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Reply on RC2

Mads Troldborg et al.

Author 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., https://doi.org/10.5194/hess-2021-477-AC2, 2021

RESPONSE TO REVIEWER 2 COMMENTS

This study presents an innovative and interesting work on probabilistic modelling of pesticide pollution risk in Jersey Island. The manuscript was well written and in my opinion, this manuscript can be accepted after a few minor corrections.

We thank the reviewer for the positive feedback and constructive comments. We have replied to each of the comments below and highlighted any proposed changes to the manuscript text using *italic*.

Specific Comments:

R2.1 - L125, Page 5: "µg l-1" should be replaced as "µg L-1". Same corrections need to be done for Table 1.
We will change the symbol for litre from l to L consistently throughout the revised manuscript.

R2.2 - Table 1: The author has provided the mean concentrations for five pesticides along with the min and max limit. However, standard deviation needs to be included for each of the pesticides to make the table more statistically robust.
We will revise Table 1 to also include standard deviation of the pesticide concentration data. We propose to revise Table 1 and table caption as shown in the attached supplement file.

R2.3 - L140, Page 5: Remove the underline from "fluopyram".
Thank you for spotting this error, we will correct it.

R2.4 - Since, fluopyram was considered for the final model instead of ethoprophos, concentration, and detection level of fluopyram need to be included in table 1.
There are no monitoring data available for fluopyram in the catchment, as fluopyram is

currently not widely used in the study catchment. As explained in P5 L141, fluopyram was included in the model as a potential future replacement of ethoprophos. Ethoprophos has previously been widely used in the catchment, but it has now been banned and its use is therefore discontinued. This will be clarified in the revised manuscript as follows:

'Five active pesticide ingredients currently or recently in use in the catchment showed evidence of significant concentrations in the reservoir offtake for the drinking water supply. These included the herbicides glyphosate, metobromuron, pendimethalin and prosulfocarb, and the nematicide and insecticide ethoprophos. Metobromuron was most frequently observed above the drinking water standard, followed by ethoprophos, prosulfocarb and pendimethalin (Table 1). Ethoprophos was not included in the final model, as its use has now been discontinued. *In consultation with Jersey Water, it was instead decided to include the nematicide fluopyram as it was considered to be a likely replacement for ethoprophos and hence a potential future risk to the reservoir water quality. Monitoring data for fluopyram are not available as it is currently not widely used in the catchment. However, fluopyram is known to be used at lower application rates than ethoprophos, making its potential risk of contaminating the water supply intrinsically lower, notwithstanding its relatively high mobility and greater persistence.'*

R2.5 Page 10, Equation 3: The author has mentioned, "combined pesticide flux" which was converted to surface water concentration to evaluate the risk. However, there is some inconsistency in the units. Not clear. please check equation 3.
The reviewer is correct, the units in Eq. 3 are inconsistent. The reason for this inconsistency was that we were referring to pesticide application rates or fluxes (i.e., mass per area per time), when in fact we were actually considering the amount of pesticide mass applied per area. We will change the term flux to load throughout the revised manuscript and make it clear that we refer to mass per area, as also outlined in reply to **R1.4**.

R2.6 - L415, Page 15: Is there any specific volume for the overland flow flux?
To assess whether a calculated overland flow load should be classified as low, medium or high, we used the same discretisation boundaries as we used for the groundwater load. We did this purely to allow a comparison of the relative contribution of the two components to the combined risk. Defining the discretisation boundaries for the groundwater load was based on an assumption that the pesticide load Lgw reaching the groundwater was mixed in the upper dmix=0.1 m of the groundwater aquifer; the resulting concentration (Cgw = Lgw/dmix) can then be compared to regulatory standards based on which the discretisation boundaries were defined. It is therefore relatively straightforward to define the boundaries for the groundwater load; all that is needed is a mixing depth assumption. It is harder to do the same for the overland flow load, as it is not easy to define the depth (or volume) that the load (or mass) should be mixed in. We therefore opted for using the same boundaries as for groundwater load. Essentially this means the mixing volume is the same for the overland flow load as for the groundwater load. To clarify this, we propose to add the following at the end of section 2.5.2:
'*It is not straightforward to assess when the calculated overland flow load from a given field to the reservoir should be considered high, medium or low, as it is not easy to define the water depth (or volume) that this pesticide load (or mass) should be mixed in. For simplicity, the overland flow load was therefore evaluated similarly to the leaching risk (Eq. 11) to allow a comparison of the relative contribution of the two components to the combined risk.'*

R2.7 - ð□□»(ð□□□) = −Σð□□□(ð□□□) log2(P(X)) should be equation 16?
We will follow this suggestion and name the entropy equation as equation 16.

R2.8: Author has mentioned the soil properties. It will be great to have that information in a tabular format. Specifically the soil organic carbon concentration and pH data.
Thank you for the comment. All the soil properties used in the model (bulk density, field capacity, organic carbon, and hydraulic conductivity) are tabulated in Appendix A. These data were all taken from the HYPRES data base. There are only limited soil property data (pH and SOM) available in the VDLM catchment and we decided to summarise these in the text (page 9) rather than in a tabular format given these data were not used in the modelling in the end. As stated in the text on line 239 of the original manuscript, we had topsoil SOM data for 40 out of the 200 fields, and pH for 37 fields. We considered using the median or mean SOM per hydrogeological unit and per crop to extrapolate to all fields, however we had no observed SOM data for fields on blown sand. Therefore, for consistency, we decided to use data from the HYPRES database, as this also provided SOM estimates for the lower soil horizons as well other soil parameters (bulk density, field capacity and hydraulic conductivity) needed for the model. For comparison, we will include summary statistics (mean, min, max) for the observed SOM data for loess and shale in the text on line 241:

'*Mean observed SOM for loess was 2.5% (1.9-3.8%) and 2.3% for shale (1.9-3.4%)*.' .

R2.9 - Page 25: "particularly in the groundwater leaching risk (Figure ??"): Please mention the figure number.
Thank you for spotting the missing figure number. This will be amended in the revised manuscript as Figure 9.

R2.10: There are quite a number of typographical errors. Please review the manuscript carefully.
We apologise for typographical errors and will carefully review the manuscript to remove any errors from the revision.

Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2021-477/hess-2021-477-AC2-supplement.pdf