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Hydrology and Earth System Sciences Discussions

Interactive comment on "Parameter extrapolation to ungauged basins with a hydrological distributed model in a regional framework" *by* J. J. Vélez et al.

J. J. Vélez et al.

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The paper is a good paper which is worthwhile publishing.
Thank you very much. We always try to do the best and we hope the results reflect this effort.

Nonetheless there are two points that have to be further clarified and discussed.
In our opinion, if it is possible, the comments and clarifications must be introduced in the final version of the paper.

The first point (a minor one) relates to Section 4.3 and Figure 7. It is well known that precipitation trends are not linear. They may appear linear up to a certain elevation, but there is an elevation, which varies with the local conditions, at which this growth disappears. The authors should discuss this aspect since they might generate higher *rainfall (or snow) rates.*

The objective of the use of a rainfall interpolation factor β is explained on Section 4.3 first paragraph. In other words, this β has been introduced to compensate the usual underestimation of the basin precipitation when using point rainfall gauge stations if there is an increment of storm precipitation with altitude. And this is due to the fact their locations are in most cases biased to the lower parts of the valleys within the basin.

Of course, when there is precipitation increment with altitude, the relationship is not perfectly linear mainly because the relation is not exactly the same in all valleys within the basin and (as pointed out by the referee) in most cases there are a precipitation decrease close to the top of the mountains due to edge effects. We did not introduce a non-linear spatial variable relationship in order to be parsimonious in the number of parameters, which is a general philosophy in TETIS model and:

1. In basins where we detected from the precipitation data the possibility of this situation (as it is shown in paper's figure 7), the increase of efficiency was always significant with the linear relationship compared with assuming no precipitation increment with altitude.

2. We did not found any clear reduction (as it is shown in the example of figure 7) in any case, which means only that the edge effect is concentrated in the very upper part of the mountains where we have few gauge stations.

3. If the actual profile would be not linear and we use a linear relationship, the calibrated value of β will try to compensate this error, but, what is the most important thing, correcting the basin precipitation underestimation increasing the precipitation with altitude but maintaining the general spatial precipitation pattern forced by the point gauge stations used in the interpolation.

4. When the density of rainfall gauge station increase, it is expected the error in the precipitation profile will be reduced.

5. Lastly and in this case less important, the snow component of the model was not used in the case study because snow was not a significant source of water resources in it. Actually, the regional government hydrologists forced us to don't use it from the very beginning of the project and, in fact, it was not necessary.

The second point is more important. It relates to the statement at the end of section 4.4: "This calibration results can be considered as excellent" The problem here relates to the fact that the authors are aware of the uncertainty introduced by the parameter estimation:

"The challenge is to estimate the best parameters set of distributed conceptual models. Due to the inability to accurately measure distributed physical properties of environmental systems, calibration against observed data is typically performed, which is most often achieved with limited rainfall-runoff data. The equifinality noted by Beven (1989) indicates that given the complexity of such models, many different combinations of parameter values may simulate the discharge equally well. These parameter sets may be located throughout many areas of the parameter space (Duan et al., 1992; Beven, 1989). This uncertainty of the appropriate parameter values yields predictive uncertainty as has been demonstrated through applications of the Generalized Likelihood Uncertainty Estimation methodology (Beven and Binley, 1992; Freer et al., 1996; Beven, 2000)". But they do not discuss this problem in their paper in relation to their results.

The paragraph in quotes is in our Introduction. In order to solve this point we have applied the calibration methodology can be found in a recent published paper (Francés et al, 2007) oriented to the automatic calibration problem. Probably for this reason we

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made the mistake don't explain too much about equifinality problem in case study of this paper (only one paragraph in section 4.4 page 928 lines 17-25). What we can add and/or clarify concerning equifinality problem is that it was solved using "hydrological sense" in:

- the search range of each calibration variable (correction factors and β in the automatic calibration process (see Francés et al., 2007)
- a final manual correction of these variables, in most cases with the cost of reducing the calibration efficiency

And we mean by "hydrological sense" the participation in the calibration process of local experts, the increase of our basins knowledge by several field trips and analyzing carefully the neighbor basins behavior, the basin water balance and the internal flow distribution into the different flow components. We made a huge effort in this stage of the project.

In particular, more than the uncertainty induced in the sections used for calibration they should investigate and discuss the uncertainty induced in the other sections (they say that they must provide information in 567 sections most of which ungauged, which is the reason for using a distributed modelling approach) by the model parameters and their uncertainty. Failing to do this, the paper would appear to be a good complex hydrological application to a catchment, but still an application that could be performed by many of hydrological consulting companies instead of a qualified research institute. In other words they should provide an assessment of the level of reliability of their work, which cannot be limited to the assessment of the calibration qualities at the gauged sites using the classical indicators, but should encapsulate a measure of uncertainty to be used by the end users, who, according to the authors will use the system for water resources planning and management.

We would not say "many": nowadays distributed modelling is mostly in the research field and it is not common in engineering practice to use grid based distributed modelling at regional scale with daily time step for water resources analysis. But we are sure it will be common in the future!

The referee is right when pointing out that it is more important for research to known the uncertainty of the results at ungauged points than to have their simulations. And for the end users must be equal important. But actually this is the objective of the spatial (different station than the calibration one but with the same period than used in calibration) and time-spatial validation (different station and period than those used for calibration), presented in Section 4.5 and table 4 and which is condensed in Figure 10b. Clearly we failed stressing the significance of the uncertainty issue in Section 4.5 and Conclusions.

What we can add is:

1. Table 4 and figure 10b represent the basin outlet discharge model efficiency (the inverse of uncertainty) at gauge stations not used for calibration (same and different periods) and it is representative what will happen simulating the discharges at truly ungauged points. The efficiency is including the uncertainty and/or errors in the inputs (precipitation at gauge stations and PET), initial parameters estimation, model conceptualization and parameters calibration. I.e., the objective is to asses the uncertainty of the state variable of interest (which is the interest of the end user), not only to estimate the parameter uncertainty.

2. The worst efficiencies in these not calibrated points are related in all cases with karstic springs close to the flow gauge stations and/or very small basins with few cells. The first one is clearly a model conceptualization error (TETIS is not intended to simulate or predict concentrated base flows). The second case is clearly the initial parameter estimation uncertainty: when the basin size is smaller than the scale of the spatial information used for this estimation, it is a "lottery" (the cell parameter estimation uncertainty) to "hit" the actual value (always un-

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known).

3. In general, the quality of the southern basins information is poorer than for the northern ones (lower spatial resolution, lower density of input gauges, lower quality of flow gauges). But this is not reflected in the efficiency results, mainly because the other sources of uncertainty have bigger effect on the discharge simulation efficiency.

4. Figure 10b (and also the figure 8b for the calibration) shows a dependence between the basin area and the model efficiency. We will introduce a regression function to the new Figure 10b.

5. Figure 10b (and also the figure 8b for the calibration) shows also a dependence between the variability of the model efficiency and the basin area. It can be shown clearer if we compute the coefficient of variation of monthly E for four basin area ranges and plot them versus the intermediate basin area value.

6. These two dependences can be explained mainly by the initial spatial parameter estimation uncertainty at cell scale. The effect of this uncertainty is maximum for basin area equal to one cell ($0.025km^2$) and its effect reduces with basin area because the discharge at the outlet is the sum of all cell runoffs within its basin. And by the Central Limit theorem, sums in statistics always reduce the new variability.

7. If the uncertainty of the simulated discharges at ungauged points (or sections) is measured by the monthly value of the Nash-Sutcliffe efficiency index (monthly E) then, the regression equations for the mean monthly E and its coefficient of variation are a very accurate indicators of this model uncertainty for our case study. It must be taken into account that it includes all sources of uncertainty and errors (as explained previously) and can be used as an example for other researches and engineering works.

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