Reply to Reviewer Comments

Reviewer #2

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 parameterizations 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.

Reply from authors: The authors thank the Dr Renata Romanowicz (reviewer #2) for her positive and constructive comments on the manuscript.

Specific comments

Comment 43) Page 1, line 18 In addition two new site-specific spatial parameter distribution options have been introduced

Reply from authors: Corrected.

Comment 44) Page 2, line 19 This is from the fact grammar should be corrected e.g. ‘this is because..’

Reply from authors: Corrected.

Comment 45) Page 3, Line 17 it is not clear what the authors mean by ‘domain’

Reply from authors: We meant “catchment” with domain. This is corrected in the revised version of the manuscript.

Comment 46) Page 3, line 22 is it for comparing spatial patterns of two continuous variables?

Reply from authors: Corrected.

Comment 47) Page 5, lines 8-11 what is the uncertainty of AET estimates?

Reply from authors: Please see reply to comment 5) by the reviewer #1

Comment 48) 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?

Reply from authors: We will elaborate on this in the revised manuscript.

Comment 49) 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.

Reply from authors: Our study builds on previous studies on the relations between surface properties of soil and vegetation and model parameters. We are not deriving these relationships ourselves from new field data. We don't see the spatial parametrization as a novel methodology, but more as a flexible spatial parametrization scheme, that allows us to explore the main topic of the paper, which is the use of a new spatial pattern metric and complementary observations of RS AET patterns and stream discharge for model calibration. The idea behind the parametrization is that we use data that is well described spatially, namely soil texture maps (based on a great number of measurements) and vegetation maps (based on satellite remote sensing) to distribute related model parameters spatially while still allowing for some calibration. The disadvantage would be the validity of the proposed relations between soil and vegetation properties and model parameters. But we regard it as a robust approach that avoids over simplification in spatially uniform parametrization and over-parametrization in grid-by-grid calibration.

Comment 50) Page 6, lines 10-24 How sensitive is water storage variability to this parameterisation?

Reply from authors: We don't quite understand the question, perhaps the reviewer could elaborate.

Comment 51) Page 7, lines 2-10 was the model tested on observations and what assumptions must be fulfilled?

Reply from authors: This concept is taken from Danish soil type and root depth studies (see references in manuscript) and is believed to be representative for the study area, where cereal crops are grown on soils ranging from very sandy to loamy. See also reply to reviewer 1.

Comment 52) Page 7, line 24 was this parameterisation tested on observations? What assumptions are imposed?

Reply from authors: Please see reply to comment 20) by reviewer #1.

Comment 53) 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.

Reply from authors: We agree, this will be corrected.

Comment 54) Page 8, lines 10-11 histograms of what?

Reply from authors: Corrected as below. Also we will elaborate on the histogram matching as requested by reviewer #3.

"In this context, we adopted the structure of the Kling–Gupta efficiency while substituting the standard deviation term by a term based on the coefficient of variation $\left({}^{\sigma_O}/_{\sigma_S}\right)$ *and replacing the bias term with a histogram comparison index to compare the intersection-percentage of two histograms of observed and simulated spatial maps."*

Comment 55) Page 8, line 24 The AET from TSEB have been treated as error free data – a comment is needed on the possible errors involved.

Reply from authors: This is a very good point, and we have given it a lot of thought how to quantify the uncertainty. However, given the way we utilize the AET maps (bias-insensitive pattern performance) the quantitative uncertainty related to comparison with point measurements would not be very useful. What is needed is a quantification of the uncertainty of the RS AET pattern. This is far from trivial, because even if the uncertainty of some of the input to the TSEB model (mainly LST) can be approximated, it is the impact this uncertainty has on the AET pattern that is important. The crude assumption is that errors in the TSEB input are largely uniform for this relatively small catchment (e.g. if the LST input on a given day has an estimated error of +1 Kelvin, this is assumed to apply for the entire area and therefore have a limited effect on the estimated AET pattern. The whole framework is deterministic, and this

is clearly a limitation, however, the study proposes several new ideas, and to put this into a thorough uncertainty framework is beyond the scope, but will be an interesting further development.

Comment 56) 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.

Reply from authors: Each iteration is comprised of more than 100 runs with different parameter sets. This means that if the objective function doesn't improve after more than 500 runs then the calibration stops. This is assumed to be a reasonable number of testing in an optimization and generally we don't see parameters that stop at the edge of the parameter range. We will add more details about iterations in the revised version of the manuscript.

Comment 57) Page 10, line 8 A and B and . (- B is confusing)

Reply from authors: Corrected.

Comment 58) 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.

Reply from authors: We agree, we will revise this sentence, it actually does not raise questions about the SCE, but is a result of the multi-objective 26 parameter problem being solved.

Comment 59) Page 14, line 8-9 Spatial calibration constraints the solution rather than reduces its uncertainty – the uncertainty was not evaluated.

Reply from authors: Correct, we will rephrase this from reduces uncertainty to better constrained.

Comment 60) Page 15, lines 23-24 Could it be explained why the improvement occurs?

Reply from authors: We describe a drop in performance, so I guess the question is why the performance drops? And yes we have a good idea why; there was a significant drop in the number of rain gauges in Denmark in 2007-onwards, this has had a direct impact quality of the precipitation input and on the streamflow performance across the country, but we fell it is beyond the scope of this paper to go into that discussion. It is the topic of other ongoing studies.

Comment 61) 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?

Reply from authors: Yes, we thought about that and given the limited trade-off and that some model parameters are mainly/only effecting either discharge or SPAEF, we would probably have reached similar results as the combined calibration.

Comment 62) Page 17, line 13 associated with .

Reply from authors: Corrected.

Comment 63) Page 17, line 32 site-specific due to .

Reply from authors: Corrected.

Comment 64) Page 17, line 34 different countries.

Reply from authors: Corrected.

Comment 65) 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.

Reply from authors: We will rephrase this, not claiming that the parameter set is more robust.