

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

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Review : 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 represent 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.

Authors : The aim of such a global scale study is to evaluate how the model is able to reproduce the evolution of inland water resources in order to run a coupled land surface-atmosphere model simulation and detect anomalies in the future climate. The point of evaluating the model on a one basin case is to validate/evaluate the added processes in a region where these processes are known as dominant/not negligible. One must not forget that the next step of such a study is to expand the simulation at the global scale. Following this method, Decharme et al. (2008) evaluated the performance of the ISBA-TRIP-CHS (including the flooding scheme) over the Amazon basin (which is known as an important flooding area and where large discharge datasets are available) and then evaluated the scheme at global scale (Decharme et al., 2011). It was noticed that in some flooding regions (such as the Niger basin), a bias of discharge was still remaining. A possible reason for this bias was that, in this complex region, some deep water reservoirs (not represented in the model) were supplied by the drainage water and supposed to not supply back the river. So, the aim of this paper is to test a simple and implicit reservoir of aquifer over the Niger basin and estimate its impact on different components of the water cycle. General global hydrology studies evaluate their simulations with few datasets (generally the discharge) as global scale datasets are hardly available (dire pourquoi ça suffit en général). However, in this study, regarding the insufficient number of discharge data that was available, the authors decided to compare the simulations with other data (water height change, GRACE total water storage, flooded fraction) to evaluate the ability of the model to reproduce the general evolution of these different components involved in the water cycle. However, the model is of course limited because of different reasons: its simplicity (for climate

applications, we do not want the computation to be too fastidious), its parametrization (for the same previous reason we want the model not to be 'over-parametrized'. Moreover, this version of the ISBA-TRIP-CHS might be limited by the fact that the aquifer reservoir is implicitly defined. A study ng on at CNRM focuses on implementing an explicit aquifer reservoir in the ISBA-TRIP-CHS (taking into account the aquifer size, the porosity and the storage coefficient). For now, the aquifer implementation is tested over several french basins, the availability of data making it possible. Before this explicite representation of aquifers to be exported at global scale, this study aims at representing these non negligible water reservoir in a simpler way in order to see its impact on water cycle. However, this study can further be compared to the version with a more detailed physics of aquifers.

Review : 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.

Authors : The authors agree your comment about the possibility to give a more critical point of view on the model. In order to do that, a more precise study was conducted separating 8 different simulations:

no aquifers, no flooding scheme, TRMM and RFE2 forcing (NOAQ-NF) no aquifers, flooding scheme, TRMM and RFE2 forcing (NOAQ-F) aquifers, no flooding shceme, TRMM and RFE2 forcing (AQ-NF) aquifers and flooding scheme, TRMM and RFE2 forcing (AQ-F)

Full Screen / Esc

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Discussion Paper



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 RFEH 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 flood-plains. A complete analysis of the figures and of the statistic scores is done in the new version of the article.

The lack of data was at first the reason why we decided to compare the model with other datasets such as the water height changes and the GRACE satellite data. Looking at Decharme et. Al, 2008, the evaluation of the ISBA-TRIP model over the Amazone basin was done only using discharge data (peut etre zones inondées aussi) which is already considered as a significant evaluation in global hydrology considering the simplicity of the model, the lack of data with a good spatial coverage (a local and sparse data is not relevant regarding the size of the studied area and the spatial resolution of the model). Moreover, the aim of such a study is to evaluate a model which will be used in climate applications. Climate studies generally aim at detecting big anomalies (in temperature, rain events ...) in the future years or decades. Thus, the model is less supposed to simulate perfectly the absolute values of the different variables of the water cycle than to detect anomalies in their evolution. In this sense, the proposed study shows that the model is able to detect different anomalies at inter and intra annual scale.

Review : 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,

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located in the sudanoguinean, 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 contrasted. 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)

Authors : This is a good and complete description of the functioning of the Niger basin. It can also allow an evaluation of how the large scale model is able to represent some smaller scale characteristics of the basin. It also highlights the limits of a global scale model compared to a smaller scale and classic hydrologic model. In fact, a global scale model such as ISBA TRIP is characterized by its simple physics. The land water evolution is represented by simple linear water mass transfer equations (in order to simplify and accelerate the computations, which might be useful for climate applications with several decades of duration). Moreover, the number of parameters used to characterize the basin must be relatively small (for the same previous reasons).

As the aim is to detect anomalies over a big timescale (several decades) there is no point to tune the model to get the correct simulations. Some of the parameters used as input variables are derived from geomorphologic relationships which might not be completely representative of the reality but allows to have spatial coverage of the studied area. Other parameters (nommer) are constant in space which is obviously not realistic according to the heterogeneity of the basin but represents a start for this kind of study according to the actual lack of data. The article points out the possibility of improving the parametrization of the model using assimilation of satellite data. However, it seems to the authors that the description (given by the reviewer) is too specific to be inserted in the text as such thin scale processes can not be simulated by the model. As a perspective, it will be interesting to insert these processes in the model at thinner scales.

Review : 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).

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, it was decided here, not to go into deep details and cite these papers as references. However, more characteristics of the model were added in the TRIP parameters section and in the Appendix A.

Review : 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).

Authors : The surface runoff over saturated area, named Dunne runoff, is computed using a simple TOPMODEL approach (Beven and Kirkby, 1979). A saturated fraction, f_{sat} , is then simulated where precipitation is entirely converted into surface runoff (Decharme et al., 2006). In addition, this approach is coupled with a sub-grid exponential distribution of the soil maximum infiltration capacity in order to enable the infiltration excess runoff mechanism, named Horton runoff (Decharme and Douville, 2006).

Review : 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 (Q_{sb}) and in turn supplies the streamflow reservoir (S). In that it suggest it is sort of a shallow or perched groundwater reservoir.

Authors : The groundwater reservoir is actually an implicit 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. The exchange between river and groundwater happens only in the vertical direction. The residence time of water in this groundwater reservoir is controlled by the time delay factor (τ , 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 (τ is of the order of the year, 4 years in the simulations). Sensitivity tests have shown a negligible impact of the value of τ 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

Full Screen / Esc

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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 characteristics 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.

Review : 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 (Q_{sb} , 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).

Authors : The addition of aquifers was done to take account of the presence of the significant recharge of aquifers in some parts of the basin. This aquifer reservoir was done in an implicit way with no real shape, and no physical characteristics (porosity, storage coefficient and aquifer thickness). However, if we look at the spatial distribution of the

Full Screen / Esc

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aquifer recharge, we can observe that this recharge is hardly happening in the Sahelian region. Other configurations were tested, like setting the alpha coefficient to 1 in this region (no water going into aquifers) but there was no significant differences in the results as there is almost no drainage in this area. According to the authors, supplying aquifers with water from groundwater reservoir will not end up in more differences in the different variables evolution due to the linearity of the model. The water from drainage which is actually transferred to the aquifers was going into the groundwater in the previous version of the model. Maybe there would be less or more water going to the aquifer or going back to the surface but as there is actually no efficient way to validate the aquifer storage, it does not seem like an important factor for the simulation.

Review : 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 be discussed.

Authors : This is an interesting point which could be changed in the model and at least, discussed in the paper. The way the water of the aquifers is lost was a suggestion that other studies (Decharme et al., 2011) made. The major point of adding the aquifers was that there was incoherence between the model and the observed data (such as the discharge) because there was too much water remaining in the river stream. However, this remark is interesting as its impact on the atmosphere might not be negligible. This could be validated with a large set of evaporation data which is not actually available. Two simulations could be done, one in which the water from aquifers drains directly to the river mouth and one in which the water from aquifers evaporates. The evaporation simulated by the model could then be compared to the evaporation data. Since this global dataset is not available on the studied period, and the simulated total water

Full Screen / Esc

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Discussion Paper



storage simulated fits the GRACE satellite data, the only thing that can be evaluated here is the evolution of the aquifer water storage. The authors suggest discuss this point in the discussion part as a perspective to go further into this study.

Review : 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 ?

Authors : The floodplains expression used in the paper refers to the second definition given by the reviewer. However, the parametrization is done in order to take into account the inundations occurring in the delta, even at first order (see Decharme et al., 2010 for more details). However, as pointed out by the reviewer, a locally based river width would result in a more realistic representation of the inundated areas. But as it is explained in the paper, a parametrization of the parameters would be long and fastidious and the authors expect that the use of observation data for assimilation would provide a way to optimize these parameters.

Review : 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.

Authors : This is now explained in Appendix A.

Review : 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.

Authors : You are actually right pointing out the fact that the model might not be completely realistic. In global applications, land surface models must provide a way to close the water cycle using simple equations and few parameters. TRIP also gives an estimation of the land water evolution for regional scale applications. However, due to the simplicity of the model, some processes are not explicitly represented such as the endorheism of the basin. However, this effect might be taken into account by the inclusion of deep aquifers in the basin. A PHD thesis is actually going on at CNRM, looking at the implementation of an explicit aquifer reservoir in ISBA-TRIP which takes into account the physical properties of the soil and the aquifers. This study actually focuses on some french rivers but aims at being extended at global scale. For the current study, there is hardly any rainfalls occurring in the Northern region (in comparison with the rest of the basin), so this concern might be negligible from a global point of view.

Review : 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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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.

Authors : The new evaluation using in-situ discharges upstream of the inner delta shows that the ALMIP models simulate a pretty coherent discharge before the inner delta, leading to the conclusion that the possible reason for a poor simulation of the discharge downstream of the inner delta might be the fact that these models do not represent floodplains. The improvement of the discharges with the model using the flooding scheme suggest that this hypothesis might be right.

Review : 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). In 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.

Authors : The availability of new discharges stations upstream of the inner delta allowed a better analysis. Indeed, it is shown that the model without floodplains is in agreement with the observed discharge upstream of the inner delta but fails to reproduce the discharge after the inner delta while the introduction of the flooding scheme in the model improves the simulated discharge. Moreover, the observed discharge

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

downstream of the inner delta is reduced by two compared to the discharge upstream of the delta, pointing out the fact that the delta must have an impact on the discharge. This area is also known as a main flooded area so this factor might be a reason for this change of discharge. The article focuses on 4 configuration to evaluate the separate impact of floods and aquifers on the discharge. It is shown that while the introduction of aquifers might slightly deteriorate the discharge in some regions it well improves the discharge in other locations. This is a good result as the aquifer is linear and poorly parametrised. A study going on in CNRM focuses on implementing a more complicated aquifer reservoir. However, model deficiencies are also pointed out like the poor parametrisation or the fact that ISBA might not be good in the runoff calculation.

Review : 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?

Authors : The available data concerning the aquifer storage are generally very localized making the comparison with such a global scale model not always relevant. A new figure added to the paper shows the repartition of the aquifer recharge over the basin. As expected, the aquifer recharge is very heterogeneous over the basin and follows rain patterns. There is also more aquifer recharge when the model is forced by TRMM than by RFEH. The aquifer reservoir is simple (only based on one parameter) and linear and cannot represent precisely the fluctuations and repartition of the aquifer recharge. However, the analysis of total water storage have shown that its contribution to this total storage is not negligible and must be taken into account to reproduce the evolution of the water budget. In order to avoid any misunderstanding, the comparison with Vouillamoz aquifer recharge was removed from the paper and the previous comment was added.

Review : 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

Full Screen / Esc

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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.

Authors : In order to avoid any misunderstanding, the comparison with Vouillamoz aquifer recharge was removed from the paper and the previous comment was added.

Review : 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.

Authors : A new figure was added to the text showing the monthly relative CPP flooded fraction over the period 2002-2007. The monthly values have been divided by the maximum monthly value over 2002-2007. According to these observations, the main inundations occur between July and December in three principal regions : the inner delta in Mali, the Northern Nigeria and the Southern basin. A figure was done showing the monthly spatial correlations between CPP and ISBA-TRIP when the model is forced by TRMM and RFEH with and without aquifers. Over the 3 principal inundated regions, the correlation is bigger than 0.4. This figures are now in the paper.

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

Authors : The area for the inner delta is [30:60°N; 13N:16N].

Review : 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

Full Screen / Esc

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Discussion Paper



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?

Authors : New figures show that the model simulates floodplains on the main inundated regions seen by the CPP data. 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 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 it 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.

Review : 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

Full Screen / Esc

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(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.

Authors : Actually, from a global hydrology point of view, there is no real point to look at this absolute value as the topography of the river in te model is not realistic at all. In fact, in each grid cell, the 'river' is represented by a simple rectangular reservoir which drives the runoff from LSM. To have a good representation of absolute water height, as it is done in small scale hydrological models, a good MNT is needed which is not the case here (at global scale, a good MNT for every basin would be difficult to get). It might be necessary to remind at some point of the study, that the general aim of this kind of study is to export the model from a 1-basin case to a global case (in order to couple the CHS with an atmospheric model). This model is aimed to be used in climatologic applications in which the interest is to highlight climate anomalies (as temperature anomalies for example). So, from a global scale point of view, what is important here is to get a good representation of the water cycle (ie to represent the relative evolution of its components) and to detect anomalies in these components evolution, such as the water height anomalies which are represented in figure 9.

Review : 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).

Authors : The green line represents the superficial soil layer, written as dWg.

Review : 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

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

Authors : dWg stands for the water stored in the near subsurface (few meters (1 or 2m)) which is generally represented in the LSM to calculate the exchanges between the land surface and the atmosphere (energy fluxes). It is here represented along with the surface water storage (couleur) because in the models with no groundwater and no aquifers, this sum is considered as the total water storage which sometimes results in a bias with GRACE data in regions where groundwater and/or aquifer storage are not negligible. The terms in the middle panel are the drain, runoff and evaporation calculated by the LSM and the rain which is a forcing variable. There are represented here because the basic mass conservation equation of global hydrology stands that the evolution of land water storage depends directly of these terms. It is important to understand the functioning of the water cycle, to see how the model distributes the rain between runoff, drainage and evaporation.

The top panel shows how the routing model distributes the water between the different reservoirs represented in the model. It allows to better determine the major components of the water cycle.

The aim is to observe the evolution of water cycle components which is a major issue of global climate applications.

Review : 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”.

Authors : The correlations scores have been calculated and are not shown in Table 6. They show a pretty good correlation of the model with GRACE products, independently of the configuration. The model is able to reproduce in a good way the variations of total water storage which is important for climate applications. Indeed, it is expected that the variations in inland waters may have an impact on climate (Gedney et al.,

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2000; Douville et al., 2000, 2003, 2004; Molod et al., 2004; Lawrence and Slater, 2007; Alkama et al., 2008). The fact that it is not greatly dependant on the configuration just points out the fact that we cannot use GRACE to evaluate which configuration is the more realistic. But, if we make the assumption that aquifers exist in the Niger basin, the analysis of TWS indicates that the aquifer reservoir has a non negligible contribution to this storage.

Review : 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.

Authors : General evaluations of global hydrological models only use discharge data for comparison. In this line, this study provides a further comparison of the model as other datasets are used for evaluation. It is shown that, considering its simplicity, the model is able to simulate the evolution of main variable involved in the water cycle. Of course, further comparisons would be valuable but large datasets are missing to complete the validation.

Review : 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).

Authors : After careful consideration of this reviewers very thorough comments, we have decided to remove this section. Indeed, representing *true endorheism* (in the strict sense as discussed by this reviewer) in a global scale hydrological model-LSM is currently not feasible it seems (for most of the LSMS which the authors are aware of in similar configurations/applied to similar scales). The crux of the problem is related to scale: at 0.5 degrees, land surface models usually have sub-grid runoff parameterizations activated. This generally means that by design, some fraction (albeit potentially small) of the rainfall is automatically removed/placed into the nearest stream reservoir. So, by definition, a true endorheism is not explicitly modeled (as this reviewer accurately suggests) at this spatial scale. But we were encouraged by the fact that most of the water in each grid cell remains "local", the model is indeed producing a first order response which contributes almost no water to streamflow (leaving the gridcell): but again, this is not true endorheism so we have deleted this part (it was not critical to the focus of this paper: river and floodplains).

Review : 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.

Authors : The text was modified consequently to this remark as the authors agree the fact that data are missing to completely validate the model dynamics.

Review : 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) ?

Authors : There are two meso-squares which contain flux measurements for several local scale sites (one in Mali, the other in Niger near Niamey) and they were not co-located aq infiltr 5(??). As explained in Boone et al. (2009), owing to the large spatial heterogeneity in this region it is not really possible to compare evaporation simulated for a 0.5 degree region to local scale observations. But in that paper, it was shown that when LSM evaporation was aggregated using 3 different sites to the scale of a single ISBA-TRIP grid cell, LSM models (and ISBA included) represented the annual cycle of the sensible heat flux for several years (20XX-20XX...see BAMS paper) reasonably well. Since downwelling radiation was imposed by a calibrated satellite-based product (LAND-SAF radiation) and over a three year period the ground heat flux was relatively small, we deduce that a good simulation of H corresponds to a reasonable simulation of Evap. Of course, this is far from being a perfect evaluation, however, given the relatively small spatial coverage of in-situ data, the spatial heterogeneity characteristic of such sites (thus necessitating multiple sites and a robust upscaling methodology), and the errors in the forcings, we argue that this is the best one can do currently.

Review : 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.

Authors : Indeed, the comparison with recent large evaporation datasets would be valuable but they currently do not exist or are not yet validated so the use of these data for comparison with the model would not be relevant at this point.

Review : l. 23-24 : I think a flood scheme is probably useful but only in those regions

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where there are flood plains.

Authors : This is correct but in regions where there are no floodplains the model is expected not to simulate floodplains. However, new figures have been added to the text showing that ISBA-TRIP simulates floodplains in the main observed inundated areas from CPP.

Review : 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 corresponds 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.

Authors : The availability of new discharge data allows a better interpretation of this 2 peak effect. Looking at the observations, it seems that this peak does not exist for the discharges upstream of the inner delta suggesting a link between the floods occurring in the delta and this second peak. Indeed, the floodplains act as water storage areas during the monsoon. Part of this water evaporates or infiltrates but some water remains in the floodplains. After the monsoon period, the river empties and is then supplied by water from the floodplains which, added to the water from the upstream region might create this second peak. However, it is difficult to know which peak the model dismisses. The supply of the river by the floodplains is supposed to be represented by the model but other model or forcing errors can result in this difficulty to represent the second peak. However, considering the time correlation of the observed and simulated discharge, it seems that the second peak is not seen by the model.

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

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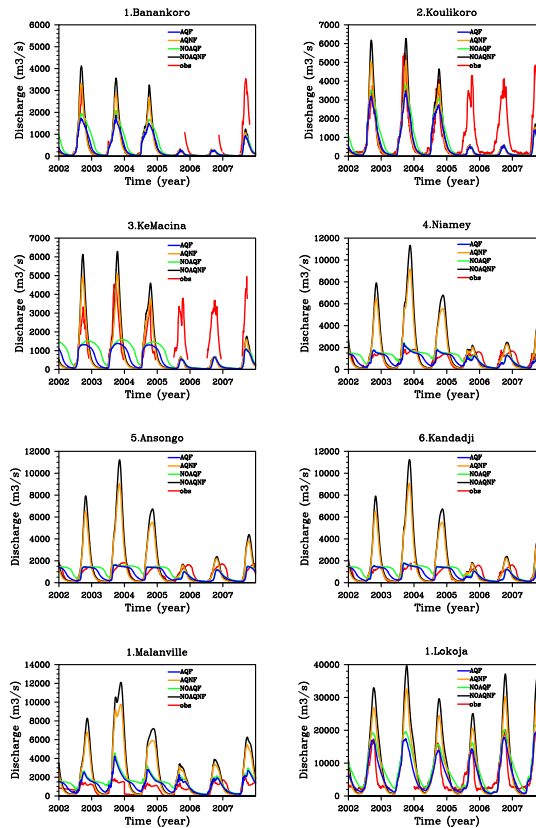


Fig. 1.

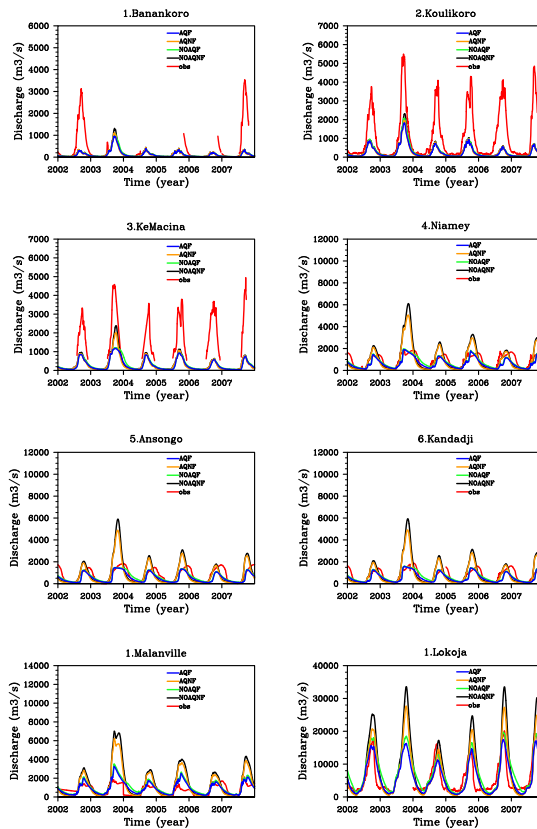


Fig. 2.

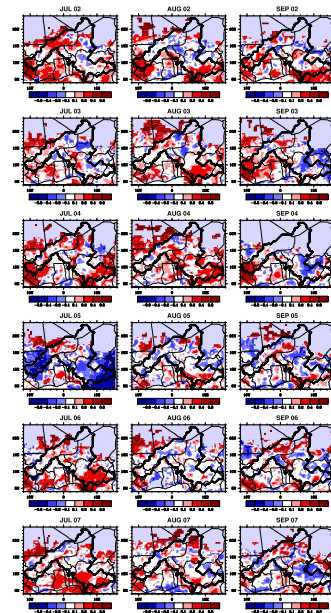


Fig. 3.

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