

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

### **Anonymous Referee #1**

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#### General assessment

I interpret this m/s to report on an evaluation and sensitivity analysis of a large scale hydrological model. The analysis is focused on Europe, Eurasia and Northern Africa and includes two independent analyses: (I) of the impact of 4 alternative precipitation estimates (the original reanalysis product and 3 bias-corrected versions); and (I) of 3 alternative vegetation parameterisations. In both cases, the streamflow estimates are compared to daily streamflow observations at 150 stations and mean seasonal streamflow patterns at another 46 stations.

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In my view the m/s makes some original contributions that merit publication. This includes evidence that (1) precipitation bias is a dominant source of model underperformance when forced with reanalysis data; (2) this can be successfully mitigated through bias correction with precipitation gauge data to produce model performance that seems quite satisfactory – at least for synoptic large-scale applications; and (3) vegetation parameterisation can have considerable impact on streamflow estimates.

In my view the methods are sound and some of the main conclusions justified and of sufficient interest to merit publication in HESS. However, I do believe fairly major textual revisions are required as outlined below.

## MAIN ISSUES

[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.

[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: (a) 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? (b) 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. (c) With regards to the (unexplained) importance of monitoring systems, how will simulating past droughts with ISBA help develop those? (d) 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) (e) “provided unbiased precipitation data are used . . . simulated river flow can be

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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.

[3] 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. (b) 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. (c) 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.

[4] The Discussion is the most problematic part of this m/s in my view. It is generally rather vague, not well structured, mixes justifiable conclusions with speculative inter-

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pretations, and misses clear statements about the new contributions this m/s makes, even though I believe they are there. Some suggestions: (a) 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. (b) 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  $R^2$  should make that clear. (c) It would seem there is little value in bias adjusting ERA-I-R instead of ERA-R. Pls 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?). (d) 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. (e) 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?; (f) The comments around Med-CORDEX do not lead to a clear recommendation and so relevance is not clear. Pls elaborate or delete. (f) 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? Pls discuss.

MINOR COMMENTS (suggestions, no response expected) 1) My interpretation of the geographical domain mentioned in the title was incorrect. The authors might want to consider something like “Europe, Eurasia and North Africa” instead?

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2) The “land biophysical variables” used in the title is not informative. Would “vegetation parameterisation” be a better descriptor? More in general, because of the missing model description I struggle to interpret what exactly was tested in this case.

3) In my view acronyms used in an abstract should be avoided and if unavoidable, be defined. Not sure what guidelines HESS applies, however, but in any case pls consider that using them limits accessibility of the abstract.

4)Some style and spelling improvements are required. A few examples: a)the verb “unbias” does not exist as far as I know, “bias correct” is what you mean? (the way you use it is confusing as “unbiased estimates” could mean bias was removed or was not there to begin with) b)“interactive LAI” What’s that? Do you mean dynamically simulated LAI? c)“at springtime” replace with “in spring” d)P 5439, line 10: “runoff models” I assume you mean “routing models”? Not the same. Line 13: what about the groundwater you mentioned in line 6? e)The word “indeed” is overused; most instances you can delete no problem. f)“In a first stage” - > “In the first step” or “First,” g)“..on a two prognostic equation” h)Pls explain where the acronyms CNF and MBS come from so the reader might remember which is what or, better still, use more descriptive terms. i)Eq (3) I suppose is written as pseudo-code. I can understand it, but don’t know if it is in agreement with HESS standards. j)“..the downstream of the .. basins” – word missing or style error.

5)Statements that need references or other supporting evidence: a)“Modelling platforms... represent efficient and powerful tools to understand the global hydrological cycle...” b)“Eff coefficient .. is above 0.5 for a fair simulation” What evidence did the two cited studies provide for that? Surely that must be subject to assumptions about the application, and is unlikely to be valid in all environments, catchment & river types, etc? Rephrase?

6)There is some surplus and/or repetitive text, e.g. page 5421 lines 12-12; page 5422 lines 7-10; page 5252 lines 12-17

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7)Minor methodological clarifications required: a)Page 5442, line 5: how was the re-sampling done? b)PIs explain briefly how SAFRAN is constructed, the first time you introduce it. c)Spinning up with only 3 years may well be too short for the deep soil stores in drier regions. Consider having a look at some time traces. d)Section 2.4: why normalised anomalies and not 'straight' RMSD? No problem, but reason required.

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**HESD**

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