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Interactive comment on "Spatial and temporal patterns of land surface fluxes from remotely sensed surface temperatures within an uncertainty modelling framework" *by* M. F. McCabe et al.

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Spatial and temporal patterns of land surface fluxes from remotely sensed surface temperatures within an uncertainty modeling framework

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Summary The two referee reports (Schulz and anon.) and single interactive comment (Montanari) provided some useful and constructive criticisms of the manuscript. After careful consideration of the requested minor revisions, the authors believe that all appropriate adjustments as suggested by the reviewers have been successfully incorporated. As a result we believe the manuscript represents an improved and more robust reporting of the results achieved during this study and we look forward to seeing its publication in HESS. Following is an outline of the specific comments and suggestions from each of the reviewers and the action taken to affect their comments in the manuscript. Best regards, Matthew McCabe

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Referee: K. Schulz

Introduction. 570. l23: add references The sentence has been adjusted to highlight the original intent which describes the widespread production of regional scale evaporation and hydrological maps by water and meteorological agencies, which while used widely in practice are not referenced in an informative way (e.g. Climate Atlas of Australia: Maps of Evapotranspiration, Bureau of Meteorology, Melbourne, VIC Australia).

Methodology. 574, l23. cited reference of Franks and Beven (1997) and overview of TOPUP model The cited reference has been changed. Additionally, a more detailed description of the TOPUP model (1-page) and a figure describing the processes occurring within, have been added to the paper.

576. l20. Reference to the Koeppen (Köppen) classification system The following reference has been included in the list: Koeppen, W. (1931). Klimakarte der Erde, Grundriss der Klimakunde. Second edition, Berlin and Leipzig.

578.l26. Please explain the quality control. Two additional references have been added to describe the quality control procedures that were employed here for the surface temperature records, and the sentence has been rephrased to make this more general.

579. l23. It is difficult to understand - regionalization strategy. There may have been some misunderstanding due to poor structure sentence so this has been rephrased to remove the idea suggesting a regionalization strategy has been developed. The sentence in question has been replaced with "In this way, numerous temporal records of observations (i.e. surface temperatures) are used to extract from the patch based land surface model a spatially distributed hydrological response (i.e. evapotranspira-

tion)" - highlighting that the approach allows the expansion of a patch based model into a spatially distributed model using satellite observations of the surface temperature difference.

579. l27. What are the meteorological forcings - influence of spatially averaging these. A list of the required forcing data has been included in the model description section. The confusion of averaging has been removed by re-emphasising that only 1 meteorological tower was available over the region. Problems associated with this such as issues in heterogeneous environments are highlighted in the discussion (e.g. "Unfortunately, while assumptions of constant forcing data may increase uncertainty in predictions, there is generally little information available to spatially interpolate these variables from regionally sparse meteorological networks.") While the single meteorological station is not ideal, the areal extent of the catchment is such that the effects should not be dramatic, particularly in relation to radiative forcing and air temperature.

Results. 581. 8ff: Fig 2. shows pattern of temperature differencesĚ This has been rectified by referring only to temperature differences.

582.l2-5. Averaging data. This has been responded to.

582.l7. I would suggest to begin withĚ This has been rephrased to make it more clearly understood.

582.l3. Is EF measured or calculated. It is restated in the text that EF is calculated from measurements of the latent heat flux and available energy.

587. What is the importance of including analysis of surface resistance? Some additional information on this has been included in the introduction "It is expected that the temporal and spatial patterns of surface resistance should corroborate those results obtained for the latent heat flux, given the close relationship between the surface resistance and evapotranspiration, while also offering some insight into the dynamic nature of this variable during periods of varied hydrometeorology."

The dynamic nature of the surface resistance is not well understood, and the approach presented here explicitly illustrates how this varies in time with the hydrological characteristics evident in the catchment. This is informative not just from an ecological and evaporative aspect, but also since it allows a examination of links between the ET performance and changing surface resistance during the study period.

Additionally, it is informative to assess the response of a model variable that is not directly calibrated to ensure that correct (or at least expected) responses are maintained. This idea has been highlighted in previous work, where the authors have examined multi-objective calibration techniques.

It is hoped that this has been reemphasised throughout the text as the reviewer suggested.

588.ff. Figure 8. Not included in the PDF. This is unusual since the copy that I have downloaded from the web has this figure. I believe this to be a publishing issue which I am sure will be resolved by HESS.

Discussion. 590. l25. Claim that temperature differences constrain predictions. The focus of this paper was on developing the spatial distribution of evapotranspiration from a patch based model using observed remotely sensed surface temperatures. It is a natural extension of previous work (McCabe et al. (2005)), which did show the constraining influence on evapotranspiration of calibrating to a surface temperature difference record. Additional mention has been made of these findings in the revised manuscript so as not to repeat already published findings.

McCabe, M. F., Franks, S. W. and Kalma, J. D. (2005). "Calibration of a land surface model using multiple data sets." J. Hydrol. 302(1-4): 209-222.

Referee. Anonymous.

Paragraph 2.5: As far as I understood itĚ.by using vegetation classes.

Only 15 parameter values are used in the model. Model output (temperature) was calibrated with observations. No detailed examination of the parameters was undertaken, rather allowing them to be sampled from broad parameter ranges. As a result, 20,000 parameter sets are produced, from which 200 are considered after calibration of the temperature records for each of the pixels (300) within the catchment. The procedures as employed herein have been made more explicit with reference to preceding publications reporting the GLUE approach. No attempt was made to decrease the degrees of freedom, since there is no a priori knowledge of parameter behaviour to do this.

Paragraph 3. The results exhibit some spatial correlation, does this hold for the parameter sets as well. How could this be used to constrain the model.

The reviewer poses an interesting question which is unfortunately beyond the scope of the current paper. Spatial correlation in parameter values would be an interesting exercise to explore, although there is no explicit accounting or specification of such in the current approach. It is worth noting that the approach employed here produces completely distributed parameterisations - allowing the data and the model to speak for itself. This is counter to most models of the land surface which specify broad parameterisations for large land units.

Paragraph. 2.3. Please give exact locations of the study area. Define the acronym EF. Exact locations of the study area are now given in Fig. 2. EF is defined.

Comments on Figures. Additional details in the figure captions have been included such as specifying the units, scale and location of colour bars. Also, the confusion with Fig. 5 has been made more explicit, noting that values of the water level are measured in AHD (Australian Height Datum) which is height above mean sea level (0.0 AHD).

Referee. Montanari

1) Details on the TOPUP model

In line with reviewer Schulz, a new section on the TOPUP model has been included in the appropriate section. Additionally, four equations have been added which describe the surface flux processes occurring in the model.

2) Application of GLUE

Additional details on GLUE are provided in the text. However, GLUE has been in the academic literature for nearly 15 years. Numerous articles have been written on this, with specific reference to its implementation as reported here, in McCabe et al. (2005). However, the authors have made efforts to ensure that each of the reviewers queries have been explicitly addressed in the text, with additional references included where required (e.g. Kuczera, 1983). In particular, in response to:

a) the likelihood measure: although already stated in the text that a least squares likelihood measure was employed, further details have been included such as "The choice of the least squares objective function was based on a number of studies that have employed it under the assumption of an error model based on zero bias with normally distributed errors (e.g. Kuczera, 1983). However, using the least squares approach does not eliminate the risk of introducing bias in model identification." Gupta et al. (1998) comment that in over two decades of investigation, there has been no clear demonstration that a particular objective function is better suited for calibration of a model than some other. b) The criteria to reject non-behavioural model runs: no such criteria were used in this model approach and as such this has not been discussed. It has now been made explicit that this is indeed the case "The specification of a priori likelihood weights can also be incorporated for particular parameterisations or model predictions, allowing the rejection of a parameter set if it falls below a certain likelihood threshold, or if it is classed as non-behavioural. No such implementation of this has been employed here, with all model simulations and parameterisations given equal weighting." c) Whether some TOPUP parameters were kept fixed and why: as stated in the text, all parameters were randomly sampled. No parameters were kept fixed. "From within these ranges, multiple parameter sets can be constructed using Monte-

Carlo sampling to randomly extract parameter sets from the pre-defined ranges to run the model."

Confidence bands in Fig. 6. We have not stated that we make a quantitative estimate of the error in model predictions, since to do so would be extremely difficult, highly subjective and most likely misleading. What we have indicated clearly throughout the text is that most model approaches do not attempt any incorporation of uncertainty in either parameterisations or calibration data. While attempts to understand model uncertainty (as opposed to accuracy) are subjective, since they are based here on the objective function used and the number of chosen parameter sets, it provides a more 'realistic' approach to land surface modelling than is evident in most of the current literature.

The authors make no claims to provide an assessment of the total uncertainty. Rather we proved an appreciation of the inherent uncertainty in model predictions and show this can be improved using novel data to calibrate the model space.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 569, 2005.