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Hydrology and Earth System Sciences Discussions

Interactive comment on "Improvement, calibration and validation of a distributed hydrological model over France" *by* P. Quintana Seguí et al.

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The authors of the article are thankful to referee 1 for the appropriate comments and interesting questions, which helped to improve the overall quality of the manuscript. All his questions have been answered and his comments addressed in the new version of the manuscript.

Answer to Specific Comments

- The paper is unduly long and the authors have spent a lot of

effort in reviewing the evolution of the model structure. I suggest that the authors remove this section and present the model structure in its present state and only focus on the need for the new addition they introduced.

It is true that this section is long and can be shortened. Nevertheless, it is important to put the evolution of the model structure into context. SIM is not a typical hydrological model and there is value in explaining how it was conceived and how, driven by the need to improve the model, the model that was conceived as simple but physically based, was modified to include empirical parameterisations, with their subsequent empirical parameters. One third of the section has been removed in the new version of the article.

- It is mentioned on page 1329, lines 5-10 that the model has a clear structural problem. The authors have mentioned that in the present state of the model structure, water that should be taken from the aquifer is artificially taken from the soil reservoir. They have also suggested possible remedies to this problem but have not addressed it. The question is: what is the point of introducing an improvement to the representation of the hydraulic conductivity in the soil zone when it is known that the model has the mentioned

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structural problem in handling the interaction between the soil zone and the aquifer? Why not first address the known structural problem using the approach suggested by the authors?

Yes, the SIM model has a structural problem related to the description of the interaction between the soil zone and the aquifer. The parameterisation used at present (sub-grid drainage) is not satisfactory. But this is not the only structural problem of the model. The first paragraphs of Section 4 describe another important structural problem: drainage is too slow and a second peak of discharge is produced after each main event, which is not satisfactory. Other structural problems are also present: the lack of an adequate description of karst aquifers, the lack of dams and reservoirs, etc. In this study, one of these problems was addressed: the description of hydraulic conductivity was improved and the form of the hydrographs and the related scores improved considerably. Hopefully, the other problems will be solved in the future.

Referee 1 suggests that it would be wiser to solve first the problem related to subgrid-drainage and then improve the representation of hydraulic conductivity. In the following paragraphs, it will be shown that this is not necessarily true.

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The parameterisation of sub-grid drainage only plays a role in those basins where there is an aquifer that is not simulated by MODCOU. This limits the scope of the parameterisation. Furthermore, sub-grid drainage is mainly relevant for low flows, which further reduces the scope of the parameterisation. On the other hand, the improvement of the description of hydraulic conductivity is important everywhere, independently of the presence or not of an aquifer. Furthermore, changes in the description of hydraulic conductivity not only improve low flows, but also other characteristics of the simulated hydrogram. For example, as Fig. 1 shows, the model better reproduces peak discharge after introducing the new parameterisation. As a consequence, the improvement of hydraulic conductivity, has wider consequences than the improvement of subgrid-drainage.

In the first paragraph of page 1325 it is explained that MODCOU simulates the aquifers on the Rhône and the Seine basins, therefore, in these basins, there is no sub-grid drainage. In these basins, the quality of the simulations, in terms of efficiency, improved, as Figure 11 shows. The quality of the simulations also improved on many other basins where no aquifers are present.

Therefore, even though the problem of sub-grid drainage remains, the model took benefit from the improvement of the description of hydraulic conductivity. Of course, the parameterisation of sub-grid drainage should be substituted by a better parameterisation in the future.

This discussion has been added in the *Discussion* section of the new version of the article.

- Section 6.1, last paragraph: Model calibration is performed for parameters f and dc leaving the other parameter b out first and a second round of calibration is performed by tuning parameter b. Why did not the authors calibrate the model for all the three parameters simultaneously? I do not understand why the authors left this parameter out based on its sensitivity to evaporation. Calibration was done based only on runoff data. I think the authors should clarify this point.

The authors agree with the referee that it would be better to calibrate the three parameters together. But, in this study, it was not possible to calibrate the three parameters together because of the computational cost of such a strategy.

SIM is a distributed model that simulates the whole of France for long periods of time. The time step of ISBA is of 5 minutes and it needs an important amount of data to describe the characteristics of 9892 cells and their meteorological forcings. Therefore, it takes a long time to

perform a simulation in the main super-computer of Météo-France.
To calibrate together d_c and f, 29 simulations were performed. Afterward, to calibrate b, five further simulations were also performed. To calibrate the three parameters at the same time, 29*5 simulations should have been realized. This number could be reduced with some optimisation, but the final number of simulations would have been too high for the context of the study.
It was decided to leave b for a second turn of calibration, because, as it is stated in section 5.2, *the outputs of the model were less sensitive to the shape parameter of subgrid runoff than were to the parameters related to the exponential profile of hydraulic conductivity. As expected, evaporation was not very sensitive to this parameter (Fig. (6))*. Therefore, it was decided to calibrate the parameters to which the model is more sensitive, and to fine tune the result with the calibration of b.
The fact of using only runoff data is justified because this is the only variable that is available evenly all over France, and the series of observations are long. Furthermore, this variable is representative of the whole basin. Of course, it would be desirable to calibrate with other observations, but unfortunately there are important problems. For example, evaporation is measured in few points and there

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are no long series available, the same problem applies to soil wetness. There have been improvements in the measure of soil wetness using remote sensing techniques, but, again, there are no long series available, and the techniques are not mature enough yet. Hopefully, in the future there will be more data available to calibrate distributed models like SIM.

The last paragraph of Sec. (6.1) was improved according to this discussion.

- In their conclusion, the authors have suggested a need for the introduction of more parameters to understand the role of interaction between the parameters. The interaction could have been studied using the present parameterisation. What is the point of adding more parameters and how would that help to understand the role of parameter interaction?

In the conclusion it is stated that *in the future it would be desirable to study if it is worth introducing new parameters to the calibration and to better understand the role of the interaction between the parameters, being conscious that each new degree of freedom can put in danger the reliability of the model*.

This statement is not very clear. By this, the authors mean that maybe more parameters could be calibrated, but not added to the model. For

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example, data about root depth is scarce. In SIM, in general, the value of the root depth is equal to 2/3 of the soil depth. This value is arbitrary, even though is plausible in many cases. Maybe the calibration of theses parameters would improve the results of the model, but, at the same time, the authors are conscious that any new degree of freedom of the calibration can lead to new problems, like the existence of several optimal sets of values, etc.

The study of the dependence of calibrated values of the parameters, to the values of other parameters, for example, how d_c changes when root depth is modified, would eventually allow to understand these dependencies and therefore to simplify the model.

This part of the conclusion has been re-written.

Answer to Minor Comments

- *What does the compacted depth dc mean physically?*

Mathematically, the compacted depth (d_c) is the depth at which $k_{sat} = k_{satc}$ (see eq. 13). k_{satc} is the value of hydraulic conductivity obtained from empirical relation often used in such models. These relations derive hydraulic conductivity from the textural properties of the soil. Therefore, d_c has not a strong physical meaning, it just tells at which depth hydraulic conductivity is equal to a reference value.

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d_c could be further interpreted as the depth where macropores caused by the presence of biomass cease to exist and the soil starts to be compacted significantly. But the exponential profile of hydraulic conductivity is an empirical relationship that more realistically reproduces the changes of hydraulic conductivity in the soil than the constant value used before. But it does not pretend to physically describe this variable.

A new phrase has been in Sec. (4) to clarify this point.

- *Section 5.1, last paragraph: What is the cause of the seasonal pattern of the sensitivity of evaporation and drainage to hydraulic conductivity? I think this should be discussed.*

Evaporation: Sensitivity is higher during two periods: spring and autumn. The minimum is in December and August. Evaporation is more sensitive to f and d_c when it is driven mainly by processes which are related to the soil. For example, the activity of the vegetation, which pumps water from the root zone. The maximum of the vegetation is in spring. In summer (August), the activity diminishes because of soil water stress. In winter, the activity also diminishes because of the phenological cycle of the plants.

Drainage: Drainage is maximum and more sensitive when there is enough precipitation to fill the soil and evaporation is low. It is in these

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situations that the soil is wetter than field capacity. This two requirements are fulfilled in winter.

These comments have been added to the article.

- Section 5.2: Why not include a figure to show the influence of b on the annual cycle of drainage and runoff?

This figure was not included because the article already contained many figures. Now it has been included.

- Section 6.4, last paragraph is not necessary. The objective functions have already been defined in section 6.1.

This paragraph has been removed from the article.

- Section 7.1: Why not include a figure to show that the model performance in terms of reproducing the water balance is similar in the two periods?

A new figure has been introduced. It shows an histogram of the overall water balance for two the two periods and two simulations.

Answer to Technical Corrections

- Section 2.2, line 26: “momentum” instead of “moment”

This has been corrected.

- Equation 1: Shouldn’t the subscript of w be 1?

Yes. It has been corrected.

- Section 3.3, line 19: remove the question mark and put the

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appropriate citation. Also on first line of section 4.1 and on line 9 of page 1337.

It has been corrected.

- The authors should also revise their grammar. For instance on line 7 of page 1326: "The values of d2 and d3 were set in function of the vegetation type ..." can be rewritten as: " ...as functions of ...". Also, I don't see the need for the comma on line 7 of page 1334. The statement on lines 23-25 of page 1335 can be rewritten. There are many similar incidents in the text where sometimes it is difficult to grasp what the authors intend to say.

The authors have done their best to improve the quality of the English language used in the article.

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