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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Modelling pesticide leaching under climate change: parameter vs. climate input uncertainty" *by* K. Steffens et al.

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General comments

This paper is concerned with assessing the importance of uncertainty in pesticide leaching model parameters relative to uncertainties in climate change predictions (as defined by a range of future climate predictions produced by different GCMs and emission scenarios).

Overall, the paper is well written and of a high technical standard. The authors have made a good attempt at performing and presenting a thorough statistical analysis of their data and the findings presented will be interesting for researchers in both the climate change and pesticide leaching communities.

The authors argue that whilst the uncertainties of CC have been explored extensively in many aspects of catchment hydrology, they have not yet been addressed for pesticide leaching. I think that this provides a reasonable rationale for the work presented.

Provided that the authors can provide adequate responses to some specific questions and observations below, I would have no objection to this paper being published in HESS.

Specific Comments

Abstract: Line 1: risk OF pesticide leaching. My understanding of risk is that it is, strictly speaking, a function of both the likelihood of occurrence and the consequences of a potentially harmful event. Here, we are only concerned with likelihood – effects are not considered. I am aware that risk is used in the same way elsewhere in the literature but I would prefer to see it reserved for studies of true risk. Same applies to Line 24.

P 10463 Effects of temperature. Probably the biggest effect of increased temperature would be to increase PET. Whether this is manifested as an increase in AET will depend on the pattern of soil moisture content and any influence of a CO2 fertilisation effect, which will probably increase transpirative efficiency. Comment?

The idea of increased diffusion rates reducing preferential flow is interesting but diffusion is slow relative to advection so how much difference would even a big increase in D make? Presumably this is covered by Jarvis (1998)?

I agree that a change in temperature should change the sorption of many pesticides. It is stated on p10466 that sorption was assumed to be temperature-dependent. However, I think some brief details of how this effect was accounted for in MACRO are warranted here (despite the fact that it may be covered in Steffens et al., 2013). It is stated on p 10476 L 20, that the effects of temperature are larger for more strongly sorbing pesticides. This implies that these compounds have a higher (absolute) enthalpy of phase change. Was this accounted for here? If not, then this statement is

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speculative and should be modified accordingly to make clear that this was not included in the analysis. Surely, if the same enthalpy of phase change is assumed for all the hypothetical compounds then the slope of log KOC versus temperature should be the same and the change in sorption for a given change in temperature should also be the same? Comment required please.

P10466 Good to see that internal state variables were used in calibration and validation.

P10467 In principle, it is good that only two system parameters and two pesticide-specific parameters were chosen for the uncertainty analysis as it avoids "over-fitting". However, more justification is perhaps warranted here about why these parameters were "considered" uncertain.

Why were uniform distributions assumed? This implies that all values in the range are equally likely. Is that really the case?

Also, why use a different KOC for topsoil and subsoil? Surely sorption will be normalised by the OC content of the soil layer. If KOC(top) and KOC(sub) are not correlated in the LHS, you could get simulations in which a very high value is chosen for one and a very low value for the other (or vice versa). Is this realistic? This comment is also relevant to the results and discussion presented on p10473.

P10468 How far is Lanna from the weather station at Satenas. Precipitation is extremely sensitive to spatial variations. I don't think this will have any major implications for the work described here but perhaps the authors might like to reassure the reader that this is the case?

P10472 Why was the reference climate disregarded?

P10473 Fig 3. Why not show the frequency as a relative frequency? This would make more sense than counts.

L13-14 re the inability of the model to simulate the 1st peak. It looks to me like there

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are no predictions of Br or bentazone concentrations for this initial period, presumably because no drainflow was predicted. The authors should be more explicit about this here.

Technical Corrections

P10464 Are GCMs called Global climate models these days? They used to be known as General Circulation Models.

P10466 L 10 systemS

Delta change method or delta method?

Re GCMs. At some point references should be given for the GCMs used in the RCM scenarios presented in Table 2. Have I missed something?

P10469 Why was relative humidity kept unchanged? Might help the non-meteorologically minded reader, like myself!

P10469 L 28 “rather” not needed.

P10470 Nice to see application date included.

P10478 L 5 rather – better approximately. L17 strengthened

L16-19 sentence not clear. Consider rewriting.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 10461, 2013.