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comment

Interactive comment on "The Latest Improvements in SURFEX v8.0 of the Safran-Isba-Modcou Hydrometeorological Model over France" *by* Patrick Le Moigne et al.

Anonymous Referee #2

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Summary:

This paper describes the new developments in the Safran-Isba-Modcou (SIM) hydrometeorological model as part of SURFEX v8.0. These new developments cover different aspects of the SIM system: atmospheric forcing, climate fields, land-surface model parameterizations and water budget parameterizations. The authors evaluate the impact of the new developments in an incremental way using different types of observations. They show that the new SIM system, considering all model changes, improves the simulation of daily river discharges and snow depth over a set of catchments and sites covering the French region, compared to the previous SIM system.

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[Discussion paper]

The discussion of the results is clear in most of the parts and conclusions are overall justified by the results shown. However, in my opinion the authors do not fully discuss the interactions among the different model changes, therefore explaining the physical mechanisms of some of the results. Also, I found the section on the soil temperature evaluation weak compared to the others. Finally, I have few comments on the introduction and model description parts: these sections are a bit difficult to read and can be improved. All these points are discussed in the main comments below. In summary, I would recommend the acceptance of the paper after major revisions, to make the paper stronger and more attractive to readers.

Main comments

- Introduction and model description: Sect. 1 and Sect. 2 are quite difficult to read. Sentences are not clear in some places, making it difficult to understand the message that the authors want to deliver. I would suggest the authors to improve the readability of these sections. Few examples are reported in the minor comments below.

- Discussion of the results: The authors performed a set of sensitivity experiments to extract the effect of each model change. However, I have the impression that they do not fully discuss the interactions among the different model changes, and how these relate to the results of the evaluation. For instance, why the SIM_PHY simulation deteriorate the scores in many of the presented metrics (Fig.8, Fig.9, Fig.11)? Is it because of errors in other components, like the atmospheric forcing, which then penalise a more physically complex model? If that is the case, are the other changes reducing such errors, therefore allowing to fully exploit the benefit from the new soil/snow parameterizations? Or are these unrelated? Another example is in the evaluation of the snow depth: the authors propose several hypothesis to explain the improvement of the simulation of the snow depth in SIM_NEW. However, the comparison of all experiments would clearly quantify which processes/changes are responsible for the improvements (see also one of the minor comments below).

Interactive comment

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Discussion paper

- Soil temperature evaluation: I found Sect. 4.6 on the analysis of soil temperature profiles rather weak and with not enough details. The scores of the new system, without a reference, cannot be put into the context of the paper and so are not adding valuable information to the results. A comparison between the different simulations would clarify at least the impact of each change on the bias. The authors state that such biases can be associated to incoming shortwave radiation or lack of geothermal heating, but what about the soil parameterization or soil/surface properties?

Minor comments

Abstract: The main scope of the paper is placed at the very end of the abstract. This could be placed earlier in the text to make clearer the main message of the paper.

Ln-35–40: This paragraph should be rephrased and clarified. Also references to previously published work on the evaluation of land surface and hydrological models should be introduced in the text.

Ln.42: What do you mean by "independent" variable? As the authors stated few lines before, surface energy and water budgets form a coupled system. Please clarify/reformulate.

Ln. 49-50: "... , where modelling contribution of SVAT . . . accounted for in models." this sentence is not clear, please reformulate.

Ln.65-73: The scope of the work can be improved, to make it more precise and easier to read. For instance the authors talk about "new parameterizations" at line 66, but changes to the atmospheric forcing (Ln. 68) are not a parameterization. Also at Ln.69 they talk about "these results", but it is unclear which results are referring to at this point.

Ln. 83: could you be more specific on the horizontal resolution of the SAFRAN analysis?

Ln. 83: is the 24h precipitation analysed every 6-hours?

Ln. 154: please clarify in which sense you mean "dry", as this can be related to various processes.

Ln. 163-167: this paragraph should be simplified/clarified, to make it clearer that the soil map is not changed between the previous SIM system and the new system (as far as I understood).

Ln. 166-167: please clarify that this map was the one used in the "old" version of SIM.

Sect. 2.3: as far as I understood, the main change in the climate fields is the update to ECOCLIMAP2. Hence, I would suggest to expand the discussion of this change, for instance explaining the impacts on the ISBA model? See also next comment.

Ln. 187-193: this sentence is very long. Coud you break it in multiple sentences, better explaining the impact of this change?

Sect. 2.4, title: could you specify in the Section title that this is downward infrared radiation?

Ln. 195-198: is the bias related to a problem in the analysis (for instance cloud cover) or a RT model issue?

Ln. 214: annual mean over which time period?

Ln. 215-217: could you clarify this sentence? is the analysis done every 300m in the vertical direction?

Ln. 233: why between 3 to 5 layers are necessary, and not more? What it is the vertical discretization between each band? Please clarify.

Ln. 264: How does the relatively low horizontal resolution of the ERA-40/ERA-I data impact the simulations? The horizontal resolution of ERA-I is ∼80km, that is one order of magnitude less than the one used by the SIM grid. I am thinking for instance at regions with a low coverage of surface stations used in the analysis.

Ln. 299-300: were the data cleaned in some way? For instance removing stations with a few number of observations? Or all data have been used to compute the statistics? The latter could introduce some artifact in the statistics. This should be better explained in the text.

Ln.318-320: could you specify clearly in the text when the transition from ERA-40 to ERA-I occur in SAFRAN?

Ln. 335-345: What is the reason for the deterioration in the lower part of the CCDF of NSE in SIM_PHY?

Sect.4.3: I would suggest to rename this subsection as it is quite vague at the moment: most of the paper regards the comparison to old SIM.

Ln.405: why not adding a third box for SIM_PHY to evaluate the effect of the new snow/soil schemes on the snow depth?

Ln. 413: "baresoil"–> bare soil

Ln.414: At which depth the soil temperature observations are taken? Is any interpolation applied to the data?

Ln.440-449: It would be nice to explicitly link this discussion on the changes of Evap/Precip with the changes in the discharge mean bias.

Ln. 489: I would rephrase this sentence for readers not familiar with detailed snow models.

Comments on the figures

- Generally, the figure captions should be improved to make them more self-explanatory.

- In the caption of Figure 1, the definition of a "mountain grid cell" should be added.

- Some of the figures could be merged together, for instance Fig. 7 with Fig. 8 and Fig.

9 with Fig. 10, for conciseness.