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Hydrology and Earth System Sciences
Discussions

Interactive comment on "An integrated water system model considering hydrological and biogeochemical processes at basin scale: model construction and application" *by* Y. Y. Zhang et al.

C. Stamm (Editor)

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Below I add a few comments. They complement what the two referees have already pointed out.

Major issues:

Catchment description: Although it is mentioned that data on the human population and livestock production is used to parameterise the model (p. 9233, L. 7), no such numbers are found in the manuscript. I suggest that you include a table summarising these descriptors for the different sub-catchments. The agronomic

descriptors should also be improved by providing more details. For example, dry land amounting to 84% of the watershed (p. 9234, L. 21) should be specified in more detail: what kind of crops etc. are included? How have the crops be considered in the model?

Urban hydrology and emission model: There is no description of the urban hydrology and the related fluxes of nutrients like NH_4. Which sources and flow paths have been considered (e.g., wastewater treatment plants, leaking septic tanks etc.)? How has this model part be parameterised?

Livestock model: In analogy to the urban part, there is no description of modeling the nutrient fluxes from livestock production. If this is considered irrelevant this should be mentioned (see 9233, L. 7).

Nitrate leaching: According to p. 9231 L. 4 - 7, Fig. 4, and p. 9256, L. 3 nitrate leaching seems not to be included into the model. It looks like that only surface runoff is considered for transport to water bodies. If this impression is wrong, please modify the manuscript such that the relevance of the different flow paths get evident. If nitrate leaching was indeed excluded, provide arguments why this should be an appropriate simplification in your study region. Make it clear that this was a relevant model limitation for other situations.

Minor issues:

Abstract: The model improvement and comparison with SWAT is not introduced as a goal, but results are shown. Please make it clear what the objectives are. The same holds for example for grain yield, which appears out of the blue (p. 9221, L. 18).

Introduction: Make it more explicit why you consider current models as insufficient for an integrated watershed management. As it is written now it appears as a claim

without much factual support. Once you have pointed out existing deficits you should make clear which of them you will try to tackle with your approach.

p. 9222, L. 2 - 11: This paragraph evokes the impression that water problems are the prime cause of non-sustainable development and that integrated watershed models will solve this problem. I think the wording in this paragraph (and elsewhere in the manuscript) should be chosen such as to be realistic about the relevance of water issues and the tools used for water management.

p. 9222, L. 26: This is not really new.

p. 9223, L. 13: The nomenclature is misleading: the environment comprises the hydrological cycle as well as ecological aspects. Hence, the three terms cannot be used to denote distinct things. Please improve on the selected terms.

p. 9225, L. 6 - 9: Based on what do have these expectations?

p. 9225, L. 6: Why is calibration a post-processing tool?

p. 9228, L. 13: This is rather conceptual and not process based.

p. 9228, L. 5: How do you link the site scale with the catchment scale?

p. 9239, L. 5: it should read *phenological*.

p. 9232, L. 8: Is *no regulation* a calculation method?

p. 9233, L. 7: Why do you need GDP, orchard area etc.? How does it feed into the simulations?

p. 9233, L. 13: How do you test the crop yield at field scale with catchment data?

p. 9233, L. 22: What is a *minimum simulation cell*?

p. 9235, L. 4: Which socio-economic data is point data that has to be interpolated?

p. 9239, L. L. 15 - 16: This sentence is not clear to me.

p. 9239, L. 21: Should *yield rates* not be replaced by *loads*?

p. 9239, L. 24: I have some problems with these correlations. First, it seems that you correlate NH_4 loads calculated for a unit area with the total area of paddy fields in a sub-catchment (Fig. 10). Why should the intensive quantity load per area correlate with the extensive quantity area of a specific crop? Second, you mention a strong correlation between the NH_4 load and rice yield (Fig. 10). However, this correlation disappears if you remove the largest value. In fact, one observes a negative correlation without this data point. Please, be more cautious in presenting the results.

p. 9239, L. 26 - 27: Under intensive cropping, nitrogen use efficiency (NUE) is much lower than the values you indicate here. Globally, NUE of cereal production (including rice) is estimated to be in the order of 33% (see e.g., Meng et.al., 2014; Raun et.al., 1999). Please provide convincing data supporting the exceptionally high NUE values. Otherwise, check whether the simulated N balances are correct.

p. 92450, L. 6, 13 - 17: How are they simulated? What kind of data is used for calibration?

p. 9241, L. 1: Provide evidence for this claim!

p. 9242, L. 12, Fig. 11: Here, the same comments holds as for Fig. 10. If one removes the largest value from the data set the correlation disappears. Hence, it seems that your statement regarding the good performance of the yield simulation is not very robust. Please comment.

p. 9242, L. 13 - 14: I could not find the data for the urban fluxes.

Conclusion: Please respond to all of the comments and explain how you intend to revise the manuscript in order to address the issues raised by the reviews.

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References

Meng, F., J.E. Olesen, X. Sun, and W. Wu. 2014. Inorganic nitrogen leaching from organic and conventional rice production on a newly claimed Calciustoll in Central Asia. PLoS ONE 9:e98138. doi:10.1371/journal.pone.0098138.

Raun, W.R., and G.V. Johnson. 1999. Improving nitrogen use efficiency for cereal production. Agronomy Journal 91:357 - 363.