Hydrol. Earth Syst. Sci. Discuss., 11, C3716–C3722, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C3716/2014/

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Uncertainty reduction and parameters estimation of a distributed hydrological model with ground and remote sensing data" *by* F. Silvestro et al.

Anonymous Referee #2

Received and published: 11 September 2014

General comments

The authors present a modelling experiment on two Italian basins to show the potential of including remotely sensed data to improve the calibration of a hydrological model. Their work is based on a sensitivity analysis of model parameters and on the assessment of model performance for four calibration strategies using several skill scores. The main contributions are the methodology to incorporate satellite observations to model calibration and some conclusions extracted from the discussion of results.

The research topic is interesting and the results are relevant. In my opinion, the work

deserves publication in Hydrology and Earth System Sciences. However, the paper needs more elaboration before it can be finally published. The manuscript is poorly organized and many details require additional polish. It lacks a clear strategy for presentation of the work and the authors mix methodology and results too frequently. Some figures need additional work.

The presentation of the work needs improvement. I suggest a clear division between presentation of methodology and results as applied to the two case studies. The authors should clearly state their objectives at the beginning and devote a methodological section to present their strategy, indicating the alternatives to be compared and the metrics that will be used in the comparison. From the text I gather that the analysis is focused on a sensitivity experiment to learn about parameter uncertainty and a calibration experiment to learn about parameter estimation. Sensitivity analyses are performed on streamflow, land surface temperature and surface soil moisture. The calibration strategies compared are four: 1) Calibration with streamflow alone, SN; 2) Calibration with land surface temperature, RS (LST); 3) Calibration with surface soil moisture, RS (SWI) and 4) Multiobjective calibration, MO. I would have liked to see an orderly presentation of the approach followed on these analyses, together with a clear definition of the metrics used to evaluate the performances. Then the two case studies can be introduced and the results presented, followed by the corresponding discussion.

Apart from the lack of structure, the manuscript is not properly finished. There are too many typographical errors that indicate that the manuscript is still at an early stage of revision. The authors should have checked their manuscript for such evident errors before submission. You may find below several of such errors but the list is not complete.

Specific comments

The abstract needs reworking. It should focus on the new material presented in the paper rather than on general comments (first paragraph). The statement that satellite observations “dramatically” reduce uncertainties in parameters is not supported by the results presented in the paper, since the improvement obtained in performance metrics while using remote sensing is not substantial. The paper compares four calibration strategies, not two. The results obtained and the conclusions drawn should also be mentioned in the abstract, since they are the most relevant information for the reader.

The basic calibration parameters of the Continuum models should be presented and discussed in more depth. Table 1 presents the six calibration parameters of Continumm, with an indication of the physical process parameterized. However, I would have liked to see a deeper discussion of the physical meaning of parameters themselves. There are three adimensional parameters (cf, ct and Rf), but these are just numbers. I can speculate on the physical meaning of Rf, if it describes the ratio between vertical and horizontal conductivity, but I have no clue on the role of cf controlling infiltration capacity at saturation or the role of ct controlling field capacity. Likewise for the dimensional parameters: flow motion in hillslope is controlled by a parameter with units of inverse time while friction in channels is controlled by a parameter with more complex units. In order to properly understand the discussions, the reader should be presented with a detailed explanation of calibration parameters and their role on model equations.

The authors state the Continuum is a distributed model, but calibration parameters are lumped. How are they related to the physical properties distributed over the basin? Do the same calibration parameters apply to forests, grassland and cultivated areas? How do their values affect, for instance, local soil properties? The author is referred to the original paper by Silvestro et al., 2013, but in that reference there is little discussion of the role of these parameters on calibration, which is the central topic of this manuscript.

On section 4, four sources of remotely sensed data are presented, but it is not clear to me how Leaf Area Index data were used. Please explain.

By looking at Figure 7 it appears that LST is as insensitive to parameter cf as it is

insensitive to parameters uc and uh. A similar situation occurs in Figure 8 with respect to SWI, although here SWI shows some sensitivity to cf. However, LST and SWI are later on used to estimate parameter cf. Is this possible? Please discuss.

Overall, I find the discussion of results in section 4 quite weak to support the conclusion stated on page 12, lines 7-8. The plots shown in Figures 5 and 6 are very confusing. They indeed convey an image of equifinality. However, the statement that calibration with remotely sensed data can help to reduce equifinality is not clear to me. Optimal values are only apparent for parameter ct under LST calibration but the optimal values corresponds to low values of the probability distribution shown in Figure 6. This would mean an additional peak in the joint distribution, increasing equifinality. Optimal values for ct under SWI calibration do coincide with peak values in Figure 6, but the maximum for ct in Figure is very weak and it is unlikely to reduce equifinality. The discussion of results should be further elaborated if that conclusion is proposed from the analysis.

Mathematical rigor is a must in scientific papers. Performance metrics should be mathematically described, even if they are common: On Equation 3, the meaning of <Q0> is missing Expressions for the four performance metrics proposed (CM, RMSE, Corr, Rel.Err) should be included. Same for Bias, introduced later on (in absolute values?). Expressions for computing the basin “Saturation Degree” and “Mean land surface temperature” should be provided. Are they temporal or spatial means? What is their time step compared to that of the model? The second term of the objective function presented in Equation 7 (page 12) has dimensions of temperature. How can it be added to the other three adimensional terms? The objective function as shown in Equation 7 is never applied in the paper. Why not define the individual terms Fi and then integrate them in Equation 8? From the description of the calibration periods (see below) it is not clear if all components of F are summed over the same time interval from t=1 to tmax. Please clarify.

Equation 9 is not clear. What is Fj,min? How can Ai depend on Fj,min?

How are time series sorted according to NS to produce Figure 4? How is it related to the estimation of percentiles every time step?

Why choosing asymmetric confidence limits (15% and 90%, page 10, line 19) for Figure 4?

How does the normalization of the four components of Fadj work? Do any of them have more weight than the others? A brief discussion is appropriate.

How are the observed values of remotely sensed data compared to simulated values in the basin? What is the temporal and spatial resolution in both cases? There are certainly scale problems associated to the procedure. Please explain, including mathematical expressions if possible.

The periods chosen for the different sensitivity analyses and calibrations in the two case studies differ without a clear justification and it is very difficult to even realize the differences. A systematic listing of such periods would be advisable. They are ambiguously described, both for the Orba and the Casentino basins. There are four calibrations (tables 4 and 8), while the text mentions only three calibration periods. What is the calibration period for MO? Is it a mixture of the three individual periods or were the four terms of the objective function applied to the same period? Please clarify. The chosen calibration periods do not even cover a full year, which is surprising considering the strong seasonality of hydrologic processes. If the calibration periods were chosen because of limited data availability, the authors say so. If they were chosen for other reasons, they should be clearly presented. The reference to “different flood and drought regimes” (page 17, lines 5-6) is too vague and does not convey real meaning.

The last paragraph of page 17 is not a conclusion. It should be removed from the conclusions section.

Minor details

Section numbering is not correct. We find section 1.1 under section 2, sections 1.2 and

1.5 under section 5, sections 1.1.1 and 1.1.2 under section 1.3 and section 1.4 and 1.2 under section 6

Figure captions are too schematic. They should be elaborated to identify exactly what is being represented.

On pages 11-12, lines 27-28 and 1, it is stated that Figure 8 shows the comparison of the model saturation degree and satellite SWI. On the caption, Figure 8 is described as representing NS vs NWI. However, Figure 8 actually shows a plot of NS versus parameter values. Please clarify.

The first sentence of the second paragraph of page 12 (lines 3-4) is not clear. It mentions the maximum of ct while it may refer to the maximum of NS.

On section 5, a subsection is devoted to calibration strategies MO, RS (LTS) and RS (SWI). Another subsection should be devoted to SN.

In some figures it is difficult to interpret the time axis. Rather than showing full dates at random I suggest to choose time tags and the beginning of months or years to facilitate the interpretation of the plots.

Three calibration strategies are mentioned on page 15, line 19, while Table 4 shows four.

On page 15, lines 25-26 a reference is made to hydrographs obtained with the best parameter sets. Where are they shown?

The notation should be consistent. The parameter uh is mentioned as uv in the text and in several figures.

On tables 4 and 8, parameter uc is mentioned two times.

Typographical errors: On page 3, line 8, change "significantly" into "significant" On page 7, line 81, change "areas" into "area" On page 11, line 12, change "estimate" into "estimation"

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