2002):"Compared with temperate regions, general differences exist in texture (less silt content), mineralogy, and structure". Detailed soil surveys in the catchment (Schuler, 2008) showed that the soil types found in our study area (mainly Acrisols and Cambisols) show a sharp decrease in hydraulic conductivity. It is, however, imprecise to say that there is a general difference of tropical and temperate soils, but it is a characteristic which is definitely given for Acrisols and Cambisols in our region.
Reply 9d) We will change it to 'northern Thailand'.

Comment 10a) Latin names are probably not necessary, **10b)** where the climate data comes from, **10c)** not necessary to mention ISCO sampler, as it is not used in this study, **10d)** asks, if there are more data gained from the field campaign than shown, **10e)** location of the chromatograph.
Reply 10a) Latin names will be removed.
Reply 10b) The climate data were measured at the weather station (which includes the MSN rain gauge) mentioned above (500 m air line from the stream gauge) and shown in Figure 1.
Reply 10c) ISCO will be deleted.

Reply 10d) Electrical conductivity was measured during September and October 2007 and in October 2008 during several days. However, we were not able to catch more complete events (meaning, the rising limb, the peak and the falling limb). The hydrochemical analysis was done based on the analysis of the EC measurements and the coverage of the complete hydrograph.
Reply 10e) The ion chromatograph is located in Thailand at the Department of Chemistry of the Chiang Mai University, Chiang Mai, Thailand.

Comment 11a) Asks if the summary of the general conditions on top of page 8 is necessary, **11b)** sample number should be listed.
Reply 11a) We think it is necessary to show the differences of rainfall distribution in both years, as the results of this work show that the amount of rainfall is important for triggering interflow. *Referee#4, (Comment 11)*, remarked that there are too many numbers in the text, too, therefore we would like to present the data/numbers in a table.
Reply 11b) The sample number will be listed.

Comment 12) Discussion is highly speculative, no data or observations support this.
Reply 12) We would like to shorten and partly re-focus the paper following the suggestions mentioned above. Data from two more events (new, from 2009, Duffner et al. in preparation) will support our conclusions.

Comment 13) In the Conclusions, the pesticide information should not be mentioned as it was the scope of the study.
Reply 13) We agree and will put more emphasis on the hydrochemical data (see Comment 2) –.

Comment 14, 15, 16, 17, and 19): Remarks on Table 1 and 2, Figure 1, 2, and 10.
Reply 14, 15, 15, 17, and 19: Corrections in Table 1 will be performed as suggested. Table 2 can indeed be removed, as the results are shown in the 'cake diagram'.
Figure 2 will be removed, and Figure 1 will be modified in such a way that is only showing the catchment under study.

Comment 18) Data in the beginning of event of September 28 are not particularly credible. Experimental errors are remarked.
Reply 18) The data in the beginning of the event on September 28 is during the recession of a small peak before the event. Thus, it is the result of a prior event and not an experimental error.