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Hydrology and Earth System Sciences Discussions

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

J. Dams et al.

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RESPONSE TO THE GENERAL COMMENTS of anonymous referee #1

Comment 1: In the introduction and discussion section there should be an overview of the recent advances in experimental field and modelling work to improve understanding of possible impacts in quality and quantity of groundwater due environmental and climate change.

Authors response to comment 1: Taking into account this comment the introduction has been considerably extended discussing more in detail recent advances in groundwater research. Research investigating the impact of environmental and climate change

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Interactive Discussion

Discussion Paper

on the groundwater system as well as the coupling of land-use change models with hydrological models is discussed.

Comment 2: 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.

Authors response to comment 2: A section (4.5) addressing the modeling uncertainties faced in this paper has been added in the discussion part and we believe that this resolves the here raised point. The uncertainties of the three separate models are extensively discussed in this paragraph, along with the model validation results.

Comment 3: 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.

Authors response to comment 3: Indeed in the original version some misunderstanding might have occurred due to changing terminology. We now use a consistent terminology: Runoff is taken as the part of the net-rainfall that flows overland to surface streams, rivers and lakes. Groundwater discharge is referred to as groundwater flowing upwards towards the soil-surface. Groundwater recharge is defined as in Freeze (1969): the entry into the saturated zone of water made available at the water table surface, together with the associated flow away from the water table within the saturated zone.

In the original text the word groundwater was sometimes not mentioned when groundwater discharge was meant, this has been corrected.

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Comment 4: 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.

Authors response to comment 4: The results are in this adapted manuscript more extensively discussed, especially with respect to the uncertainty (section 4.5 and rephrased in the conclusions). As it is the case for all environmental models, also the model results discussed in this paper suffer from uncertainty. The approach followed here to decrease the uncertainty in the future land-use was by using a model that is able to predict future land-use changes instead of using artificially defined changes. Accurate model build-up and calibration in this paper, results in relatively good validation results. Consequently, using the land-use prediction methodology decreases the uncertainty of the impact of the future land-use change on the recharge, groundwater level and baseflow. Therefore, we are confident that the results can serve as indicative trends for future land-use changes on the groundwater system.

RESPONSE TO THE SPECIFIC COMMENTS of anonymous referee #1

Introduction

Comment 6: 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.

Authors response to comment 6: The authors agree with this comment, the referred part of the introduction is modified according to the suggestions.

Comment 7: 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

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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.

Authors response to comment 7: As distributed models are gaining popularity amongst hydrological modelers, there is a need for optimization of the modeling techniques especially with respect to model parameterization, calibration and validation. However, simulations in this paper assume the validity of current model practices regarding land-use change, water balance and groundwater flow and focuses explicitly on the amelioration of the prediction of the future land-use to incorporate in modeling the future state of groundwater system. It is in our believe that discussing general problems related to distributed hydrological modeling is out of the scope of this paper, it does not contribute to the goals of the research and would confuse readers. However, we have extended the specific discussion with respect to the uncertainty of the model results.

Comment 8: 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.

Authors response to comment 8: From this question we understand that the objectives of the paper were not clearly described. The part of the introduction regarding the objectives was therefore improved. The difference between the approach used in this paper and the approach used by Batelaan and De Smedt (2001) and Batelaan et al. (2003) is the incorporation of the land-use change model. The predictions of the land-use models allow us to estimate the spatially distributed effect of the land-use changes. From the modeling results it can be seen where problems will arise in the near future, for example at places with a groundwater dependable fauna and flora that coincide with locations with a drop in groundwater level.

Comment 9: p. 4269: Formulate your objectives more clearly.

Authors response to comment 9: This has been changed.

Methodology

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

Authors response to comment 10: In theory a spatial resolution of 10 meters is perfectly possible. However, the main problem is data availability at this resolution. Available GIS data for the Nete basin had a resolution of 50 meters, corresponding to the accuracy of the information content of the data layers. A higher resolution is technically possible by way of resampling the data, however it gives a false impression of accuracy, which is not supported by the data content. Besides, the data availability the WetSpass model is extensively tested on 50 meters resolution data and not for smaller resolutions.

Comment 11: 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)

Authors response to comment 11: In this paper a physically grid-based distributed model is used. The resolution of the data layers has been carefully selected on basis of the in point 10 described arguments. The catchment delineation is defined in the study of Woldeamlak (2007), who developed a groundwater model for the area and carefully delineated the model area on basis of a high resolution DEM and groundwater data. A discussion of this delineation is out of the scope of this paper and does not contribute to a clear message for the readers.

Comment 12: Do you refer to figure 1 somewhere?

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Authors response to comment 12: A reference to Fig. 1 is added.

Comment 13: 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?

Authors response to comment 13: Indeed the three measurement of goodness-of-fit do only relate to the current (2000) simulation of the WetSpass-MODFLOW model combination, not incorporating the uncertainty of future land-use changes. In new added section concerning uncertainty assessment, the validity of the land-use change modeling is discussed and a broader overview of the whole spectrum of model uncertainty is given.

Comment 14: 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.

Authors response to comment 14: On basin scale, it is difficult to obtain mean absolute errors and root mean square errors lower than the 0.41 m and 0.51 m as calculated in this case study. These uncertainty values are largely due to the differences between measured and simulated groundwater levels at locations where the groundwater is relatively deep e.g. 20 to 40 meters. At those locations the interaction of the groundwater table with the surface hydrological process is of no importance. At locations where the groundwater table is shallow the error is much lower. Moreover, the simulated changes are averages of different locations, which indicate a particular trend, whereby the absolute level is of much less importance than the relative direction of the changes.

Study area

Comment 15: p. 4274, L. 16: loamy sand etc. is not a soil type. Please be specific and

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give soil type (if possible international classification, e.g. FAO). This is important to be able to assess any groundwater recharge etc.

Authors response to comment 15: Indeed the WetSpass model uses soil textures instead of soil types. The mistake is changed in the text. As large areas in the basin are used for agriculture the original soil type is mostly destroyed. Some sandy areas still show Podzol soils.

Results and discussion

Comment 16: 16 p. 4275, L. 14 and following: what do you understand as runoff? Hortonian overland flow? Total discharge?

Authors response to comment 16: The runoff values mentioned p. 4275, L. 14 refer to the total runoff (over land flow) on pixel scale. Runoff in the WetSpass model is caused by both Hortonian overland flow as well as from saturated zone overland flow.

Comment 17: p. 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!!

Authors response to comment 17: The discharge areas mentioned here refer to groundwater discharge areas: areas where groundwater is being forced out of the groundwater system towards the land surface. To be clearer in the text we use now everywhere the term ‘groundwater discharge’.

Comment 18: 4276, L. 11: why should discharge be excluded? Do you mean surface runoff here?

Authors response to comment 18: As in the previous remark also this discharge refers to groundwater discharge. By excluding the groundwater discharge we are only looking at the groundwater recharge instead of the groundwater budget.

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Comment 19: The whole result section: discussion of actual results need to be expanded.

Authors response to comment 19: The whole results and discussion section has been considerable modified and expanded. We believe that the present discussion reflects much better the value of the research results.

Conclusion

Comment 20: 2p. 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).

Authors response to comment 20: The conclusion is adapted, we believe we rephrased some of the conclusions of the research in a meaningful and well balanced manner. In order to keep the text as clear as possible only the most important conclusions are given in the concluding section. A more detailed discussion of the modeling results is given in the results and discussion section.

Comment 21: p. 4279, L. 11-12: what is no exactly the advantage of coupling CLUE-S, WetSpass and MODFLOW?

Authors response to comment 21: The MODFLOW package is widely used for simulating groundwater flow. However, MODFLOW only models the saturated zone and as such requires the groundwater recharge as an input. In order to quantify the effect of land-use changes on the groundwater recharge a coupling between the surface and the sub-surface water balances is required. The WetSpass model is able to model the surface water balance and the resulting recharge information can be used as input for the MODFLOW. In this way it becomes possible to simulate effects as for example land-use change on the groundwater system. The advantage of incorporating the CLUE-S model is that we can allocate land-use changes based on a statistical relation of the current land-use and some of its physical properties. Following this approach it

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becomes possible to allocate some of the possible problem zones, with regard to the groundwater system, in the future. We added in the conclusion a sentence describing the advantage of the coupled models.

Comment 22: p. 4279, L. 20: I do not fully agree with this statement that land use change models are valuable tools to assess the hydrological impact of land use change. You are correct they provide valuable information about possible changes.

Authors response to comment 22: This is indeed true. To assess for example the hydrological impact of historical land-use changes, land-use change models are of minor use. The sentence was changed to: The novelty of this study is the successful coupling of a land-use change allocation model (CLUE-S) with a groundwater flow model, which allows estimating the range and spatial distribution of the effect of future land-use changes on the groundwater system.

Comment 23: 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.

Authors response to comment 23: An additional section dealing with the uncertainty is added.

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

Authors response to this extra comment: The precipitation is added to the table.

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

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