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Discussions

Interactive comment on "Towards ecosystem accounting: a comprehensive approach to modelling multiple hydrological ecosystem services" *by* C. Duku et al.

Anonymous Referee #2

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The topic of this paper "accounting of hydrological ecosystem services" is interesting. However, the topic is a rather complex one, as it combines hydrological modelling of different processes at relatively large scales, the translation of the hydrological states and flows into hydrological ecosystem services and subsequently a meaningful way of accounting for the whole of hydrological ecosystem services. In my opinion in many parts of this research the treatment of certain processes or the calculation of hydrological ecosystem services was rather too simplified. I worry about the application of a spatially explicit model with so many parameters when in various aspects data and processes have to be so strongly simplified: can you still justify the use of such a

model?

The English is generally fine, however the structure of the article is not very good and in some cases the explanations are fuzzy and therefore not clear despite correct English / grammar. I personally find the article a bit difficult to read and would recommend the authors to work on the structure and style so that it may improve the possible impact of the article.

More specific comments: - I did not find which parameters of the model were used to calibrate the model with and how this calibration was performed, I think that is important information.

- The calibration was performed for both streamflow as well as sediment and nitrogen load, but validation only for streamflow. Based on the strongly simplified model, I expect the validation results for the sediment and nitrogen load to be very poor. It would be good to show these validation results too, as you present results from the model for these ecosystem services and it would be good to have some measure of how reliable these model outcomes are.

- the result of this simplified approach of estimating water purification based on saturation would mean that with a higher amount of saturation there is a larger denitrification, while I would expect that in case of a higher saturation in the soil there is a stronger groundwater recharge and more potential flow of pollutants to groundwater. So is denitrification really the best way to estimate the purification? And though I understand that purification is a service is it giving us the right signal? In case 80% purification takes place in a location with a high load and high flow to groundwater, there may still be much more nutrient flow to groundwater than in another location with only 20% purification but a rather low load and maybe only very low groundwater recharge. So the service is high but the problem is still remaining much stronger in the first case.

- I think the reference to Galloway et al 2003 is not very appropriate here, I would expect something more specific for the region, how is fertilizer use in this region? Is

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it really so widespread in these catchments or is it spatially very variable how much fertilizer is applied? What kind of data did you use for the nitrogen input?

- if I understand the description well, all crops except for rice are simulated with the parameters for maize and have a standard growing period of 103 days. I do understand that it is difficult to obtain spatially explicit data, but in case the spatial parameterization is not possible, can you such a fully spatially explicit model?

- I think to calculate the soil water capacity for plants as an average moisture content over the full growing period is also really quite critical. Whether the soil is middle moist for the full time or really dry at the beginning and wet at the end or the other way around has a huge influence on the yield, so I think it is not right to define the service capacity as an average moisture content over the growing period.

- In the case of soil erosion I find the definition of service capacity as the maximum potential soil loss in absence of vegetation is strange. I would expect to see the maximum potential reduction in soil loss produced by the vegetation of the area and the actual to be that which occurs under the actual soil cover by that vegetation. The sentence " It reveals the soil conservation capacity of a particular vegetation cover type." (p 3492, l 18-19) is not even true in case the capacity is calculated as the maximum soil loss in absence of vegetation cover.

- Table 3: it should be possible to read every table on itself without looking through the text so please define the abbreviations.

- Table 4: GP is only specified in the header but nowhere in the table. . .

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3477, 2015.