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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.

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REFEREE 2 We would like to thank the reviewer for his/her invaluable feedback and constructive comments. We have taken many of the comments and suggestions on board and we will incorporate them in the final paper. In the following, we provide a point by point response to the comments. For the sake of clarity we first repeat the reviewer's comments and then provide our response.

Comment 1: The topic of this paper "accounting of hydrological ecosystem services" is interesting. However, the topic is a rather complex one, as it combines hydrolog-

ical 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?

Reply 1: Many approaches have been used in modelling and quantifying hydrological ecosystem services. Some of them are specifically tailored towards quantifying hydrological ecosystem services whereas others build on existing hydrologic models (Vigerstol and Aukema, 2011). There are methodological challenges as well as advantages associated with each of them. For ecosystem accounting (which is an emerging field that provides a systematic framework to link ecosystems to economic activities (EC et al., 2013;Edens and Hein, 2013;Obst et al., 2013), one of the essential issues yet to be resolved is the incorporation of hydrological ecosystem services into ecosystem accounts for environmental management, monitoring and decision-making . This necessitates that several methodological challenges associated with modelling hydrological ecosystem services must be addressed. The challenges include appropriate decisions regarding model process inclusion, spatial heterogeneity, physical and mathematical representation, temporal resolution, and model accuracy. In order to address these issues, we considered several hydrological models out there and selected the SWAT model to be more appropriate for this purpose. The SWAT model has been tested extensively for different applications and in different landscapes and climates with satisfactory results (Gassman et al., 2007). Its wide applicability, process representation, spatial explicitness and extensibility make it a suitable tool to support modelling and accounting for hydrological ecosystem services. We understand your issue with over-parametrization, which is not limited to the SWAT model but other complex models as well. However, model complexity, process inclusion, operational feasibility and explicitness are some of the trade-offs that must be made.

Comment 2: 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.

Reply 2: We will restructure and rewrite some sections of the paper to make it clearer, concise and coherent.

Comment 3: 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.

Reply 3: We agree that calibration parameters are important information that must be included in the paper. We have provided them as a supplement to this response. We will also include this table in the final paper. Calibration of the HRU-based SWAT model was carried out using the SUFI-2 optimization algorithm in SWAT-CUP (Abbaspour et al., 2008). Streamflow calibration was first undertaken and followed by sediment and nutrient calibration respectively. These calibrated and validated parameter sets were then transferred to the grid-based setup of the SWAT Landscape model. This setup uses a spatially distributed topographic index to partition flow into landscape and channel components. The parameter that controls the flow separation is drainage density factor (Rathjens et al., 2014). This parameter was further calibrated manually in the grid-based SWAT Landscape setup.

Comment 4: The calibration was performed for 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. Reply 4: As we pointed out in p 3485 line

16 to 17, due to the absence of a long time-series of data, we were only able to calibrate the nitrogen and sediment loads. We were not able to validate this part of the model. Whereas this increases the uncertainties associated with model results for nitrogen and sediment loads, this approach is not uncommon. In the absence of a long time-series of data either only calibration or validation is undertaken (Gassman et al., 2007). In the final paper, we will include a subsection under Discussions to discuss model parameter uncertainties and model limitations.

Comment 5a: - 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?

Reply 5a: We acknowledge that denitrification as modelled by the SWAT model is simplified and we explicitly stated these model simplifications and limitations in our paper (p 3489 line 28 and p 3490 line 1 to 5). Our approach models the spatiotemporal distribution of ecohydrological conditions necessary for denitrification to occur. Namely, soil moisture content, soil temperature, presence of a carbon source and nitrate availability. Furthermore, we would like to state that the assumption that higher saturation leads to higher groundwater recharge and in turn a higher potential flow of pollutants to groundwater is not necessarily true. Denitrification controls the amount of nitrates available in the soil profile for leaching into groundwater systems (Jahangir et al., 2012). Therefore, higher denitrification rates brought on by soil saturation reduce nitrate leaching even though groundwater recharge may be high. Fine-textured soils (high clay), which are more prone to saturation and consequently denitrification, are generally less susceptible to nitrate leaching than sandy-textured soils because water permeability and hence groundwater recharge is much lower. Because of this, a key practice for reducing leaching losses is to minimize the amount of nitrate present in the soil at any given time either through denitrification or other means. For example, Kramer et al. (2006)

observed that organic farming supports more active and efficient microbial denitrifier communities leading to a considerable reduction in nitrate leaching as compared to conventional farming.

Comment 5b: 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.

Reply 5b: Ecosystem services are the contributions of ecosystems to benefits used in economic and other human activities (EC et al., 2013). The benefit may be realized directly from the biophysical outcome or may be realized only after economic and human inputs have been combined with the biophysical outcome. Therefore, using your example of 80% purification taking place in a location with a high absolute load and high absolute nitrate leaching; we argue that in such a place, denitrification removing 80% of nitrates from the system and therefore increasing the groundwater quality is a great example of an ecosystem service to the dependent population. Even though 20% leaching is high in absolute terms and therefore problematic, the 80% purification would have reduced the level of economic or human input that would otherwise have had to be added in order to make groundwater suitable for human consumption.

Comment 6: 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 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?

Reply 6: We will change the reference to Galloway et al 2003 to a more appropriate one such as Bossa et al. (2012). Fertilizer use is increasing in the region and high fertilizer inputs are associated with crops such as maize, rice and cotton (Bossa et al.,

2012). An estimated average of 100–250 kg ha-1 of fertilizer (NPK + Urea) is applied to cotton, rice and maize (Bossa et al., 2012). To simulate these high fertilizer inputs, we used the auto-fertilization routine in the SWAT model. Fertilizer (NPK and/or Urea) was applied automatically whenever the nitrogen stress fell below a specified level.

Comment 7: 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?

Reply 7: We want to clarify that crop growth parameters for maize were not used to simulate all crops. For our study area, we simulated only two crops, maize in croplands (which were referred to in our manuscript as uplands) and rice in inland valleys. For our simulations, we assumed that all croplands are used for only maize cultivation with the growing period of 103 days (which is the most common crop in our study area) This assumption was made because of the lack of spatially explicit data on the types of crops grown on agricultural fields. Rice on the other hand was grown in inland valleys.

Comment 8: 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.

Reply 8: Whereas we agree that the timing of soil water availability also influences the yield we do not agree with the assertion that defining service capacity as an average soil moisture content over the growing period is wrong. Timing of availability and actual quantities of soil moisture are all important factors that affect crop yield. The challenge then is to select a single indicator that can capture all these important factors. Even though the indicator we used aggregates soil moisture availability across the growing period, it provides a consistent basis for comparison between service capacity and service flow.

Comment 9: 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.

Reply 9: Yes, it is true that service capacity should be the maximum potential reduction in soil loss produced by the vegetation cover of the study area. This maximum potential reduction in soil loss (maximum potential soil retention) can be said to be equal to the maximum potential soil loss. The maximum potential soil loss is modelled assuming there is no vegetation cover (Leh et al., 2013;Terrado et al., 2014). For example, for a specified spatial unit, if the maximum potential soil loss in the absence of the vegetation cover is estimated as 2tons/ha/yr then it indicates that the potential reduction in soil loss due to the vegetation cover cannot be greater than 2tons/ha/yr. Therefore, the maximum potential reduction in soil loss can be said to be equal to the maximum potential soil loss. We also agree that the sentence "it reveals the soil conservation capacity of a particular vegetation cover type (p 3491, 118-119)" can be misleading. We will rewrite this whole methodological section to remove any ambiguity and to make our point clearer.

Comment 10: 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.

Reply 10: Thank you for pointing out these inconsistencies to us. We shall correct them accordingly.

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Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C2406/2015/hessd-12-C2406-2015-supplement.pdf

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