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Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Temporal interpolation of land surface fluxes derived from remote sensing – results with an Unmanned Aerial System" *by* Sheng Wang et al.

Anonymous Referee #2

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This manuscript introduces a simple but effective coupled surface exchange model, with the goal to use it for gap filling of surface states and fluxes between measurements by remote sensing. The model requires higher resolution meteorological data as input for the forward simulation that serves as the gap filling procedure. The calibration is based on a very small number of snap shots of surface temperature and Normalized Difference Vegetation Index. As a proof of concept the method is applied using data obtained during seven flights of a drone, and continuous data from an eddy tower. The performance of the model es evaluated by comparing with independent eddy tower data of fluxes and states.

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Discussion paper

The manuscript presents an intriguing approach tested in a well designed study. The results are impressive, especially given the deliberate simplicity of the applied exchange model. The manuscript is well written manuscript. While I have some comments on the manuscript, I also recommend its publication in HESS and expect that it will find strong interest in the readership.

Major comments

(1) I found if very difficult to disentangle the different data sources used for the different application steps, which are: parameter estimation from literature and nearby observations calibration (UAS derived data, surface temperature and soil moisture) input for forward modeling (meteorological data from the eddy tower) validation of model output (independent eddy tower data) To make this more accessible I am missing an overview table systematically showing which data source was used for what purpose (as above). This would really help navigation,

(2) I would have liked to see some more discussion on the next challenges for the more widespread application of the proposed method with less ideal input data for the forward model. What are the expected limitatations of the approach? Currently the discussion regarding this point is very short. For example, the discussion mentions that the method could be extended to larger scales by using online weather data. However, those have also higher uncertainty compared to the data from the tower. Also, the JPL-Priestley-Taylor-ET estimate is less reliable in more arid climates which probably requires additional adjustments in those conditions, etc. I recommend enhancing the discussion regarding this.

(3) I am confused about what is the underlying hypothesis motivating the comparison of the residuals across different stages of diffuse light conditions? The analysis is motivated by stating that remote sensing is typically biased towards collection in direct sunlight conditions. But this was probably not the case in your exercise, since you were collecting data from a drone. Therefore the calibration data set should not be affected

by this bias? Why are you expecting the bias in the residuals?

(4) I find the equations of the manuscript difficult to read because the abbreviations of the variables are of several letters. I understand that in some instances this is done to adhere by the nomenclature in the discipline, e.g converting LAI to a one letter variable would probably cause confusion. But in most cases this is not an issue. For example, radiation can be abbreviated with R and the components by indices, fluxes with Q or J with indices. Also canopy storage, soil water storage etc. This would also increase consistency. I strongly recommend incorporating the one letter abbreviation paradigm as much as possible. See also HESS author guidelines (Mathematical requirements) https://www.hydrology-and-earth-system-sciences.net/for_authors/manuscript_preparation.html

Detailed comments

Abstract, Page 1

Line 18: "SVEN interpolated the snapshot Ts, Rn, SM, ET and GPP to continuous records“ This phrase is confusing, as it sounds like measurements of each of those variables were used, when according to the methods section only Ts and NDVI were used for calibration.

Line 21-22 I would not mind, if the errors were not stated quantitatively here, but if this is desired: An indication of the errors in percent would be more meaningful.

Introduction

Line 19/20: I think you mean "high persistence“

Methods

Page 9, Line 5 "low pass filter for T_s“: Can you be more specific about the cutoff frequency? Which interval does this roughly refer to?

Page 9 Line 24 Wind speed seems to be one of the variables that need to be available

continuously to apply the method. Is it reasonable to have such good knowledge of the wind speed? How sensitive is it?

Page 10, Line 15-20 The PF-JPL works much better in temperate then drier climate. Your appraisal does not mention this limitation, but I think it may be important for applying this method more generally. Could you add a note on this, either here or in the discussion?

Page 11, Line 27 should probably be "equation 29“ instead of "equation 28“

Page 12 Line 2 Soil water storage has different units here (m) and on page 9, Line 10 (mˆ3). I think it is fine to stick with m.

Page 12, Eq. 30-32, Page 13 Line 19-20 I am not sure how theta_r and theta_s are dealt with? They are not calibrated and not mentioned for the look-up table. Based on Table S5, where they are included, I am assuming they were looked up too. But please be more specific and include them in the list of parameters in Table 2.

Page 13 Table 2 It will help navigating the text, if in the table included a column indication of whether this parameter was looked up or calibrated in this study. I suggest adding this.

Page 13 Line 22 In my understanding calibrating SWS_max boils down to calibration the root water uptake depth?If yes, would be good to indicate this. While I have no objections against this procedure here, I conjecture that root water uptake depth may vary with time over the growing season. Thus, this may be a limitation of the model, which could be mentioned in the discussion.

Page 13, Line 7-9, Supplement Table S3 Please add the values for each of the initial conditions.

Results

Page 15, Section 4.1 Not sure whether I overlooked this, but can you please indicate

the values of the calibrated parameters? Also: I like Fig 4 showing the objective function. Near the pareto optimum plot a number of potentially very good model runs. Are they all roughly similar parameter values or do they differ substantially? This would give an indication of how well defined this model is in terms of the processes that are represented or/and the sensitivity of some of the parameters. Can you comment on this?

Page 15 Lines 19-20, Page 18, Lines 16-20. I feel the numbers are crowding the text, and are difficult to take in. It is enough to refer to Fig 5, Fig 7 or alternatively collect them in a Table.

Page 16, Line 5, Line 8 To me Ts does not appear to be underestimated only in high NDVI conditions. Ts is also underestimated in May, when GPP is still very low. I am not convinced of this distinction .. but in order to support your point, you could color the points in the top right panel of Fig 5 with shades indicating NDVI (or GPP).

Page 16, Line 24, Fig 5 Would be good to indicate the times of the seven snapshots in Fig 5 by vertical lines (solid for all UAS, dashed for augmented with tower data), so it is easier to see when the data was obtained for calibration.

Page 16, Line 28 Do you mean "nearby“ instead of "nearly“?

Page 18, Line 1 I think "be“ should be erased

Page 18, Line 4-5, Fig 6 Can you please indicate in Fig 6 what the red lines refer to? I am at a loss, especially in panel (a). Also, I am not sure how the conclusion "GPP was underestimated under diffuse radiation conditions“ is seen from the Figure, I am assuming in panel (j). Does the point cloud show a trend?

Page 18, Line 6 Add "of“ after enhancement

Fig 7 Fonts in the top and bottom panels are not the same. Fig 5 & Fig 7 I was confused at first about the difference of the Fig 7 to the right panels in Fig 5. I concluded they are the same, just showing different time intervals. Can you collect them in one Fig? It

would be easier to compare.

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