September 13., 2012

Answers to the review of HESSD Manuscript
'Continuum: a distributed hydrological model for water management and flood forecasting'
By F. Silvestro, S. Gabellani, F. Delogu, R. Rudari, and G. Boni,

Dear Reviewers dear Editor,

In the following we report the responses to the reviewer 2. Some issues are discussed also in the responses to the other reviewer.

We thank both reviewers for their accurate and frank review and used their precious suggestions to improve the paper. We tried to answer to all the comments made and we are ready to prepare and submit a new version of the manuscript.

The point by point answers are written in italics.

With our Best Regards,

Francesco Silvestro
Simone Gabellani
Fabio Delogu
Roberto Rudari
Giorgio Boni

Reviewer #2:

In the following the answers to reviewer 2 are shown. We reported the responses to the three main points (titles) reported in the review, since initial and final comments mainly report the summary of these latter.

* multiobjective calibration

I find the section on model parameterization quite confusing (How is the short period calibration brought forward to the global calibration, what sampling method was used, which prior parameter distributions, how many model evaluations etc) but essentially the model is calibrated in a classic way on streamflow using the Nash-Sutcliffe efficiency as a performance measure. The surface temperature is then compared with surface temperatures obtained from satellite data.

Again, this approach is little informative. It is not surprising that the modelled surface temperature is in good agreement with the observed values, especially if air temperature is used as a model input. In fact, without plotting air temperature values in Fig. 15 and 16 it is very hard to evaluate the contribution of the model. Instead, it would be much more interesting to explore the possibility of assimilating the observed LSTs and evaluate their potential contribution to constraining model parameters and internal states and improving (if at all) streamflow predictions.

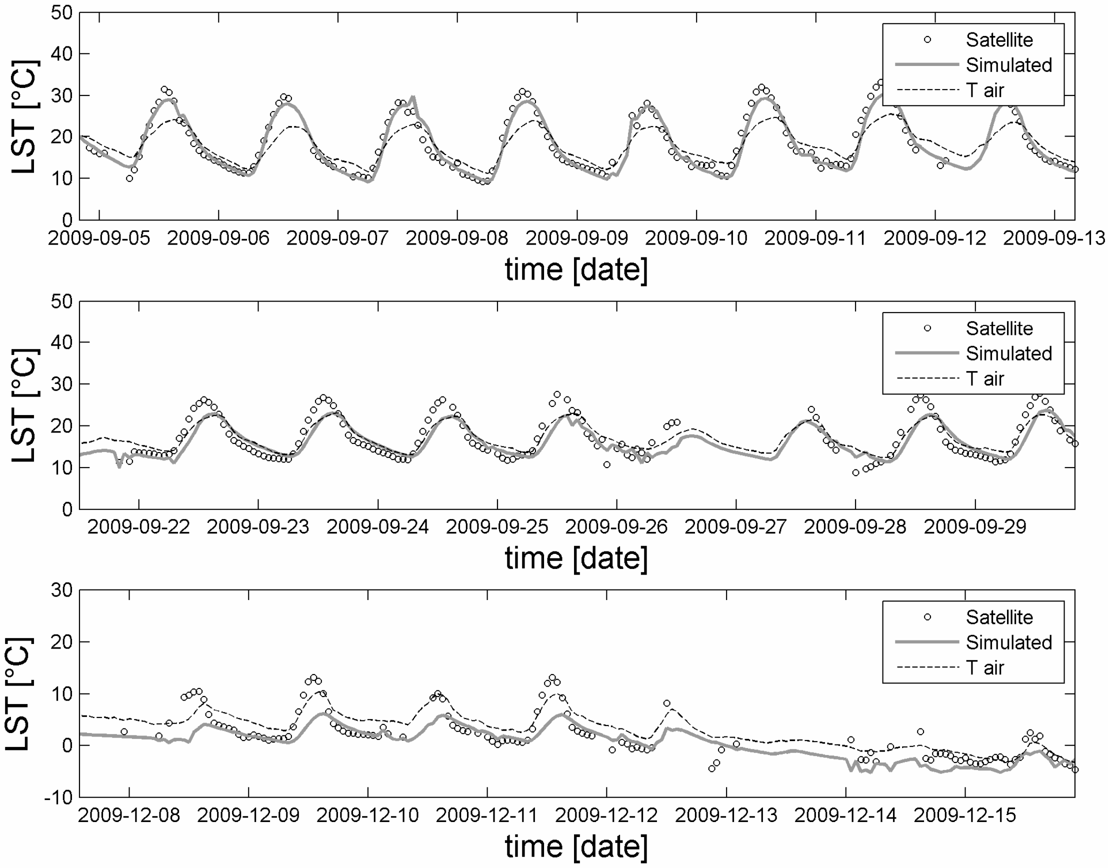
We agree with the general comment of the reviewer that is in line with what was highlighted also by reviewer 1, in that we could use LST not just as a validation control but more for driving the model parameters calibration. We propose a revision of the paper in this direction using LST as a calibration variable, even if in a more traditional fashion than a data assimilation approach as proposed by the reviewer. We hope that this would satisfy the understandable concern of the reviewer that the paper would bring something new in the scientific panorama.

The objective is to prove that LST is a constraint that can be used to drive a calibration procedure thanks to the model structure. This has a strong impact especially in environments where reliable stream-flow data are not available, given the worldwide availability of LST data.

This has an additional meaning even in data rich environments. In fact, even if the improvement of streamflow prediction is certainly one of the main tasks to pursue with an hydrological model, hydrologists should not concentrate on this objective alone; different Soil moisture distributions could lead to similar stream flow values especially in flow regimes where the stream flow observations present the higher uncertainties. In this case the distributed nature of LST as an independent constraint to be used for calibration could mild this problem, linked to the equifinality of the model.

However, there is one important point of the reviewer comment that needs to be better discussed, so that we can agree on the new cut given to the work. The reviewer states that Model LST and observed LST agreement is largely due to the use of T_{air} as a model input. In the used formulation, as well as in many other explicit formulation to compute energy balance, T_{air} is in fact a very weak constraint. T_{air} intervenes in determining only the sensible heat flux which is by far the less efficient way the system has to exchange energy. Therefore the strongest control in the energy balance system equation is soil moisture. The fact that Tair and LST seem so closely related is because of the Radiation input that is the main driver of the equations. The cycle is then linked to Rn while the amplitude of the LST cycle is modulated by soil moisture.

In Figure 15 and 16 the Air Temperature can be plotted (this will be added in the new version of the manuscript) (See the new figures below). From the figure the model added value in simulating LST is evident and it is at the basis also of the possibility of using LST as a calibration constraint also in a DA framework as it is rightly suggested by the reviewer.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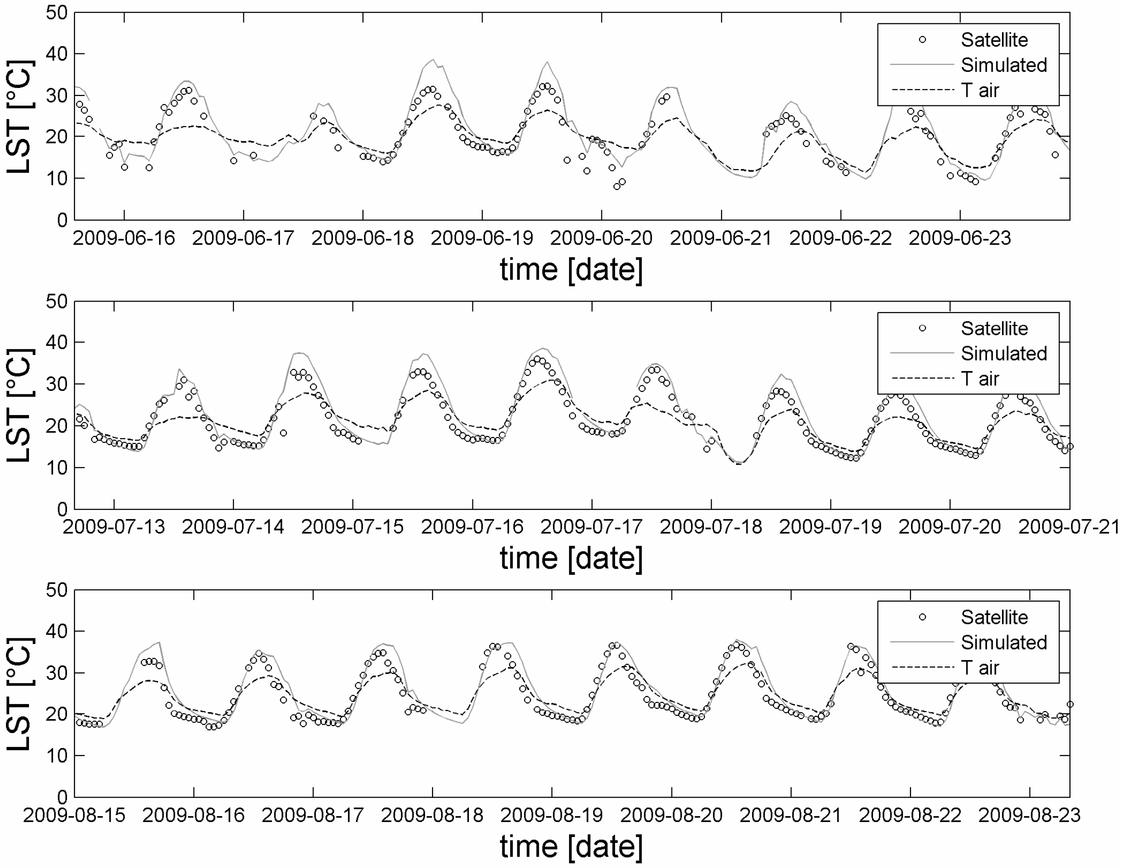


overparameterization

The model claims to be a balance between representing physical processes and avoiding overparameterization, but very little argumentation is given for this claim. The model is probably no less overparameterized than any physical model of which all but a few parameters are kept fixed. It seems that the reason for doing so is the availability of satellite-based information about land-cover (LAI, and presumably also CH?), from which relevant parameters are obtained, and keeping many (often empirical) parameters of the various equations constant. Also, despite the claim that it is a distributed model, soil parameters seem to have been kept homogeneous over the catchment.

This of course limits the parameters to be calibrated, but also greatly reduces the ability to represent spatial patterns and thus the claim of being a distributed model. This approach of course suffers from the classic issues of errors in the observations and non-commensurability of the processes. So how does the presented approach differ from standard practice? One potential option would be to use a data assimilation approach (as is suggested but not elaborated upon) and check the contribution of data sources to model performance, the robustness of the model etc.

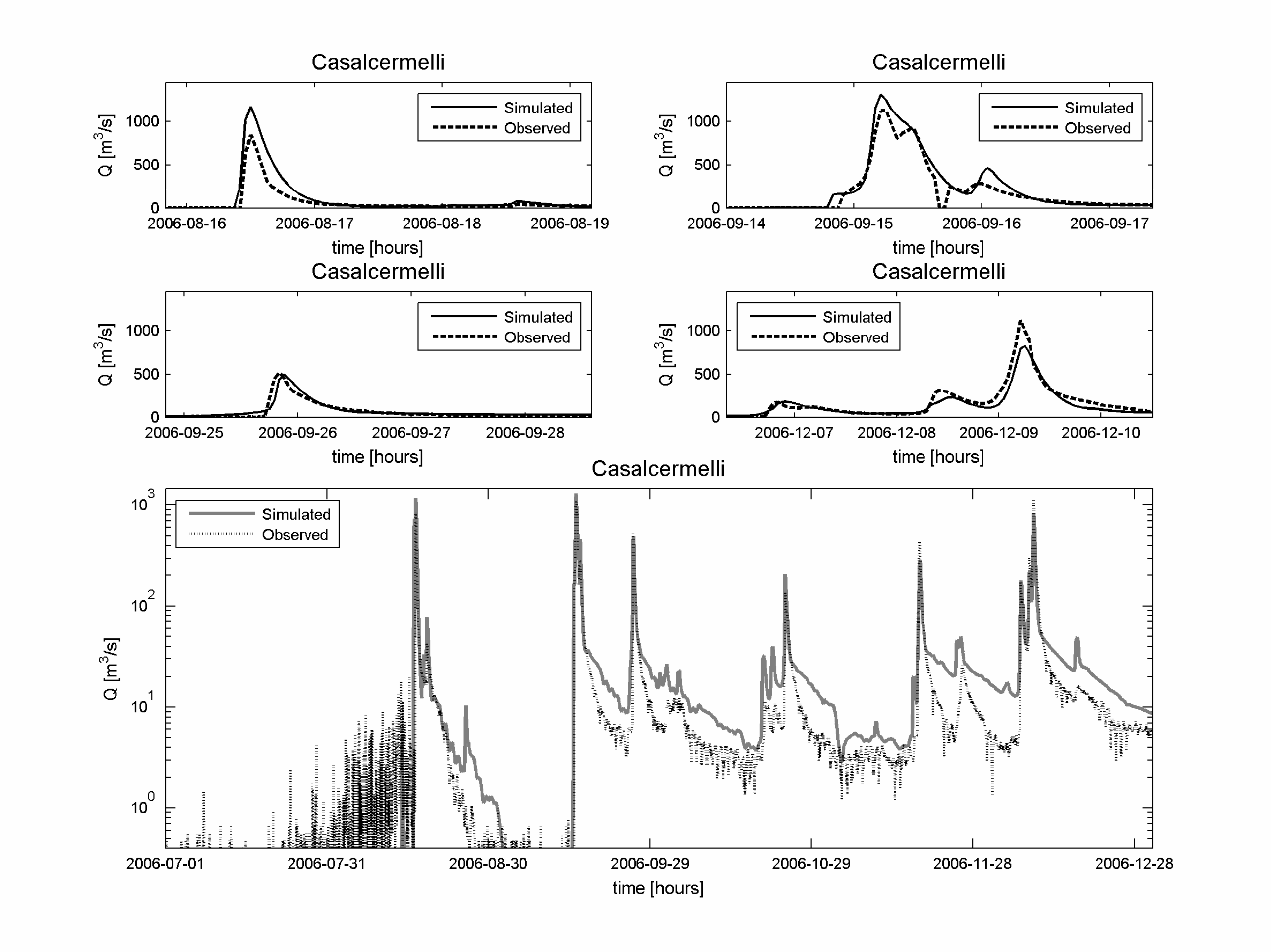
The reviewer correctly points out the philosophy behind this model that is somewhat different from, lumped or "leaf-approach" hydrologic models on one side and from fully distributed models on the other. The nature of the model is in fact fully distributed and the parameters distribution kept in space building on the spatial pattern of some basic soil characteristic (one value per cell different in every cell is then preserved) such pattern is then rescaled by means of physical considerations and the scaling factor remains as a single catchment scale parameter to be calibrated. This approach reduces dramatically the calibration effort and allows the implementer to perform a manual calibration in case this is his/her preferred option. As an example, the soil calibration parameters are constant at basin scale, but they are combined with a distributed information (in this case derived by the CN maps) as can be seen

in par. 2.3. The parameter c_t is constant at basin scale, but the water content that can be held by capillarity against the force of gravity is distributed
$V_{fc}(i,j)=c_t*V_{max}(i,j)$, so that it assumes different values for each cell of discretization.
This allows representing spatial patterns with a simple but efficient (the calibration parameters are reduced) scheme.

In other words, physical equations retain a similar structure in every model, what is often more specific to a model is the way parameters are treated and distributed in time and space. From this point of view Continuum proposes an approach that is in line with model parsimony. This characteristic, as again noted by the reviewer, allows the calibration in scarce data environments and would be a critical asset in case the model would be used in a DA framework.

Identification of model errors
It is hard to interpret the model performance. Although the performance measures (tables 3 and 5) look quite OK, tables 4 & 6 and the hydrographs in Figs 6 and 7 show very big discrepancies between simulated and observed peak flows (up to 70% ?!).
This indicates severe issues with the catchment water balance, which are likely to be caused by the input data (precipitation). Also, the apparently very flashy regime of the catchment makes it impossible to evaluate the model performance in base flow conditions. Is there any base flow at all in the catchment, or is it zero most of the time?
If there is base flow, plotting the hydrographs on a logarithmic y axis may facilitate the visualisation.
The catchment chosen has a flashy behaviour in terms of its significant events (the coefficient of variation of the streamflow time series is fairly high, as in most Apennine catchments) however it maintains a measurable base-flow condition that allows an evaluation of model performance in this flow regime. Such thing was hardly visible from the graphs as noted by the reviewer, now it is better visualized following the suggestion of the reviewer putting in logarithmic scale the observed and simulated hydrographs. In doing that we should keep in mind the nature of the analyzed catchment. The base flow is often very low and its measure is often uncertain. In these kind of basins when the low flow regime is present the stage-discharge transformation could be really uncertain due to the modifications in the river bed: e.g. the presence of small meandering that can change between events on one side and on the other measurements errors due to the gauges (e.g. the influence of Temperature in ultra-sound gauges).
Keeping this in mind periods with very low flow should be considered as indicators of the flow regime and of the soil moisture condition of the catchment that should be followed by model simulations.

The catchment regime is surely a challenge and in some cases, as the one pinpointed by the reviewer the precipitation distribution could seriously hamper the performance of the model in reproducing that specific part of the hydrograph: this problem is common to all models (see e.g. applications in Liu and Todini, 2002, Rigon et al., 2006). We will discuss this explicitly in the new version of the paper. However, despite one specific period in the calibration phase, we can however conclude that model performances are satisfying for many hydrological applications and comparable to international standards



…For instance, one question that arose while reading the paper is the type of time stepping that is used. The emphasis on being a continuous model would suggest that an implicit time stepping scheme is used (which would cater for some of the issues highlighted by Clark et al. (2010)) but no information at all is given about this.

In the new version of the Manuscript we will add information about the numerical integration schemes used by the model. We did not think about inserting it in a first version of the paper, but this is a very important point as much attention has been devoted in selecting the best numerical scheme configuration so that the model perform efficient simulations while closing all balance equations with negligible errors and without presenting numerical stability problems. The different timescales of processes in the model ask for a differentiation of the numerical integration scheme used. Therefore, as final result the model uses the Huen integration method (Clark and Kavetski, 2010) to solve the mass balance equation, the method is a semi-implicit method (predictor-corrector scheme) and is second order accurate. It is a modification of the implicit trapezoidal method. The final result is a predictor-corrector method with forward Euler's method as predictor and trapezoidal method as corrector. Such choice allows the best balance between accuracy of the solution and performance when an appropriate time-step is used (this would change in applications where the integration step is large e.g. daily). A different scheme is used to solve the Force Restore Equation where more accuracy is needed and a Runge Kutta 4th order method is used: this method proved in Meteorological models and SWAT-like models to be quite efficient.
As said, the use of appropriate time steps is crucial. In Continuum model the timestep is also differentiated as a function of the process analyzed to speed up integration. As an example, 1 hour for deep flow and 30 sec for surface flow (this latter can be variable as a function of the discharge value as a consequence of varied flux velocity, this configuration has not been used in the paper application because in testing phase). In essence, the time step of the model is comparable or less with the time scale of the variability of the physical processes. Considering as an example the Modified Horton infiltration equations (equations 11-14), if they are

integrated with a small time step (e.g. 5-60 min) the dV/dt(t=n) is a good approximation of dV/dt(t=n+1). The errors are generally smaller then those caused by parameterization uncertainty,.. Similar considerations about using the adequate time steps are reported also in Clark and Kavetski (2010).

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References:

Clark, M. P., and D. Kavetski (2010), The ancient numerical demons of conceptual hydrological modeling: 1. Fidelity and efficiency of time stepping schemes, Water Resour. Res., 46, W10511, doi:10.1029/ 2009WR008894

Dingman, 2002, Physical Hydrology 2nd edition, Prentice Hall, NJ ISBN 0-1-09695-5

Rigon, R., Bertoldi, G., Over T., M., (2006), Geotop: A Distributed Hydrological Model with Coupled Water and Energy Budgets, Journal of Hydro-Meteorology, 7, 371 – 388.

Liu & Todini (2002), Towards a comprehensive physically-based rainfall-runoff model. Hydrology and Earth System Sciences, 6(5), 859–881" for a continuous hydrological model calibration and validation.