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Interactive comment on "Shallow water table effects on water, sediment and pesticide transport in vegetative filter strips: Part B. model coupling, application, factor importance and uncertainty" *by* Claire Lauvernet and Rafael Muñoz-Carpena

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Dear authors,

I think this paper is of good quality and that it complements its companion paper (HESS-2017-405) well. However, I have a few comments (see below).

General comments:

As already pointed out by Marnik Vanclooster in his comment, the Morris sensitivity

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Discussion paper

screening and eFAST are not really complementary methods: Both methods produce indices with similar interpretation, as you state yourselves: "Interestingly, the Morris indices (μ^*, σ) have been found to provide a good approximation to the eFAST indices (STi, STi-Si) at a much lower computational cost (Saltelli et al. 2004, Campolongo et al. 2007) making it ideal for large and computationally expensive models." The main advantage of the Morris sensitivity screening is that is computationally much more efficient than eFAST, especially if one has a large number of input factors. Usually, Morris and a true variance-based method such as eFAST are used in sequence: First, a Morris sensitivity screening is performed to identify and eliminate non-sensitive input factors. In the second step, eFAST is run with a reduced set of input factors (i.e. without the ones identified with Morris as non-sensitive). This two-step approach makes sense because most modellers do not have the computational resources available for a "brute force" approach. If, however, after the Morris sensitivity screening one runs eFAST with the same number of input factors as before, the Morris sensitivity screening becomes obsolete.

As a justification of using the same sets of input factors for both Morris and eFAST, you wrote "In this study, both methods were run with the full set of inputs as a check for the consistency of the GSA results." If you want to keep both sensitivity analyses in the paper, maybe you should expand this further. However, this would draw the focus from the new VFSMOD version with shallow water table. Maybe an in-depth comparison of Morris and eFAST results should be the subject of another paper?

Specific comments:

Materials and Methods

(1) p. 5, l. 130: equation 4: deltaRO is defined as the "change of cumulative excess rainfall" within a given time step. Maybe this quantity would be easier understandable if referred to as "surface runoff volume within a given time step"? In general, I think that "surface runoff" would be easier to understand than "excess rainfall" (which is a sort of

ambiguous term). (2) p. 5, l. 140: equation 8: As pointed out by Marnik Vanclooster in his comments on the companion paper (part A), the infiltration rate f cannot be a function of the vertical coordinate z. (3) p. 6, l. 149: “phytosanitary products”: This term is very French. In English one would say “crop protection products”. (4) p. 6, l. 151: Please mention the soil type according to WRB or FAO, if available. (5) p. 6, l. 154: Feel free to mention that the Jaillière site is also the basis of the regulatory scenario FOCUSsw D5 (La Jaillière) (6) p. 6, l. 163: “on Morcille”, “on Jaillière” It should be “at”, not “on” (7) p. 6, l. 1746: “complementary”: In what sense are the two methods complementary? (cf. above) (8) p. 6, l. 177: “a variance-based method is computed”: either “variance-based measures are computed” or “a variance-based method is run/applied” (9) p. 6, l. 190 f.: You mention only three groups. Would a fourth group (low mu* and high sigma) be mathematically possible? (cf. comment by Marnik Vanclooster)

Results

(10) p. 9, l. 245: “end-vertical boundary condition”: you mean bottom boundary condition? (11) p. 10, l. 279: “average soluble properties”: better “average sorption properties”. In practice, the distribution between water and soil or water and sediment is usually governed only by the adsorption parameters (Kf and nf for Freundlich adsorption, Kd for linear sorption) and not by water solubility, since concentrations in solution usually stay way below solubility limits. (12) p. 10, l. 291: “sub-saturates”: What do you mean by that? (13) p. 10, l. 294: “This is exacerbated with WT”: There is something missing here before WT. (14) p. 11, l. 326: What is a “formal” uncertainty analysis? Can you explain briefly how you did the uncertainty analysis (probably better in the methods section)? (15) p. 11, l. 335: "Diflufenicanil" –> diflufenican (16) p. 11, l. 336: “Reduction . . . higher than the other two pesticides”: There is something missing here. Maybe: “higher than for the other two pesticides” or “higher than reduction of the other two pesticides”

Summary and Conclusions

(17) p. 13, l. 369: “rivers drainage networks”: “river drainage networks”
(18) p. 13, l. 375 ff.: Just for information: The GERDA software package which was developed for the German EPA (UBA) as a future regulatory tool for surface water does include VFSMOD simulations with a shallow water table where present. However, while the final report of the GERDA project has been published recently (https://www.umweltbundesamt.de/publikationen/bewertung-des-eintrags-von-pflanzenschutzmitteln-in), the GERDA tool itself is not publicly available yet.

References:

(19) p. 18, l. 542: “UIPAC”: This reference seems to be wrong. First, it should be IUPAC. Second, the link http://sitem.herts.ac.uk/aeru/iupac/index.htm is just a mirror of the original PPDB homepage (http://sitem.herts.ac.uk/aeru/ppdb/en/index.htm), and both pages and the PPDB are maintained by AERU, University of Hertfordshire, UK

Figures:

(20) Figure 7: The legend needs clarification. I presume that the grey bars denote Si, the black bars (STi – Si) and the sum of both bars STi? (21) Figure 8: “Uncertainty analysis results”: Some further explanations needed. It seems that these are probability density functions?

Best regards,

Stefan Reichenberger

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