Interactive comment on "Combining satellite data and appropriate objective functions for improved spatial pattern performance of a distributed hydrologic model" *by* Mehmet C. Demirel et al.

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The authors present a methodology aimed to improve predictions of a distributed hydrological model both in time and space. In order to identify model parameters responsible for the spatial predictions, an additional complex objective function is introduced taking into account the match between observed (satellite based) spatial evapotranspiration patterns and those predicted by the model.

The paper is interesting and the approach taken opens a new direction of research towards the use of spatial remote sensed observations to improve predictions of a distributed hydrological model. In particular, the authors applied site-specific param-

eterizations to increase the flexibility of the description of actual evapotranspiration characteristics (root zone and potential evapotranspiration corrections).

The authors formulated the problem in a deterministic framework, which might be a good introduction to a new approach, but the discussion on uncertainty is missing.

The paper is well written and requires only some clarification of the presented material and a wider discussion of the assumptions taken. I recommend to publish the paper after minor corrections.

Specific comments:

Page 1, line 18: . . . In addition two new site-specific spatial parameter distribution options have been introduced . . .

Page 2, line 19: This is from the fact . . . grammar should be corrected e.g. 'this is because.'

Page 3, Line 17: it is not clear what the authors mean by 'domain'

Page 3, line 22: is it: . . . for comparing spatial patterns of two continuous variables. . .?

Page 5, lines 8-11: what is the uncertainty of AET estimates?

Page 5, line 30: Could you please give more detail about the way monthly AET maps are applied in the model and the disaggregation method used?

Page 6, line 9: The parameterisation introduced is a very interesting way forward and requires a separate paper backed up with field experiments. Could you please give the possible disadvantages of the parameterisation? Even though the parameterisation decreases the number of parameters of a distributed hydrological model, the parameters require a sufficient amount of observations to be properly identified. The question is how to test the parameterisation using very limited and uncertain information obtained from the indirect and fragmented satellite observations. The other question is, how to estimate the uncertainty related to that parameterisation. Some comments would be

welcome.

Page 6, lines 10-24: How sensitive is water storage variability to this parameterisation?

Page 7, lines 2-10: was the model tested on observations and what assumptions must be fulfilled?

Page 7, line 24: was this parameterisation tested on observations? What assumptions are imposed?

Page 8, line 7: Since comparison . . . should start from a new line and a new objective function responsible for reproducing spatial patterns should be introduced.

Page 8, lines 10-11: histograms of what?

Page 8, line 24: The AET from TSEB have been treated as error free data – a comment is needed on the possible errors involved.

Page 9, line 31: That criterion might be very misleading when the response surface is flat. The optimisation algorithm might stop in any part of the optimisation range or, most likely at the edge of the parameter range. The authors are asked for a comment.

Page 10, line 8: . . .A and B and . . .. (- B is confusing)

Page 13, line 12: The existence of local minima depends on the form of the objective function which defines the parameter response surface. In the case of a model with 26 parameters the objective function will show local minima. Following the equifinality hypothesis, there are many parameter sets which give the same value of the objective function and therefore it is not surprising that many different optimum solutions can be found.

Page 14, line 8-9: Spatial calibration constraints the solution rather than reduces its uncertainty – the uncertainty was not evaluated.

Page 15, lines 23-24: Could it be explained why the improvement occurs?

Page 16, Discussion: As I understood, the authors did calibration using different sequences of cases. Might it be advantageous if the optimisations Q-only and Spatial-only were applied iteratively?

Page 17, line 13: . . . associated with . . ..

Page 17, line 32: . . . site-specific due to . . ..

Page 17, line 34: . . .different countries . . ..

Page 18, line 28: The conclusion on achieving a more robust parameter set because the trade-off disappears is not well founded and too general after only one validation exercise.