Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-297-RC2, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Can integrative catchment management mitigate future water quality issues caused by climate change and socio-economic development?" *by* Mark Honti et al.

Anonymous Referee #2

Received and published: 5 November 2016

This paper describes the application of an integrated-water-resources-management model to a small catchment in the Swiss Plateau in order to analyze potential effects of different drivers on future water quality. The authors have chosen a question as title ("Can integrative catchment management mitigate future water quality issues caused by climate change and socio-economic development?"), but I am not sure whether they really answer it. It appears that some aspects of current practice (e.g., the application of pesticides within the catchment) are so uncertain that almost no scenario leads to significant changes, whereas other factors (in particular climate change in the study area) are simply not strong enough that a model could quantify effects. This is

somewhat frustrating – and I would actually have loved to get a clearer answer to the question posed in the title, which seems to be "no". Or maybe "not as long as we don't know what the farmers are actually applying on their fields".

I believe that the authors are stuck in a classical dilemma. They have chosen a study area with a comparably good data situation (even though the data in pesticide applications were still bad), in which not much direct climate change is expected. The temperature will rise, but won't make Northern Switzerland semi-arid. Precipitation estimates are highly uncertain and projections even don't agree on the sign of the change. Even before performing the study one could have guessed that climate change will have a smaller impact on water quality in the chosen catchment than changes in legislation regarding the use of certain pesticides. It would have been much more exciting to perform the analysis in a Mediterranean setting where much more severe climate-change effects can be expected, and maybe also more change in land use. Unfortunately, the data situation in these countries with respect to micropollutants is typically much worse than in Switzerland. Thus, while the authors' intension is a noble endeavor, their choice of application leads to some inconclusiveness.

I was not able to understand what the hydrological model does. Also the supplementary information is illusive in this regard. In particular it has been impossible for me to figure out how evapotranspiration is modeled, which in the study area most likely will more strongly be influenced by climate change than precipitation. The authors talk about three flow components (baseflow, subsurface flow, runoff), but it's not clear to me which physics are behind the separation between baseflow and subsurface flow. Neither could I see which flow component is how important. Maybe this has been described in one of the two preceding papers; but I have not read them.

The paper contains hardly any description of the catchment. However, the appropriateness of the model choice strongly depends on the geological setting and the agricultural land-use management. The authors need to explain this. My own quick research yielded: The geology is dominated by upper freshwater molasse (poorly permeable bedrock) with limited Quaternary overburden containing peat (particularly close to lake Greifensee). There is practically no groundwater body. The Mönchaltorfer Aa is connected to several drainage channels and/or tile drains, which exist essentially all over the valley. This information may justify a model concept that essentially denies explicit groundwater pathways. The infiltrating water is rapidly captured by the tile drains, so that restricting transformation processes to soil layers may be OK. However, other catchments are quite different and require an explicit treatment of groundwater flow, transport, and management. That is, integrated water resources management in the chosen catchment has the stream (and lake Greifensee) as its target, whereas in other catchments drinking-water production from groundwater is a major issue. In as much, "integrated water resources management" means different things in different catchments, requiring different model concepts.

The authors choose comparably simple descriptions for all processes, which I can understand given the difficulty of calibrating more complex models. However, if the transformation behavior of the pesticides is mainly based on calibrating simple first-order elimination models, the important influences of climate and land-use may be neglected. Some processes within the model contain an influence of air temperature (which is not identical with soil temperature). But I have not seen a potential influence of soil moisture, which may change more than precipitation in a warmer climate because of stronger evapotranspiration. Personally, I believe that including such effects will still not lead to strong climate signals in Northern-Swiss water quality. But, the best stochastic analysis does not help if the decisive dependencies are lacking in a model. Conversely, identifying the decisive dependencies requires data that cover a sufficient range in the controlling variables. In as much, I agree with some of the statements made by the authors in their discussions regarding the uncertainty caused by not identifying the decise mechanisms. While I don't have a solution either, it's not clear to me what the authors are recommending. Should we do more stochastic analysis on parameters that we can handle even though we know that the highest model uncertainty is on conceptual levels?

I am sorry that my (very much delayed) review contains remarks that are almost philosophical rather than going into specifics of the model and the application. But is appears that most of the latter was described in the two former papers. Therefore, it's also not easy to grasp what was actually done in this particular study without studying the preceding papers by the same authors.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-297, 2016.