

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

Anonymous Referee #2

Received and published: 10 December 2011

General Comments

Thank you for your request to review the above manuscript by Pedinotti et al., entitled “Characterisation of the hydrological function of the Niger basin using the ISBA-TRIP model”. The paper presents an interesting evaluation of a new scheme to represent flood inundation within a land-surface model. The authors add an overbank inundation scheme and a simple groundwater store to the existing ISBA-TRIP model and evaluate its performance over the Niger basin. They conclude that the model performance is acceptable and present a series of evaluations of model accuracy based on river gauging, satellite and other data. A number of concerns prevent me from recommending publication of the manuscript in its current form, but I suggest that the paper may be

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suitable for publication after major revisions covering the points listed below:

Specific Comments

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

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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. 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.

Technical Points

p.9174 l.19: Model → models

p.9174 l.25: “really optimistic and show” → “really promising and shows”

p.9178 l.14: chanel → channel

p.9180 l.12: fflood → flood; determinate → determined

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.

All figures: it would be helpful to label the individual figure panels (a), (b), (c) etc.

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

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