

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
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Comment on hess-2021-477

Anonymous Referee #1

Referee comment on "Probabilistic modelling of the inherent field-level pesticide pollution risk in a small drinking water catchment using spatial Bayesian belief networks" by Mads Troldborg et al., Hydrol. Earth Syst. Sci. Discuss.,
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This manuscript presents an interesting development of an hybrid Bayesian Belief nNetwork, applied to the contamination risk of surface water by pesticides. This presents an interesting example of decision support tool with an implementation of a mix of different management options.

Though it is well written and nice to read, I have some concerns that need to be addressed before this manuscript gets published:

Major concerns:

- P7 l184-186: have you checked for time tendencies in climatology data. The mean values observed over the 1894-2019 period might be different from the 2016-2019 climate period on which the concentrations are observed.
- You describe your dataset as sparse. Could you please tell more about it, as it appears that the catchment on which you are working is rather well documented with many

different sources of data.

- P10-13: the units in bracket along the text are not SI unit. Could you please check that and make the necessary changes, coherently with units used in Appendix A.

- P10/11: there is an incoherence between equation (3) and C_{sw} unit in $\mu\text{g.L}^{-1}$. According to equation (3) which refers to fluxes, C_{sw} should be a quantity per volume per year. Please check carefully your formulas and units to make it coherent.

- I have a concern about the way C_{sw} is computed. Why did you divide the fluxes by the total volume of the reservoir. Doing so, you consider that a field is at risk only if its contribution to the reservoir contamination is enough to make it higher than the standard drinking limitation. This appears rather limiting the potential risk. And what about the sum of fields contribution to assess the catchment risk?

- P15/416-417: what volume did you use as a d_{mix} for the overland flow flux (comparatively to eq 11)?

- Have you performed any inference according to observed concentrations data. If so, please give details about it, if not, please state clearly your model remains an expert based model.

- The results/discussion sections are merged. This could be acceptable, but the subsections 3.2/3.3/3.4 are only results with no confrontation to literature or prior hypothesis. This would be great to add discussion elements to these paragraphs or to move them to an independent result section.

- P16 l456-458 and Section 3.4: have you simulated 10.000 results for each field? It is unclear here. And once again, I have a concern with the direct comparison of C_{sw} and concentrations as concentrations are the sums of all field contributions. Furthermore, it might be interesting to also consider / discuss the fact that ground fluxes are probably slower than surface ones.

Minor comments and typos:

P5 l142: why is fluopyram underlined?

P11 l320: check reference brackets

P15 l419: typo on 'asses'

P16 l471: Piffady (2020) approach was clearly not mechanistic. This would be more helpful to cite here an example of mechanistic approach.

P24 l578-580: a reference would be needed.

P24 I581: typo on 'metabrouron'. Please check metabromuron spelling through the document, you sometimes write it with an h, sometimes without.

P25 I584: please precise which figure you refer to.

P29 I666: '..' typo

P32 actual evapotranspiration: there are overlapping boundaries between low and medium classes

P35 'groundwater fluxes': I would expect an 'f-leach' factor in the Leach formula, coherently with eq (1).

P36 'surface water risk' : according to my units checking, there should be a time factor in the unit (see major concerns).