Hydrol. Earth Syst. Sci. Discuss., 8, C5355–C5367, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C5355/2011/

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.

Anonymous Referee #3

Received and published: 19 December 2011

1.General comments

The objective of the paper is to evaluate a coupled land surface and river routing model including flood plain dynamics (ISBA-TRIP-CHS) over the Niger basin. An additional module dealing with deep aquifer recharge is also evaluated. The evaluation of a such models, is an important issue, as a realistic reprsentation of surface processes, including hydrological cycle, is required at the regional/global scale. This is praticularly true if those models are planned to be used for water resources assessment in a present or future climatic context. The Niger basin is of major importance for all the neighbouring countries. In that the present papers addresses relevant scientific questions.

Using a series of diagnostics based on ground or satellite observations, the authors

conclude their model can reasonably well reproduce the Niger river dynamics. I do not share this optimistic conclusion, as I consider that the model has been insufficiently evaluated. Several times in the paper, the analyses/comparisons made are seen from an optimistic point of view, and a more objective criticism is, in my opinion, lacking. As a result, I would not recommend the paper to be published in its current form, but I think it can be significantly improved after major revisions. The main scientific questions are summarised below.

This paper actually shows the fundamental difficulties associated with the evaluation of land surface models, such as ISBA-TRIP-CHS, with "real world data". This is particularly true for models dedicated to large/global scales. On the one hand, such models must be sufficiently general/generic to be applied in any region of the world, and must be based on simple schemes. On the other hand, one wants those models to represents actual behaviours in a "reasonable" manner. In this trade-off between generality and realism, the models should, at least, be able to represent the key hydrologic features of the basin they are applied to, if one wants confidence in the model results. Otherwise there is always a risk that the model performs well for wrong reasons. Consequently, on a particular case-study basin, conclusions such as "the model generally works well" are of little interest. I think the author use such rapid statements too often. A cautious evaluation of the "good" and "bad" points of the model would be more useful, would lead to a more objective evaluation of the model performances, and to a discussion on model deficiencies and the way to improve them, if any.

The authors say (p. 9174, l. 5-7) that datasets concerning the Niger basin are missing. This is only partially true, as some datasets exist but they are not always freely available trough public databases. Moreover, there have been numerous studies on this basin, specially on the inner delta and the sahelian part of the basin, but it is true that only few of them have been published in easily accessible international journals ; most of the studies are written in french. Concerning the inner delta and the upper basin, the PhD Thesis by Picouet (1999) and related papers gives a comprehensive overview of the hydrologic functioning. d'Orgeval and Polcher (2008) also have worked on flood plains and LSM river routing in Africa. Other references are given in the text below.

2.Scientific questions

2.1 Key hydrologic processes.

I think it is useful to recall some key hydrologic processes affecting the main different parts of the basin. Such a large catchment is rather complex but can be decomposed into the following main areas characterised by marked, contrasted, functioning. This knowledge derived from a series of past or current field/model studies.

* The upper basin, extending over Guinea, Southern Mali, located in the sudano-guinean, actually provides the major part of the water routed downstream; there are two main channels : the Niger main course and the Bani tributary. On the upper Niger, river discharge is supplied by shallow, perched water tables, with a rapid recession. On the Bani (sedimentary substratum), the ground water supply to river flow is more pronounced, and recession slower (Mahe et al., 2000). * The inner delta (Sahelian climate). This a major "loss" area (evapotranspiration and groundwater recharge), and the river flow is hardly sustained by groundwater (eg Valenza et al., 2000) * The sahelian sub-region, with an endoreic left-bank sub basin (largely inactive area with respect to water supply to the river, see Fig 13a), and an active right-bank sub-basin, with a series of small to medium size tributaries (L. Descroix et al., 2009 ; Amogu et al., 2010) * The lower basin, with essentially the same properties than the upper basin, except a higher yearly rainfall southward, with the large Benue tributary, routing water from central Africa *The mouth/delta

Depending on the climate zone and the geological substratum, the hydrologic cycle and the water pathways are highly contrsated. See Séguis et al., (2011a) for a review on the sites of the AMMA programme.

Except the inner delta, which is obviously a large flood plain, only minor parts of the

Niger main course may also be affected by seasonal flooding, and only in the sahelian area. Flood plains are very unlikely in the upper and lower basin.

The authors should mention these elements when referring to the hydrologic processes over the basin (p. 9175, l. 14-20).

2.2 Model structure

The authors should present the model with more details to let the reader clearly understand how it is built. I had to refer to Decharme (2010, 2011) and Oki en Sud (1996) to fully understand how the model was built. For example, it is not clearly explained that each grid-cell as a channel, underground reservoirs and flood plains in it (If I understood well).

The TOPMODEL approach (p 9179, l. 24-25) is clearly not relevant to simulate surface runoff in arid and semi arid areas where the horton-type runoff generation process is dominant. It is not discussed whether saturated surfaces vs horton runoff occurs where it is expected (see the "good results for good reasons " issue).

It is not clear to me what the groundwater reservoir (G) actually represents (as also pointed by anonymous referee #2). Eqs 1 and 2 show that it is supplied by the LSM drainage (Qsb) and in turn supplies the streamflow reservoir (S). In that it suggest it is sort of a shallow or perched groundwater reservoir.

In the modified model tested here, the authors added an aquifer reservoir (Aq) to account for "significant aquifer recharge" (p. 9181, l. 7). Aq is supplied by a fraction of the LSM drainage (Qsb, the remaining feeding G), and discharges to the ocean at the river mouth.

This model structure implies a 2-layer groundwater system, which can be relevant for some parts of the catchment (upper and lower basin for example), but inconsistent, for instance, in the Sahelian left-bank part of the basin where actually G does not exist. As recalled in the previous section, the supply of Aq by S is only one-way (S-> Aq) in this region.

In the areas where the two systems exist (G above Aq), it seems more realistic to me to supply Aq with a drainage from G, rather than from the LSM topsoil reservoir (eg Séguis et al., 2011b).

The assumption that Aq contributes to the flow at the river mouth (p. 9181, l. 15-16, p. 9198 l. 28-29 onward) is largely incorrect, except perhaps in the neighbourhood of the mouth. This assumption is probably related to mass conservation purposes for global approaches. A simple sink term only emptied by evapotranspiration should rather be considered for those endoreic areas disconnected from any large scale river streams. Otherwise those areas artificially loose water. If this assumption it can not be changed in the model, its impacts on the water budget should should be discussed.

The authors should precise the definition of "Flood plains" they used : either seasonally inundated areas surrounding the main river stream, renewed each year as a consequence of the locally reduced transfer capacity of the stream bed (eg the inner delta). These so-called flood plains consist in areas where the stream channel locally enlarge and separate into diverging channels that eventually converge downstream and join the main channel (ex inner delta, regions of Kandadji, Tossaye, ..). Irrespective of the rainfall amounts, "flooding" occur each year. Conversely, flood plain may refer to the largest extension of the riverbed, only flooded when above-than-normal precipitation occur. To me, the parametrisation of flooding with a critical height is relevant in the second case. In the first case, depending on the value of the critical height, flood plains in the model may remain empty when precipitations remain low, whereas they are not in the reality. I wonder if a parametrization based on locally wider river bed should not be more adapted. May the author discuss this issue ?

The way flood plain width is computed is not detailed in the paper (also noticed in the review by D. Yamazaki. A map of floodplain width (like those on fig 3) would help to figure out if the model represents flood plains in areas where they are expected, and

none where they hardly exist.

In line with this, Fig 3 shows stream parameters on the whole west african region. It suggests that any grid-cell over the region can route water in a channel. As explained above, large parts of the Niger basin actually do not contribute to the Niger discharge (see also Descroix et al, 2009). The TRIP initial setup (fig 4 in Oki and Sud, 1998) considers channels connecting the central Sahara to the Niger river. It is not clear if these flow paths have been adjusted or kept as is and how these endoreic areas have been accounted for in the model.

2.3 Results

2.3.1 Simulated discharges

TRIP (p. 9187-9189) used with simulations from 11 land surface models (ALMIP1) results in large over-estimation (2 to 5 times) of the observed discharge (Fig 4.), which is attenuated when the flood/aquifer scheme is activated, even if too much water remains in the dry season. In the commentary, the authors implicitly consider that the flood/aquifer scheme is required to have a good simulation, ie to reduce runoff. The possibility that LSMs generate too much runoff (before routing) is never considered. This trend has been demonstrated with ALMIP 1 data on a southern AMMA site (Peugeot et al., 2011). I've noticed, along with anonymous referee #2, that all the stations used for observed discharges are located downstream the inner delta. I suggest the author should evaluate the simulated discharge upstream as well (Bamako and or Koulikoro stations, data available at Niger River Authority-ABN), with or without flood/aq scheme. If LSM+TRIP discharges are still overestimated upstream, the LSM runoff must be questioned : activating the flood scheme to reduces stream flow in this area is nonsense as, in the "real life", there are no flood plains in the upper basin likely to reduce discharge.

As pointed out by D. Yamazaki (Referee) in his comment, the flood/aq scheme can act as a purely tuning process that allows a correction of biased runoff simulations (see for example p. 9197, l. 23). It this case, the model would give “a good result for a wrong reason”. As a result, the arguments proposed p. 9189 l. 4 onwards seem inappropriate to me at this stage. A more probable reason may be the generation of runoff by the model on inactive parts of the basin. Could the author check this ? Fig 5 and comments in the text show that the model performance improves for the southern station (Lokoja). My interpretation is that the model structure is more adapted to the actual processes in this area than further north (2-layer reservoirs, stream flow supplied by “G”). This holds probably also for comments p. 9189, l. 26-28.

2.3.2 Deep aquifer

page. 9190 l. 5-11 The map showing AQ recharge/storage would usefully complement fig 7 in the understanding of the model functioning : what is the spatial pattern of aquifer recharge ; does it comply with what is expected ?

As pointed out by anonymous referee #2 , the work by Vouillamoz et al, 2007 (cited p. 9190, l. 10) must be used cautiously. It concerns an area located in the endoreic, sedimentary zone of the Niger basin (left-bank). Endoreism means that none of the grid-cell (at the model resolution) contributes to the river discharge (either in surface or sub-surface). Water transfers here are purely vertical. Incidentally, the underlying aquifer (equivalent to Aq in the model) exhibit a continuous rise from the 1950s, due to increased vertical inputs (Favreau et al., 2009) thus supporting the fact that Aq in this region is a net accumulation reservoir. The author must restrict the comparison of simulated recharge with Vouillamoz et al. data to this particular region.

2.3.3 Flooded areas

The authors say (p. 9190 l. 23) that the flood plain extension is important to know. However they do not provide any maps showing what the model simulates (eg maps of flooded fraction per pixel at the flood maximum and/or minimum...). Times series in Fig 8 do not allow to figure out if the flooded areas are simulated where they are expected.

The limits of the area used to evaluate on the inner delta are not detailed (Fig 8a).

I am not convinced by the conclusions drawn p. 9191 l. 16-20. The spatial distribution of inundated areas has not been evaluated as such (only global flood fraction over 2 domains, Fig 8). Normalised data (Fig 8 c and d) show that the seasonal variability of the simulated flood plains is in phase with the observations (and with the monsoon !), but as long as their extension is clearly under-estimated (fig 8 a and b), can we really conclude that they are correctly represented ? The authors suggest the reference data (CPP, JFC) may be biased, but, more simply, what about a model deficiency ?

2.3.4 River height

In my opinion, the whole section can not lead to the optimistic conclusion written lines 1-2 (p. 9192)

The authors evaluate river height changes on Fig 9. What about the comparison of the absolute simulated and satellite-derived values of river height ? As far as flooding is computed with respect to a critical height (and not an anomaly), the evaluation of the absolute water height seems more important than the change in water depth.

Additional evaluations could be done at the river gauge stations. As river discharge derive from water height measurements, those height data should be available as well. Alternatively, the discharge time series and the stations rating curves (height-discharge experimental relationship) would allow to infer the observed time series of water depths. These data are probably easily available from ABN (Niger Basin Authority) upon request.

2.3.5 Terrestrial water storage

The whole 4.5 section is a bit confusing, and should be re-written, as well as caption for fig 10 (the green line in the bottom panel is not well documented).

The legends on fig 10 must be easily understandable. What does dWg stand for ? Which one is the GRACE estimate ? In the middle panel, what these terms refer to

: components calculated by the LSM before routing, Others ? What is the correspondence with the terms in the upper panel ?

p. 9192, l. 21-23 : the correlation between both signal has not been measured (or is not shown) : one can only believe it is good ! The graphs on Fig 10 only show a fairly good “co-fluctuation”

Indeed, Fig 10 (bottom panel) shows that the simulated water storage change is fairly close to the GRACE estimate. However, this result does not imply that the individual contribution of each component to the budget is correct. In line with that, the conclusions p. 9190 l. 15-21 are inappropriate, and I think at not point one can consider that the model is able to correctly distribute water in the various hydrologic components. This conclusion can only be supported by additional, detailed, evaluations, and I think the model is still insufficiently evaluated.

2.3.6 Endorheism This section is not convincing :

In endoreic areas, the drainage calculated by ISBA should be totally diverted as aquifer (Aq) recharge. Then Fig 13b should show values of runoff only (and not Runoff + Drainage) in order to assess the endorheic nature of the simulations. A map may be added for the drainage term if the authors wish to assess this variable too.

The authors say that Roff+Drainage over rainfall is “nearly zero” in the northern part of the basin (p. 9195, l. 27). The author should be more precise, or show time series of simulated runoff on the northern subregion. The color scale in Fig 13b is not sufficiently detailed and the ratio is somewhere between 0 and 0.1 (dark blue). Incidentally, a ratio of 0.1 is not at all “low” as it is comparable to runoff ratio observed in more humid areas to the south (see Peugeot et al., 2011). Moreover, a “low” ratio averaged over 6 years may hide “high” values at smaller time scales (day), as runoff only occurs during the 4-5 months on the monsoon season. I fear that the ratio can be much higher, due to an overestimation of LSM runoff over these regions too (see above). This point must be carefully checked before one can make any conclusion on the endorheic behaviour

of the model over those region. At this stage, I would not agree with the the conclusion of the section (l. 67, p. 9196).

2.3.7 Discussion and conclusion

I disagree with the statement p. 9196 l. 20-23. As far as I’ve understood the work, I think the author can not say that “Three reservoirs out of four” have been “constrained by measurement data”. At most, the model has been “compared” (not constrained) to some datasets, and/or on some limited areas.

p. 9197 l. 13-14 : this phrase seems confusing. Do the author say that the monthly evaporation from ISBA-TRIP-CHS is reasonable ? Why not showing a comparison with the products mentioned above in the same section (l. 4-14) ?

l. 15-22. Warning, the two years mentioned were extreme ones : severe drought in 1984, largely excess rainfall in 1924. Considering the climatic break around 1970 (beginning of the prolonged drought period), the simulated evapotranspiration should be compared to an average over recent years, preferably a period with similar yearly rainfall.

l. 23-24 : I think a flood scheme is probably useful but only in those regions where there are flood plains.

p. 9200 l. 21-22. A 2-peaks pattern is observed in the discharge time series, mainly in Niamey (see figs. 4, 5, and 6). The early peak correspond to the contribution of the local tributaries of the right-bank, and the late peak is the delayed contribution of the upper Niger basin (Amogu et al., 2010). It is unclear whether the model misses the first or the second peak.

3.Technical corrections

p. 9182, l. 17-18. “monsoon basins” is inappropriate : the whole west African region is affected by the monsoon. “Humid basins” seems more suitable.

p. 9193, l. 14 : Table1 → Table 2

p. 9196, l. 18 : Sect5 → Sect 4

p 9198 l. 21-22 : “Indeed, the model ...” : this phrase is confusing

p. 9212, l. 23-24 : the reference is incomplete

p. 9215. Caption fig1. Add that this sketch applies for all grid-cell ?

p. 9216. Fig2. Purple symbols are hardly visible : use an other color ? Elevation scale on the right : limit to the range of the map (0-3000 m ?). outline the Niger main course and the inner delta ?

p. 9218. Fig 4 enlarge font size . Improve the inserted legends (explicit what FLD/NOFLD means), move these informations in the caption ? same for figs 5, 6, 11, 12

p. 9221, Fig 7. Delete fig title [AQ-NOAQ...] or replace with something meaningful ; check caption : first “AQ” should be replaced by “NOAQ”

p. 9293 Fig. 9. Enlarge the figure/font size/axis titles, etc...

p. 9224, fig 10. Make the legends on the left understandable. Bottom panel : use an other color for the “blue” curve (mixed up with the blue range)

p. 9227. Fig 13a is too small, text illegible.

References cited

Amogu, O., Descroix, L., Yéro, K.S., Le Breton, E., Mamadou, I., Ali, A., Vischel, T., Bader, J., Moussa, I.B., Gautier, E., Boubkraoui, S., et Belleudy, P., 2010. Increasing River Flows in the Sahel? Water, 2 (2), 170-199.

Descroix, L., Mahé, G., Lebel, T., Favreau, G., Galle, S., Gautier, E., Olivry, J., Albergel, J., Amogu, O., Cappelaere, B., Dessouassi, R., Diedhiou, A., Le Breton, E., Mamadou, I., et Sighomnou, D., 2009. Spatio-temporal variability of hydrological regimes around

the boundaries between Sahelian and Sudanian areas of West Africa: A synthesis. Journal of Hydrology, 375 (1-2), 90-102.

d'Orgeval, T., et Polcher, J., 2008. Impacts of precipitation events and land-use changes on West African river discharges during the years 1951–2000. Climate Dynamics, 31, 249-262.

Favreau, G., Cappelaere, B., Massuel, S., Leblanc, M., Boucher, M., Boulain, N., et Leduc, C., 2009. Land clearing, climate variability, and water resources increase in semiarid southwest Niger: A review. Water Resour. Res., 45 (7), W00A16.

Mahe, G., Olivry, J.C., Dessouassi, R., Orange, D., Bamba, F., et Servat, E., 2000. Surface water and groundwater relationships in a tropical river of Mali. COMPTES RENDUS DE L ACADEMIE DES SCIENCES SERIE II FASCICULE A-SCIENCES DE LA TERRE ET DES PLANETES, 330 (10), 689-692.

Peugeot, C., Guichard, F., Bock, O., Bouniol, D., Chong, M., Boone, A., Cappelaere, B.A.-.G., Besson, L., Lemaître, Y., Séguis, L., Zannou, A., Galle, S., et Redelsperger, J., 2011. Meso-scale water cycle within the West African Monsoon. Atmospheric Science Letters, 12 (1), 45-50.

Picouet, C., 1999. Géodynamique d'un hydrosystème tropical peu anthropisé, le bassin supérieur du Niger et son delta intérieur / Geochemistry of a large river : Upper Niger river and inland delta (Mali).: Université Montpellier II - Sciences et Techniques du Languedoc.

Séguis, L., Boulain, N., Cappelaere, B., Cohard, J., Favreau, G., Galle, S., Guyot, A., Hiernaux, P., Mougin, E., Peugeot, C., Ramier, D., Seghieri, J., Timouk, F., Demarez, V., Demarty, J., et al., 2011a. Contrasted land-surface processes along the West African rainfall gradient. Atmospheric Science Letters, 12 (1), 31-37.

Séguis, L., Kamagaté, B., Favreau, G., Descloitres, M., Seidel, J., Galle, S., Peugeot, C., Gosset, M., Le Barbé, L., Malinur, F., Van Exter, S., Arjounin, M., Boubkraoui, S., et

Wubda, M., 2011b. Origins of streamflow in a crystalline basement catchment in a sub-humid Sudanian zone: The Donga basin (Benin, West Africa): Inter-annual variability of water budget. Journal of Hydrology, 402 (1-2), 1-13.

Valenza, A., Grillot, J.C., et Dazy, J., 2000. Influence of groundwater on the degradation of irrigated soils in a semi-arid region, the inner delta of the Niger River, Mali. Hydrogeology Journal, 8 (4), 417-429.

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