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Hydrology and Earth System Sciences Discussions

Interactive comment on "Impact of precipitation and land biophysical variables on the simulated discharge of European and Mediterranean rivers" *by* C. Szczypta et al.

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The authors thank the anonymous reviewer #1 for his/her review of the manuscript and for his/her comments.

1.1 [A clear upfront statement of the hypothesis and/or objectives is missing: the apparent objectives on page 5440 line 14-16 are too vague, though I ultimately think I found them more usefully expressed at the start of Section 2.4. Pls use that instead to better set the scene of the remainder and summarise those goals in the abstract also - it would have really helped me understand the m/s the first time around.]

RESPONSE 1.1

Yes. The first sentence of the Abstract and the third paragraph of Sect. 1 could be reworded, respectively, as:

- "This study investigates the impact on river discharge simulations of errors in the precipitation forcing, together with changes in the representation of vegetation variables and of plant transpiration. The most recent European Centre for Medium-Range Weather Forecasts reanalysis (ERA-Interim) is used to drive the Interactions between Soil Biosphere and Atmosphere - Total Runoff Integrating Pathways (ISBA-TRIP) continental hydrological system over Europe and the Mediterranean basin over the 1991–2008 period. As ERA-Interim tends to underestimate precipitation, a number of precipitation corrections are proposed."

- "The two major objectives of this study are: (1) reduce the bias of the ERA-I precipitation using ancillary data and validate the bias-corrected precipitation through river discharge simulations, (2) test different LSM configurations driven by the best available atmospheric forcing."

1.2 [There are some lapses of logic in the Introduction. They are probably fairly easy to address but that is necessary for the relevance of this m/s to become clear. For example: the sentence "Because the Mediterranean basin will probably be affected by climate change. . ." needs to mention the type of evidence - based on GCM modelling presumably? Also, what does IPCC have to say about it? It does not automatically follow from the previous that it "is important to build monitoring systems"- I am sympathetic, but you should use a few words to explain how that will be helpful in the face of climate change. With regards to the (unexplained) importance of monitoring systems, how will simulating past droughts with ISBA help develop those?]

RESPONSE 1.2

This part of Sect. 1 could be rephrased as:

"The Mediterranean basin will probably be affected by climate change to a large extent (Gibelin and Déqué, 2003; Giorgi, 2006; Planton et al., 2012). The fourth assessment report of the Intergovernmental Panel on Climate Change (IPCC) emphasized that over Europe and Mediterranean areas, the annual mean temperature of the air is likely to increase more than the global mean. In most Mediterranean regions, this trend would be associated with a decrease in annual precipitation (Christensen et al., 2007). In this context, it is important to build monitoring systems of the land surface variables and of the hydrological variables over this region, able to describe extreme climatic events such as droughts and to analyze their severity with respect to past droughts.".

REFERENCE:

Christensen, J.H., Hewitson, B., Busuioc, A., Chen, A., Gao, X., Held, I., Jones, R., Kolli, R.K., Kwon, W.-T., Laprise, R., Magaña Rueda, V., Mearns, L., Menéndez, C.G., Räisänen, J., Rinke, A., Sarr, A. and Whetton, P.: Regional Climate Projections. In: Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change, edited by Solomon, S., Qin, D., Manning, M., Chen, Z., Marquis, M., Averyt, K.B., Tignor, M. and Miller, H.L., Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 2007.

1.3 [What is ISBA, what was it developed for and how is it a useful? Not until quite a bit further in do these things become clearer (bringing page 5444 line 14-18 forward would be an easy fix)]

RESPONSE 1.3

Yes. The two first sentences of Sect. 2.3.1 could be moved to Sect. 1 and adapted as follows:

"The ISBA LSM was developed at Météo-France to describe the land surface processes in weather forecast and climate models. ISBA uses a limited number of pa-

rameters, mapped according to the soil and vegetation types provided by the global 1km$\times$1km resolution ECOCLIMAP land cover and look-up table database (Masson et al., 2003).".

1.4 ["provided unbiased precipitation data are used. . .simulated river flow can be used for verification" That is overstating it a bit; what about the coarse resolution spatial average represented by the forcing combined with non-linearity in the hydrological response? (Just a caveat to be recognised). See e.g. Van Dijk and Renzullo (HESS 2011) and references therein to support/elaborate on some of the above points and for a discussion on the traditional difference between LSMs and large scale hydrology models.]

RESPONSE 1.4

The sentence "Therefore, provided unbiased precipitation data are used (Fedeke et al., 2003), the simulated river flow can be used for the verification of the LSM simulations (Boone et al., 2004)" (P. 5440, L. 5-7) could be replaced by "Therefore, provided bias-corrected precipitation data are used (Fedeke et a., 2003), and that errors caused by the coarse resolution spatial average of the atmospheric forcings combined with the non-linearity in the hydrological response are not too large (Van Dijk and Renzullo, 2011), the simulated river flow can be used for the intercomparison of LSM simulations (Boone et al., 2004)."

REFERENCE:

van Dijk, A. I. J. M. and Renzullo, L. J.: Water resource monitoring systems and the role of satellite observations, Hydrol. Earth Syst. Sci., 15, 39-55, doi:10.5194/hess-15-39-2011, 2011.

1.5 [The Methods need more detail about a few key assumptions and representations; (a) it would help enormously to have a brief "method overview" that explains you are effectively doing two independent experiments: one evaluating streamflow derived with

4 different forcings; and one derived with 3 different vegetation parameterisations.]

RESPONSE 1.5

Yes. The following sentence could be added at the start of Sect. 2 (P. 5441, L. 14):

"Two independent experiments are described: evaluation of the river discharges derived with (1) four different precipitation forcings, and (2) three different vegetation parameterisations".

1.6 [an important issue with LSM streamflow is that groundwater stores and dynamics (inc. e.g. groundwater uptake by roots, groundwater discharge) are often not, or poorly described. Can you pls add some more details around what the model includes (e.g. which processes are represented, what type of approach is the Noilhan-Mahfouf scheme you mention, where does the deep drainage go? How is capillary [sic] rise described? How is it coupled with TRIP?) and provide some assessment of their importance for model performance or errors in the Discussion.]

RESPONSE 1.6

The following details could be added to Sect. 2.3.3:

"The groundwater outflow is linearly related to the groundwater mass, G, through a uniform and constant time delay factor, t. Changes in the G reservoir do not represent the groundwater dynamics, but permits the representation of the lagged contribution of the groundwater flow to the surface river reservoir within a particular grid cell: while the surface runoff produced by ISBA directly supplies the rivers, the deep drainage produced by ISBA is first injected into the groundwater reservoir. In this study, the latter supplies the rivers with a time delay factor of t = 30 days. More details about these parameterizations can be found in Decharme et al. (2010)."

1.7 [There is not enough detail for me to understand exactly what the main differences are between the three vegetation parameterisations used. How does the prognostic model simulate day-to-day LAI dynamics; and what driving processes does it consider?

What type of approach does it use? (e.g. classify/describe the main features e.g. using the framework set out in Arora, Rev. Geophys. 2002). What exactly are the functional and/or parameter value differences between the 3 variants used, so I can understand how they might behave differently? The description is partly to be found on p 5445, part on p5446, but after reading them I did not have a good understanding and had trouble reminding myself of what the different acronyms signified.]

RESPONSE 1.7

According to the model classification framework set out in Arora (2002), the photosynthesis model within ISBA-A-gs is based on a soil-vegetation-atmosphere transfer biochemical approach. The representation of photosynthesis is based on the model of Goudriaan et al. (1985) modified by Jacobs (1994) and Jacobs et al. (1996). This parameterization is derived from the set of equations commonly used in other land surface models (Farquhar et al., 1980 for C3 plants and Collatz et al., 1992 for C4 plants), and it has the same formulation for C4 plants as for C3 plants, differing only by the input parameters. Moreover, the slope of the response curve of the light-saturated net rate of CO2 assimilation to the internal CO2 concentration is represented by the mesophyll conductance (gm). Therefore, the value of the gm parameter is related to the activity of the Rubisco enzyme (Jacobs et al., 1996), while in the Farquhar model, this quantity is represented by a maximum carboxylation rate parameter VC,max. The model also includes an original representation of the soil moisture stress. Two different types of the plant response to drought are distinguished, for both herbaceous vegetation (Calvet, 2000) and forests (Calvet et al., 2004). The plant response to drought is characterized by the evolution of the water use efficiency (WUE) under moderate stress: WUE increases in the early soil water stress stages in the case of the drought-avoiding response, whereas WUE decreases or remains stable in the case of the drought-tolerant response. This is achieved through the parameterization of the impact of soil-moisture on gm and on other parameters of the photosynthesis model. The approach for carbon allocation and for phenology is specific to the ISBA-A-gs model and is based on a sim-

ple growth model driven by photosynthesis (Calvet et al., 1998; Calvet and Soussana, 2001). The leaf biomass is supplied with the carbon assimilated by photosynthesis, and decreased by a turnover and a respiration terms. LAI is inferred from the leaf biomass multiplied by the Specific Leaf Area ratio, which depends on the leaf nitrogen concentration (Calvet and Soussana 2001; Gibelin et al. 2006). A more complex version of the model is able to describe the wood biomass and carbon storage (Gibelin et al., 2008). The latter is not used in this study as is has no impact on the LAI and on the plant transpiration simulated by the simpler version. Note that LAI can be either simulated by the model or prescribed to the model using the ECOCLIMAP look-up tables.

REFERENCES:

Arora, V.: Modeling vegetation as a dynamic component in soil-vegetation-atmosphere transfer schemes and hydrological models, Rev. Geophys., 40(2), 1006, doi:10.1029/2001RG000103, 2002.

Gibelin, A.-L., Calvet, J.-C., and Viovy, N.: Modelling energy and CO2 fluxes with an interactive vegetation land surface model – evaluation at high and middle latitudes, Agr. Forest Meteorol., 148, 1611–1628, 2008.

Goudriaan, J., van Laar, H. H., van Keulen, H., and Louwerse, W.: Photosynthesis, CO2 and plant production, in: Wheat Growth and Modelling, edited by: Day, W. and Atkin, R. K., NATO ASI Series, Plenum Press, New York, Series A, 86, 107–122, 1985.

Jacobs, C. M. J.: Direct impact of CO2 enrichment on regional transpiration, Ph.D. Thesis, Agricultural University, Wageningen, 1994.

Jacobs, C. M. J., Van den Hurk, B. J. J. M., and De Bruin, H. A. R.: Stomatal behaviour and photosynthetic rate of unstressed grapevines in semi-arid conditions, Agr. Forest Meteorol., 80, 111–134, 1996.

1.8 [For me Figure 4 was probably the highlight of this paper, as it neatly demonstrates

a point regularly assumed but not often very well illustrated. Some specific discussion of this is worthwhile, e.g. with reference to the set of NLDAS inter-comparison papers (JGR 2004) and similar relevant studies.]

RESPONSE 1.8

Indeed, it was important to show that the rescaling of ERA-I with the GPCC monthly precipitation is needed to produce simulated river discharges close to observations over the considered area. A number of past studies have shown the usefulness of bias-corrected precipitation forcings (e.g. Syed et al., 2004, and Decharme and Douville, 2006b). Using two years of data from the North American Land Data Assimilation System (NLDAS) over the United States, Syed et al. (2004) showed that precipitation dominate the temporal and spatial variability of the hydrological cycle. Decharme and Douville (2006b) quantified the impact of precipitation on river discharge simulations and presented efficiency CDF figures similar to Fig. 4 over the Rhone basin.

REFERENCE:

Syed, T. H., Lakshmi, V., Paleologos, E., Lohmann, D., Mitchell, K., and Famiglietti, J. S.: Analysis of process controls in land surface hydrological cycle over the continental United States, J. Geophys. Res., 109, D22105, doi:10.1029/2004JD004640, 2004.

1.9 [Because Nash-Sutcliffe Efficiency is very sensitive to bias, it was not clear to me what part of the efficiency improvement was attributable to bias reduction alone. Presumably most of it, but comparing it to a figure showing R2 should make that clear.]

RESPONSE 1.9

Yes. Most of the Eff improvement is attributable to bias reduction. For both Fig. 4 and Fig. 5, differences in Qsim/Qobs CDFs are consistent with differences in Eff CDFs. The r2 CDFs are not shown as all the curves are almost confounded.

1.10 [It would seem there is little value in bias adjusting ERA-I-R instead of ERA-I. Please discuss whether that is a trivial conclusion (e.g. if the ERA-I-R scaling method

is near linear) and whether there are practical implications (e.g. is either more easily available?).]

RESPONSE 1.10

The ERA-I-RG bias correction presents slightly better results than ERA-I-G. This is due to the better preservation of small scale features of precipitation provided by the GPCP rescaling method of Balsamo et al. (2010). However, the monthly GPCP data set is not available after 2009, and showing that a much simple bias correction produces nearly equivalent results is encouraging.

1.11 [I am not convinced that the 3 different vegetation schemes produce significantly different agreement with one exception (the seemingly much poorer performance of STD in Autumn; Fig 8). All the other interpretations in my view need some sort of significance test; or at least be stated with stronger caveats.]

RESPONSE 1.11

Yes. Figures 8 and 10 show that the most significantly different Eff values are observed in Autumn, with poorer performance of STD. Deriving general conclusions for the other vegetation schemes is more difficult, as the relative performance of AST vs. NIT vary from one region to another (Fig. 9).

1.12 [Sections 4.1 and 4.2 in particular are very vague and without a clear conclusion; the few concrete interpretations (page 5455 l20-22; 5457 line 21-24; p 5458 line 6-9) seem ad hoc and speculative. Those interpretations need more supporting arguments - alternatively just focus on the one very clear different simulation result (mentioned under d above) and delete everything else?]

RESPONSE 1.12

Section 4 is a discussion section aiming at interpreting the results presented in Sect. 3 and linking to past results. Concrete and quantitative information is provided based on the results of Sect. 3, together with three new figures (Figures 12, 13 and 14).

In particular, the possible causes of mismatch between model and observations are listed in Sect. 4.1 and Sect. 4.2. The main conclusion is that a number of model shortcomings exist (e.g. delay in the leaf onset, misrepresentation of snow) and Sect. 4.3 investigates possible solutions.

1.13 [The comments around Med-CORDEX do not lead to a clear recommendation and so relevance is not clear. Please elaborate or delete.]

RESPONSE 1.13

Yes. This sentence (P. 5459, L. 20-24) is not essential and could be deleted.

1.14 [page 5461, lines 1-6: How is that relevant in this context? Related to this, you seem to make a very ad hoc interpretation of results for one record (Chelif, p 5452) – if it is affected by dams you should not have used it in the first place.. Are there any other rivers affected by dams, and why did you not remove them from your set? Please discuss.]

RESPONSE 1.14

The objective of this paper is highlighting the current limitations of large scale hydrological models, not producing perfect simulations of the river discharge. Among these limitations, there is the lack of a representation of the anthropogenic impact on the continental water cycle.

1.15 [Minor comments]

These issues will be addressed in preparing the final version of the manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 5437, 2012.