

Interactive comment on “Spatially-distributed influence of agro-environmental factors governing nitrate fate and transport in an irrigated stream-aquifer system” by R. T. Bailey et al.

Anonymous Referee #2

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In the submitted manuscript Bailey et al. present a multi variate sensitivity analysis on a distributed reactive solute transport model (MODFLOW with the UZF1 unsaturated zone package). Looking at NO₃ concentration in groundwater, NO₃ leaching from the unsaturated zone to the saturated zone, and NO₃ mass loading they evaluate the importance of 9 governing model parameters that control fertilizer loading, crop N uptake, and NO₃ concentration of applied irrigation water and canal seepage, litter and humus organic N decomposition, nitrification of NH₄ to NO₃, heterotrophic and autotrophic reduction of NO₃. Using a modified version of Morris' sensitivity analysis they show that fertilizer loading, crop N uptake, and heterotrophic denitrification control NO₃

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mass transport. They recommend using their results for improved sampling schemes and model calibration.

The study is novel and its goals are definitely of high relevance for HESS readers. However, in its present state it cannot be published yet. Major revisions are necessary in terms manuscript structure and study layout.

Manuscript structure:

Study site description is too long. The same is true for the model description. Methods need an overview and more focus. It is not clear why this particular set of methods is used and why more focus is set on particular parts of the results. The results and discussion section is hard to read because there are too many parameter references/acronyms in the text. The authors should find a more elegant way of presenting their results. There are some interpretations and conclusions but a real discussion is missing. There is now single reference in this section. Please add a real discussion (also to add more reliability on the results, see below).

Study layout:

The way the parameter sets are created for Morris' method is rather unusual. In typical applications realistic parameter ranges are chosen before the analysis and a (uniform) random sampling is performed (Pianosi and Wagener, 2015; Saltelli et al., 2008). Here, the parameters are varied locally around their "base values" that were found in Bailey et al. (2014) and sampled from a (log)Gaussian distribution (as far as I understand the sampling scheme). There is no elaboration, why this particular way is chosen and how it will affect the interpretations compared to a more typical application of Morris. Considering this modified parameter sampling the term "global" sensitivity analysis appears not adequate and I recommend changing it to "regional" or "local".

There is no evaluation neither of the model nor of the SA results. Neither by field observations nor by comparison to other studies. This decreases the plausibility/realism

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of the study and I strongly recommend adding / mentioning all information that gives reason to believe that the simulations are actually representing reality. The complete lack of references in the discussion adds to this point of criticism.

Some more comments are provided in the supplement.

References

Pianosi, F. and Wagener, T.: A simple and efficient method for global sensitivity analysis based on cumulative distribution functions, *Environ. Model. Softw.*, 67, 1–11, doi:10.1016/j.envsoft.2015.01.004, 2015. Saltelli, A., Ratto, M., Andres, T., Campolongo, F., Cariboni, J., Gatelli, D., Saisana, M. and Tarantola, S.: *Global sensitivity analysis: the primer*, John Wiley & Sons., 2008.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/12/C2945/2015/hessd-12-C2945-2015-supplement.pdf>

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