

## ***Interactive comment on “Hydrological model performance and parameter estimation in the wavelet-domain” by B. Schaefli and E. Zehe***

**Anonymous Referee #2**

Received and published: 21 July 2009

The manuscript presents an innovative method for model calibration, based on the match of the frequency contents of the observed and simulated output time series (in the present case, runoff data). The approach is extremely interesting, due to its ability to analyse the periodical features of the simulated variable, for a better representation of the different hydrological processes taking place in the basin. Particularly interesting is the possibility to use the results also for model diagnostic, that is for identifying the possible drawbacks in the modelling choices.

The paper is nonetheless, a bit too long and the reader gets lost in the excessive high number of case studies and relative discussions, particularly since many analyses performed by the authors for highlighting the behaviours of the different calibration

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procedures over the different case studies (even if demonstrating a deep knowledge of the proposed tools and a wide experience on the watershed behaviour) are not always straightforward and easy to follow. The first part that may be removed is that on the visual inspection (section 3.1.1) of the catchment that will not be modelled in the following (the Massa River) and Figure 1 may be simplified accordingly and deleting also panel c (which is not understandable at this point of the reading and which, by the way, refers to one of the case studies that I suggest to remove). Secondly, I agree with Referee #1 that the toy examples may be skipped and I would go further, removing also the ARMAX and HYMOD modelling examples, distracting the reader (without adding indispensable insights) from the main application on the real-world case study (and the relative synthetic applications), which is the most important application and which is perfectly able to explain, alone, the main characteristics of the proposed approach. In fact, both Model diagnostics (section 5.3.2) and Conclusions and outlook (section 6), describing the key results of the work, are entirely based on the experiments with the GSM-SOCONT model alone.

### **SPECIFIC COMMENTS**

Section 4.1.2: Specify, when introducing for the first time experiments 3 and 4, which is their difference (different model structure).

Section 5.2.3: The definition of the parameters of the GSM-SOCONT is not clear in the different applications: in table 3 not all the parameters of table 2 are listed. In addition it is not clear to me how it is assessed which ones are the most (and least) sensitive parameters.

Section 5.3.1: I would suggest removing the first paragraph (lines from 3 to 14) and also Figure 4, since they are not significant enough to be necessary in such a complex presentation. Figures 7 and 8 have too many panels and such panels are not described in the correct order in the text. The final part of the section (from line 11 page 2476 to line 6 page 2477) is not clear to me (and the same holds for the meaning of Figure 7b)

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(it may perhaps be removed?)

Figure 5: why do the upper panels (synthetic case) show parameters that are different from those shown for the real-world case?

Figure 6: please specify to which case study it refers.

Figure 8: please specify if the panels refer to calibration or validation data.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 2451, 2009.