Submission of new manuscript version

UNCERTAINTY REDUCTION AND PARAMETERS ESTIMATION OF A DISTRIBUTED HYDROLOGICAL MODEL WITH GROUND AND REMOTE SENSING DATA

by F. Silvestro, S. Gabellani, R. Rudari, F. Delogu, P. Laiolo and G. Boni

Dear Editor and Reviewers,

We uploaded a new version of the manuscript trying to insert the requests and the suggestions of both the reviewers and of the editor. We highlighted in yellow the part of the manuscript where major changes have been applied

Please find below the replies to editor's and reviewers' comments and the main changes done to the manuscript.

We modified the structure of the chapters basing on the suggestions of the editor.

We thank you for your useful comments and suggestions that helped us to improve our work.

We hope that the manuscript is now publishable in HESS.

Best regards.

Editor

Two reviewers re-assessed the manuscript and mention that the manuscript has improved considerably. However, they both have still issues that need clarification or improvement. Beside the comments by the reviewers I also reassessed the manuscript and based on my findings I think you still need to do quite some work on rearranging the manuscript. The manuscript needs further improvement regarding the seperation of methods and results.

Section 2.3 is a mixture which is unacceptable And Part of chapter 3 page 19 line 3-20 and page 19 line 2330+page 20 line 1-2
and page 22 line 3-9 need to be merged with paragraph 2.4
to seperate the methods from the results
I advise to make re arrange according to
2 Material and Methods
2.1 Model Overview
2.2 Data sets
2.3 Experimental Setup
2.3.1) Uncertainty analysis
2.3.2) Calibration experiments

=> re arrange 2.4 + part of 3
2.3.3) Verification measures
- streamflow
- LST
- SWI
3 Results
3.1 Uncertainty analysis
3.2 Calibration Experiments
etc
Please also check the complete manuscript with a check on the use of past and present tense (for example page 10 2.3 I think should be in present tense)
Note that Discussion and conclusion has the wrong number
Page 23 line 13 what do you mean by encouraging ~ vague
Page 12 line 6 begins with .
Last but not least you now focus in the evaluation on NS and RMSE, CORR etc
I think besides this it would make sense to evaluate on peak flows as this is in many applications relevant

Replay: The structure of the manuscript has been changed following the suggestions of the editor and the other corrections have been done. We propose to put the section with the statistics and scores before the experimental set up.
A statistic based on peak flows has been added as requested.
The sentence on Page 23 line 13 (..encouraging..) has been modified.

Reviewer 1

General comments

The authors have made a great job at improving their manuscript in their revision and I congratulate them for this. They have properly addressed all the comments and suggestions I made in my first review. I find this version easier to follow than the previous one. The paper is now better organized, the objectives are clearly explained and the methodology can be easily followed. In my opinion, their contribution is interesting and their results deserve publication.

I only have minor comments:

On figure 4, a different behaviour is observed for high and low values of Uc and Uh. While for values of Uc greater than 35 and values of Uh greater than 9 the values of the NS coefficient seem to be uniformly distributed over the range, for lower values of Uc and Uh the NS coefficient converges to high values. I think this circumstance deserves a discussion.

Replay: We added some comments in the uncertainty analysis results (section 3.1) as suggested by the reviewer.
"…For values of uc greater than 30÷35 m0.5s-1 and values of uh greater than 7÷9 s-1 the values of the NS coefficient seem to be uniformly distributed over a large range, this indicates that different combinations of the two parameters can lead to very different performances. For lower values of uc and uh the NS coefficient converges to high values highlighting a minor variability of the score and

general better performances....”

With the metrics used by the authors, it seems that Cf parameter is not observed properly with the data available. By looking at figures 4, 8 and 9, one can see at least one plot where the skill score (NS coefficient or bias) shows clear sensitivity to parameter values for parameters Uc, Uh and Ct, suggesting that these parameters have a clear effect on the observations. However, for Cf most plots show a uniform distribution and only in Figure 9 d) a weak sensitivity is appreciated. Does this mean that Cf cannot be observed with the metric selected? What are the hopes of getting a good calibration of Cf with the available data? Please discuss.

Replay: It mainly means that cf does not tend to be directly related to the observed variables so different combinations of ct and cf lead to similar results. So, especially when using LST only, calibration is still possible but without a substantial equifinality reduction. We tried to show these findings in a comment of section 3.1 and here reported:

“....This ends up increasing the set of equifinal parameter set when only LST is used, on the other hand ct and cf influence in a complex way the different terms of the multi objective function during the calibration process driving to the direction of a reduction of equifinality. When using the R.S (LST) strategy the benefit of exploiting LST data seems more related to opportunity of doing a calibration in case of lack of streamflow data than in reducing equifinality....”

Typographical errors:
On page 2, line 1, change "techniques that allows exploiting" into " techniques that allow exploiting "
Corrected
On page 4, line 9, remove comma in " This kind of data is by now, available "
Corrected
On page 13, line 14, change " especially if the parameters uc versus uh is considered" into " especially if the case of parameters uc versus uh is considered "
Corrected
On page 22, line 27, change "2. DISCUSSION AND CONCLUSIONS" into "4. DISCUSSION AND CONCLUSIONS"
Corrected
On page 24, line 12, change "Remotely sense data can" into "Remotely sensed data can"
Corrected
On page 24, lines 13 and 14, change "an alternative way of calibration these models where standard observation are lacking" into “an alternative way of calibrating these models where standard observations are lacking"
Corrected
On page 53, line 5, change "This occur even for parameter cf even the independence is weaker" into "This occurs even for parameter cf although the independence is weaker "
Corrected
On page 55, line 2, change " Nash Sutcliff coefficient" into " Nash Sutcliffe coefficient"
Replay: We *corrected the errors.*

Reviewer 2

Summary: The manuscript has improved after the last round of revisions and the authors did a good job in including the comments of the previous review. However, some issue remain that I would like to see addressed before publication.

Major issues:

Page 9 Line 21: Why is the SWI with a lag-time of 10 days selected? This seems rather arbitrary to me and I think the authors should explain to the reader why 10 days was selected. If this represents the soil moisture dynamics in the catchment or that is the standard product of any other reason. Preferably the 10 days is selected because it mimics most closely the observed soil moisture dynamics.

Replay: We added a sentence in section 2.2:
"Since in situ soil moisture measurements are not available and the soil properties are not known quantitatively with high detail, the parameter T has been set to a priori value that has been estimated, as order of magnitude, using the definition of T of Wagner et al. 1999 and used also in Parajka et al. (2006) based on the mean soil characteristics of the considered catchments as described in the model (the average potential soil moisture capacity of the considered basins is around 150-170 mm, assuming a porosity of 0.3, a pseudo diffusivity of 10 days would then translate into a wetting front celerity around 50 mm per day that is a reasonable value for these soils)."

I did not notice it last time, but a comparison to existing studies is missing from the discussion. How does this work relate to other studies on for example SM calibration? For example what is the overlap between this work and the work of Montzka et al (2011), Sutanudjaja et al (2014) and Wanders et al (2014). They all use soil moisture observations to constrain the model calibration. Please mention this in the discussion. Where is this work different and how does it related to these existing studies. Furthermore, I would do the same for LST calibration.

Replay: These are interesting works and we added the references (Sutanudjaja et al., 2014 and Montzka et al., 2011 were already cited) and comments that mention these works in the text (section 2.4.2.2):
"…. This approach follows and improves the investigations carried out by other authors (Crow et al., 2003; Santanello et al., 2007; Koren et al., 2008; Flores et al., 2010; Montzka et al., 2011; Ridler et al., 2012; Corbari and Mancini, 2014; Sutanudjaja et al.; 2014; Wanders et al., 2014) who attempted to combine, in the calibration process, remote sensed and in situ observations of variables other than streamflow....".
Some of these works are now mentioned even in the discussion and conclusions section.
"….The first methodology consists of the minimization of a multi-objective function that depends on streamflow, LST and SWI and it is inspired by the work of Sutanudjaja et al. (2014) and Wanders et al. (2014)…."
".., similarly to what demonstrated by other authors (Corbari and Mancini (2014); Montzka et al., 2011; Sutanudjaja et al., 2014; Wanders et al., 2014), they highlight the opportunities given by remote sensing..."

These works are on the same topic and have similar objectives (using different sources of data to estimate parameters…) of our work.
But we used three sources of data (LST, SWI, discharge) performing an uncertainty analysis to relate the model parameters and the data; we also applied the methodology on different

environments and different size catchments expanding some concepts of the aforementioned works.

Regarding the use of LST in calibration process, we already cited our previous work (Silvestro et. al, 2013) and we now added on more reference Corbari and Mancini (2014).

Minor issues:

Page 3 Line 17-20 is still unclear. The authors write about increased data availability and then about a data shortage. Please rephrase to more consistent.

Replay: We rephrased the sentence:
"In a world where the data sharing capacity is increasing, it seems that the problem of data shortage for hydrologic models calibration is not going to disappear; the level gauge stations can be a limited number and in some areas are very rare, additionally in some cases the access to river discharge information have been declining since the 1980s (Vörösmarty et al, 2001)"

Section numbering of Discussion and Conclusion and Acknowledgements is incorrect.
Replay: Corrected

Montzka, C., H. Moradkhani, L. Weihermuller, H.-J. H. Franssen, M. Canty, and H. Vereecken (2011), Hydraulic parameter estimation by remotely-sensed top soil moisture observations with the particle filter, J. Hydrol., 399(34), 410–421, doi:10.1016/j.jhydrol.2011.01.020.

Sutanudjaja, E. H., L. P. H. van Beek, S. M. de Jong, F. C. van Geer, and M. F. P. Bierkens (2014), Calibrating a large-extent high-resolution coupled groundwater-land surface model using soil moisture and discharge data, Water Resour. Res., 50, doi:10.1002/2013WR013807.

Wanders, N., M. F. P. Bierkens, S. M. Jong, A. Roo, and D. Karssenberg (2014), The benefits of using remotely sensed soil moisture in parameter identification of large-scale hydrological models, Water Resour. Res., 50, 6874–6891, doi:10.1002/2013WR014639.