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Hydrology and Earth System Sciences Discussions

Interactive comment on "Characterization of the hydrological functioning of the Niger basin using the ISBA-TRIP model" *by* V. Pedinotti et al.

V. Pedinotti et al.

vanessa.pedinotti@gmail.com

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1. There is insufficient information included in the manuscript to fully describe the details of the modeling approach employed. For example: The inclusion of re-infiltration in flooded areas is a significant additional feature in the model which should be described in more detail.

Authors : Actually, the flooding scheme used in this study was evaluated over the Amazonian basin (Decharme et al., 2008) and was evaluated globally (Decharme et al., 2011, accepted?). In the study from 2011, it was noticed that over the Niger basin, a bias still remained between the simulation and the observations, even after the flooding scheme was added. Among other possible reasons, the author identified as a possible cause of this bias, the fact that in this particular region, deep aquifers exist that are not represented in the model. As these two articles from Decharme et al. gave a detailed description of the flooding scheme and because this flooding scheme was used as is for this study (with the excpetion of an aquifer reservoir which is added and described herein), we do not repeat the details here and merely cite these papers as references. However, more details were added to Appendix A in the new version of the paper.

2. It is not clear what the groundwater reservoir actually represents. It is stated (p9180, line 9) that it represents soil water, but in other parts of the manuscript the reservoir is described as a groundwater store. Does the store operate in a onedimensional sense in the model (i.e., do transfers into and out of the store happen only in the vertical direction), or is lateral groundwater flow included too? Moreover, it is not obvious how the parameterization of the groundwater is related to the physical properties of an aquifer. Are the groundwater-related parameters defined in Eqs. 1 and 2 calibrated to fit observed data or do they derive from independent measurements? The Vouillamoz et al. study provides a useful comparison, but it is not stated whether this study is applicable at the same scale as the model is used, nor is it stated whether the aquifer recharge data were obtained at a single location or across a wider area.

Authors : The groundwater reservoir is actually a simple linear reservoir (it does not have an explicit volume and shape) which represents the water (from drainage) going into shallow soil layers and supplying back the river after a certain time. In a sense, it models the sub-grid lateral transfer within the soil. The exchange between river and groundwater happens only in the vertical direction. This part of the model is unchanged from the original scheme which has been published. But since there seems to be an ambiguity as mentioned by this reviewer, we have added more details (along with the references) .

The residence time of water in this groundwater reservoir is controlled by the time delay factor (, constant in time and space). A study from Decharme et al., has estimated this factor to be between 45 and 60 days. Sensitivity tests have shown that increasing lengthens the drying up period (time for the discharge to return to its lowest level after the water rise). In the article, the time delay factor is 30 days which was the best value for simulations. The aquifer reservoir is, as the groundwater reservoir, an implicit reservoir which represents deeper soil layer and is characterized by a longer water time residence (is of the order of the year, 4 years in the simulations). Sensitivity tests have shown a negligible impact of the value of on the simulations (3 other values have been tried : 1, 8 and 16 years). The recharge of this reservoir occurs only vertically as for the groundwater reservoir. This reservoir does not supply back the river as the water is supposed to be lost (by lateral drainage to the river mouth or by evapotranspiration). These reservoirs are characterized by only two parameters : the time delay factor which controls the residence time of the water in each reservoir and the distribution parameter, , which allows to distribute the drainage between those two reservoirs. A study going on in CNRM focuses on implementing an explicit groundwater reservoir in TRIP for several basins in France. This study takes into account the physical charactristics of the soils (porosity, storage coefficient and aquifer thickness). However, this necessitates a large data network which is not available in the Niger basin (or at least we don't have access to these data). As a calibration of these parameters would be too long and difficult, and because it is not needed for climate applications, it was decided to represent the impact of these reservoirs in an implicit way as their contribution to the water cycle is supposed to be not negligible. We recall that the main point of global scale hydrology is to determine the evolution of the different components of the water cycle.

3. The task of producing accurate meteorological driving data is not easy and whilst section 3.2 gives a thorough description of how the data were produced it would be good to see some comparison between the original TRMM product and the RFEH product as used in the remainder of the paper. How much better is the RFEH rainfall, and are there spatial and/or temporal variations in the difference?

Authors : This is a good point: a comparison of the two rainfall datasets was done for every year and has now been included in the text. For presentation within the paper, we have presented the monthly average ratio (TRMM-RFEH)/(TRMM+RFEH) each year which represents the relative bias of one dataset regarding to the other. The analysis of the precipitation forcing is now given in section 4,4.

This ratio is calculated only when the monthly sum (TRMM+RFEH) is bigger than 1mm/day.

It seems to the authors that the most significant differences are seen in the upper basin during the monsoon season. Figure 9 shows the previous ratio for the monsoon season of every year. From 2002 to 2004, the TRMM rainfall gives 20 to 80 % more rainfall than RFEH. This area is known to its primordial role in the river supply and this difference is related to the fact that the discharge simulated when the model is forced by TRMM is generally bigger than the discharge when the model is forced by RFEH especially when there is no flooding scheme (twice bigger than RFEH). Moreover, the discharge simulated using TRMM rainfall has a longer recession period probably due to the fact that there is more water going from the floodplains to the river after the flooding season. Figure 9 also shows that in 2005, 2006 and 2007, the relative bias between the two datasets is no longer as coherent. Looking at the simulated discharge, we can see that during these 2 years, the two rainfall forcings produce quite similar discharge in amplitude, which results in a big reduction of the discharge amplitude simulated by TRMM in comparison with previous years. One possible cause for the reduction in input rainfall is that the gauge analysis source was changed from the GPCC Monitoring analysis to the Climate Prediction Center (CPC) Climate Analysis and Monitoring System (CAMS) in May, 2005. This change was made to take advantage of the timeliness in CAMS, but in retrospect it introduced a discontinuity in the error characteristics of the gauge analysis (G.J. Huffman, personal communication).

4.I am surprised that the observed discharge was 5 times greater than modeled. Working with one of the AMMA datasets, Dadson et al. (2010) found good agreement between discharge modeled with ALMIP driving data and gauged flows. How likely do the

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authors think this finding relates to the fact that all of the gauges used are downstream of the inland delta?

Authors : The authors decided to emphasize the differences between the simulations by focusing on 8 cases : 5.no aquifers, no flooding scheme, TRMM and RFE2 forcing (NOAQ-NF) 6.no aquifers, flooding scheme, TRMM and RFE2 forcing (NOAQ-F) 7.aquifers, no flooding shceme, TRMM and RFE2 forcing (AQ-NF) 8.aquifers and flooding scheme, TRMM and RFE2 forcing (AQ-F)

Moreover, new daily in-situ discharge were provided by the Niger Basin Authority (ABN) and 3 new locations situated before the delta were added. Figure 1 and 2 show the daily discharge simulated by ISBA-TRIP in the four different configurations when the model is forced by TRMM and RFE2 respectively. There is a clear change of behaviour of the observed discharge after the delta (Niamey, Ansongo, Kandadji, Malanville, Lokoja) compared to the observed discharge before the delta (Banankoro, Koulikoro, Ke Macina). Indeed, the discharge before the delta is almost twice higher than after. This suggest an impact of the inner delta on the discharge amplitude due to the floodplains. A complete analysis of the figures and of the statistic score are done in the new version of the article.

5. The authors rather casually dismiss the order-of-magnitude differences in inundation extents across the three different datasets used in the paper. It is not sufficient simply to point to a few speculations about why the EO-based inundation estimates might be an order of magnitude higher than the model predictions. More detail is needed. The difference between soil moisture saturation and open water is important and, at the very least, plots of the two components of modeled inundation should be presented in order to see if either of these matches the observations.

Authors : Unfortunately, there are no large scale spatially distributed observations of soil moisture saturation, and the different datasets do not "see" open water in the same manner, thus the problem (and why we opted to use several products).The PP product

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represents the upper bound (since, as mentioned, it incorporates saturated surfaces). The MODIS product is also based on a classification, and includes areas which are not open water. Therefore is is also likely an overestimate, although not to the same extent as the PP product. To our knowledge, there are no other readily available open water surface products available (and the PP product has been used in numerous scientific investigations). But, these products do not provide spatially distributed error estimates, so unfortunately, we can not offer any more specific information about their errors. Our goal is not to judge the products, but to compare several available products to our simulation. Given the errors in the products and the simple nature of our model and the large spatial scales considered (with obvious significant heterogeneity within each 0.5 pixel), we try to see if general spatial patterns and the timing are reasonably reproduced. We tried to explain this objective a bit better in the text.

6.The last part of the conclusion (9201; line 17 onwards),which is about SWOT, is only marginally relevant to the paper and so it should, in my view, be shortened.

Authors : This article was actually written as part of the SWOT mission project. The first part of this study was to show the actual performance of the ISBA-TRIP Continental Hydrologic system. The second part of this study focuses on determinate how the SWOT simulated data (water heights and water expansion at the surface with 100m resolution) will help to improve the performance of the model, especially its parametrization assimilation. But indeed this is a valid point: so we have tried to integrate the need for remaotely sensed data as a way to get at spatially distributed parameters at several points within the text (notably in the discussion) so that the final comments do not seem out of context.

7.Figure 5 & 6: The addition of inundation and groundwater does not lead to an obvious improvement in model performance. Further explanation of this is warranted.

Authors : See comments about the new 4 different cases analysis.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 9173, 2011.

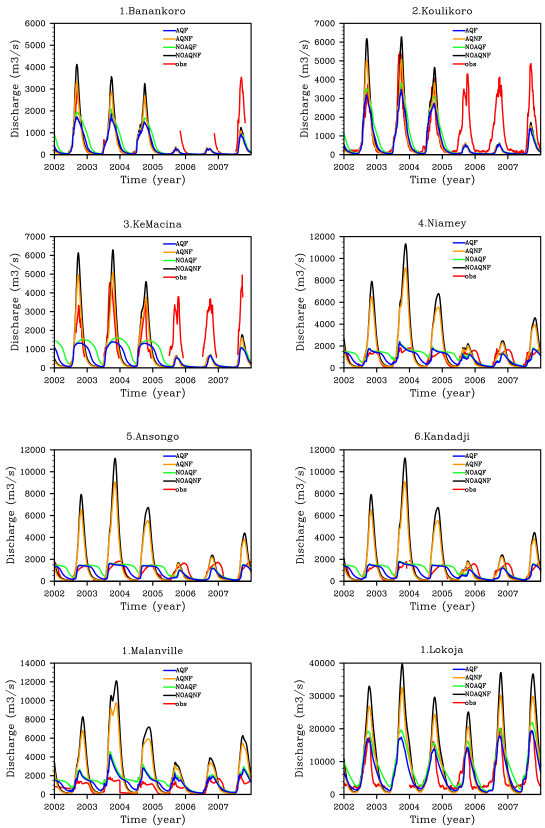

Fig. 1.

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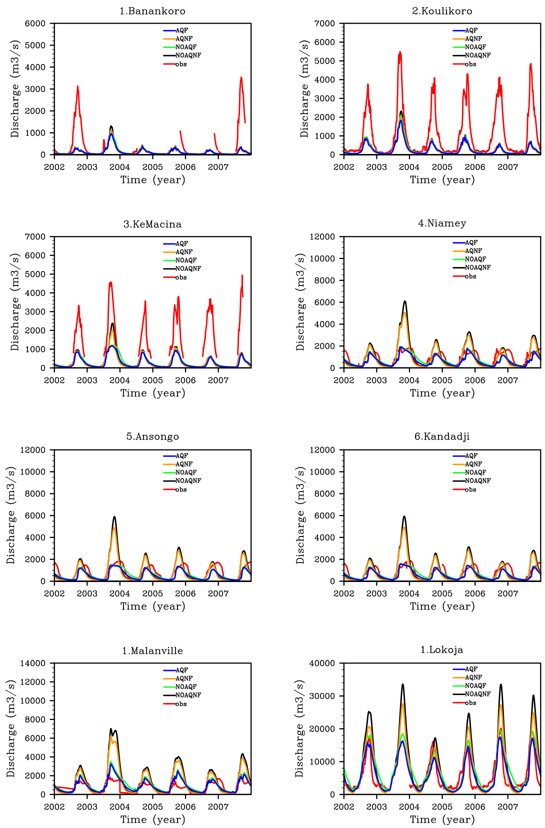

Fig. 2.

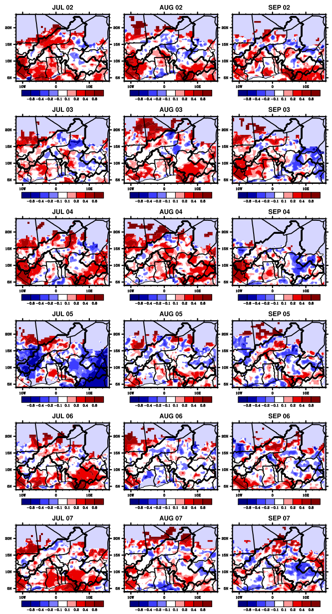

Fig. 3.

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