

Interactive comment on “Forecasting land-use change and its impact on the groundwater system of the Kleine Nete catchment, Belgium” by J. Dams et al.

Anonymous Referee #1

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Forecasting land-use change and its impact on the groundwater system of the Kleine Nete catchment, Belgium

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General comments: The paper addresses the issue of changes in groundwater quantity due to land use change coupling a land use change, water balance and groundwater model. Sustaining river flows and water resources will be dependent on catchment

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groundwater reservoirs. Thus, this study provides insights which can aid management responses to climate change.

First of all, I would like to acknowledge the work and efforts the authors put into the analyses and modelling applications, which form the basis for this paper and writing this manuscript. The paper for the most part is clearly written and well structured and is acceptable for publication in the journal after revision.

My main point of concern is that in my point of view there is only a limited consideration of recent advances in experimental field and modelling work to improve our understanding about such possible impacts in the quality AND quantity of groundwater due to environmental and / or climate change. Obviously, this is a modelling study. However, at least within the introduction and discussion section it should be made clear that there is a clear awareness about these recent developments in experimental design.

In addition, I find a critical discussion of model application issues totally missing and this should be expanded. Such issues are, again, recently widely published, i.e. the issue of parameter and process equifinality, parameter estimation, uncertainty (which is attempted to deal with by calculating 4 different scenarios). I find this a particular limitation in such a modelling paper where 3 different model types, i.e. land use, water balance and GW model, are applied and each of these types is linked to particular problems which need to be considered and critically discussed.

Thirdly, not all hydrological terminology is used in specific way and terms as runoff, discharge, recharge seemed to be mixed up in large parts of the result section. Please correct and be specific.

Fourthly, the discussion of results is much too limited and needs expansion! The authors state for themselves on p. 4279, L. 26 that some caution should be considered evaluating these results. This needs to be discussed in length considering recently published work on this issue.

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Specific comments: Introduction 1. p. 4276, L. 10-114: I am not sure whether the original landcover of Western Europe should be used as a reference condition in this context. In todays discussion about environmental change and possible impacts of climate change more recent reference conditions (e.g. pre-industrialisation or similar) might be a more reasonable approach.

2. p. 4276, L. 21-22: you state that nowadays the use of distributed models offers increasing opportunities. But, obviously due to data constraints (or other reasons??) you use a 50m raster and thus, can not use the full potential of these opportunities. This (e.g. the problem of considering spatial variability, physically meaningful spatial delineation, value of distributed hydrological models and how can we deal with uncertainty, equifinality etc within such model applications etc.) is just one example (further to main general comments above) which needs to be much more critically discussed and considered.

3. p. 4269, it is not clear to me what the difference of this paper is to the cited studies of Batelaan and De Smedt (2001) and Batelaan et al. (2003) is. What is the clear contribution of this paper in comparison to previous approach? Be more clear and specific in which way the new modelling approach is new and why it is important.

4. p. 4269: Formulate your objectives more clearly.

Methodology

5. p. 4270, L. 4-7: choice for spatial resolution: it is not clear to me why this is not possible with a 10m resolution.

6. p. 4270, L. 4-7: in addition to point 5, at this point is also a clear discussion necessary about spatial delineation of modelling units in a physically meaningful way (advantages / disadvantages of different methods to delineate the catchment in question spatially)

7. Do you refer to figure 1 somewhere?

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8. p. 4273, L. 14: I am not sure whether just using these 3 measurements of model goodness-of-fit are sufficient enough. Particularly within this study where different types of models were applied a number of different model evaluation parameters should be used to capture the whole spectrum of model results (and thus, whole spectrum of possible modelling errors). Could you please expand on that?

9. p. 4273, L. 14: mean absolute error: If you give an error of 0.41 m how can you state as one of your results that there were changes in groundwater levels between 0.025 m and 0.009 m?? Same for root mean square error.

Study area 10. p. 4274, L. 16: loamy sand etc. is not a soil type. Please be specific and give soil type (if possible international classification, e.g. FAO). This is important to be able to assess any groundwater recharge etc.

Results and discussion 11. p. 4275, L. 14 and following: what do you understand as runoff? Hortonian overland flow? Total discharge?

12. 4276, L. 6: similar to point 11: what do you understand as discharge? What are discharge areas? Be specific in all your terminology throughout the whole paper and clearly define your terms. Discharge is the volume of water flowing through a river!!

13. 4276, L. 11: why should discharge be excluded? Do you mean surface runoff here?

14. whole result section: discussion of actual results need to be expanded.

Conclusion 15. p. 4279-4280: Such a simple conclusion is not sufficient for such a controversially discussed topic (environmental and climate change and model application to investigate possible impacts of those).

16. p. 4279, L. 11-12: what is not exactly the advantage of coupling CLUE-S, WetSpa and MODFLOW?

17. p. 4279, L. 20: I do not fully agree with this statement that land use change

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models are valuable tools to assess the hydrological impact of land use change. You are correct they provide valuable information about possible changes.

18. p. 4279, L. 26: exactly, but not just SOME caution should be considered evaluating these results. This needs to be discussed in length considering recently published work on this issue.

TABLES TABLE 2: please give precipitation information to be able to assess the results better

1) Does the paper address relevant scientific questions within the scope of HESS? YES

2) Does the paper present novel concepts, ideas, tools, or data? NEEDS EXPANSION

3) Are substantial conclusions reached? NO

4) Are the scientific methods and assumptions valid and clearly outlined? YES, SEE ALSO SPECIFIC COMMENTS

5) Are the results sufficient to support the interpretations and conclusions? NOT FULLY, SEE SPECIFIC COMMENTS

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? YES

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES

8) Does the title clearly reflect the contents of the paper? YES

9) Does the abstract provide a concise and complete summary? SEE COMMENTS

10) Is the overall presentation well structured and clear? YES

11) Is the language fluent and precise? YES

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12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES, BUT TERMINOLOGY IN PARTS UNCLEAR

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? SEE COMMENTS

14) Are the number and quality of references appropriate? SEE COMMENTS

15) Is the amount and quality of supplementary material appropriate? YES

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 4265, 2007.

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