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Hydrology and Earth System Sciences
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Discussions

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

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Response to the review of Referee 2. We have copied the comments of the referee hereunder with our comments appearing after the referee's comments.

1. 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. 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.

Reply: We appreciate the reviewer's insightful comments and suggestions, which were very helpful to improve the manuscript. We totally agree that the great potential of utilizing the simple but effective land surface models to fill gaps between observed surface states and fluxes from remote sensing. Here, we have addressed your comments point-by-point.

Major comments 2. 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, Reply: Thank you for your suggestions. To make the data and parameter sources clear, we have added one figure on the flow chart of this study. Please see Figure 4, which includes details on the model inputs, parameter, outputs and calibration procedures. We also have added the sources of parameter values into the figure. For details, please refer to L1-5 on P14 in the revised clean version. 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 limitations 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.

Reply: Thank you for your suggestions. We agree that there are still challenges and limitations for the more widespread application of the proposed model, particularly when applying models to the large scales and data-scarcity regions. First of all, the SVEN model is a very simple and parsimonious process-based model. For instance, the current soil moisture module in the SVEN model is a simple water balance model with considering one soil layer, which has limited capacity to simulate soil water dynamics particularly in regions with complex landforms. In addition, the soil layer depth refers to the maximum root water uptake depth, which can vary with time, but SVEN simplified this soil depth parameter to keep it consistent. Thus, in our study, SVEN only achieved moderate performance to simulate soil water dynamics and it can be expected that in water limited drylands, soil moisture simulation has a larger impact on the ET than in our site. Additionally, compared to the Penman-Monteith approach, the Priestley–Taylor approach may need adjustment of the aerodynamic term, when extending the study from radiation controlled sites to arid climates. Regarding the model-data integration, our study used a two-objective optimization scheme, there are more advanced algorithms e.g. data assimilation could enable the consideration of data and model uncertainties in the integration process. Moreover, when applying the model with satellite coarse resolution data to the large scale, there will be four major impacts. First, the space-borne remote sensing data have much coarser spatial resolution. If we move the simulation to the large scale with satellite data, we need to find accurate gridded meteorological data as forcing. UAS imagery has limited coverage and thus this study only used one meteorological station data as forcing. As satellite data have coarser pixel sizes, we also need to consider the sub-grid heterogeneity and identify the effective values for model parameters. Note all parameter values of models were obtained from parameter calibration with remote sensing based estimates. For instance, in our study,

we used the look-up tables with soil texture information to identify soil parameter values. In the large-scale simulation with satellite data, the plant functional type and soil type parameterization scheme for different ecosystems and environmental conditions would be needed. However, the integration of accurate remote sensing estimates with land surface models would be beneficial to reduce the dependency of plant functional type parameterization schemes and achieve a higher accuracy to predict land surface variables. In addition, coarse resolution satellite data may have limited accuracy to predict land surface fluxes compared to the detailed UAS data. Applying SVEN with satellite data to large scale, we also need to be careful about the accuracy of remote sensing based estimates and the error propagation from the model inputs to the outputs. Satellite data in the optical and thermal ranges can only provide observations during the sunny weather conditions. However, the UAS data in this study were collected in both sunny and cloudy conditions. We envision that using satellite based data to calibrate model may lead the model estimates biased towards the sunny conditions. We also agree with the reviewer that compared to the Penman-Monteith approach, the Priestley–Taylor approach may need adjustment of the aerodynamic term, when extending the study from radiation controlled sites to arid climates. We have added these contents regarding the model improvement and challenges to the discussion part. For details, please refer to section 4.4 in the revised clean version.

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?

Reply: Thank you for your comments. We have revised Figure 7 to be boxplots to make the results clear. We agree that due to that UAS data collection happens on both sunny and cloudy weather conditions, we did not see significant differences of residuals in simulating surface temperature, net radiation, soil moisture, latent heat flux, and gross primary production for different sky conditions. We have revised the description and for details, please refer to L6-10 on P20.

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-systemsciences.net/for_authors/manuscript_preparation.html

Reply: Thank you for your suggestions. We have revised the abbreviations of variables to be one letter abbreviation as much as possible. For instance, we used ALB to represent surface albedo in the previous version. In the revised version, we used one letter abbreviation A to stand for surface albedo. Please see L15 on P8. (Notably, most studies used the Greek letter α to represent surface albedo. However, α has already been used as the PT coefficient in Eq. 22.) We also have changed soil moisture (SM) to one Greek letter θ. We have changed the wind speed from WS to u. Furthermore, we have also summarized all abbreviations in the supplementary material.

Detailed comments 5. 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.

Reply: Thank you for your suggestions. We have revised this sentence to be clearer. Based on model parameter calibration with the snapshots of land surface variables at

the time of flight, SVEN interpolated the UAS based snapshots to continuous records of Ts, Rn, θ, ET and GPP for the growing season of 2016 with forcing from continuous climatic data and NDVI. Please see L17-19 on P1.

6. 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.

Reply: Thank you for your suggestions. In order to make clearer, we have added the statistics to be in percent (normalized root-mean squares deviations, NRMSD). The NRMSD was calculated as the ratio between root-mean squares deviations and the range (maximum minus minimum) of observations. Please see L20-21 on P1.

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

Reply: Thank you for your suggestions. It is a mistake. We have changed the word to “high persistence”. Please see L25 on P2.

8. 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?

Reply: Thank you for your suggestions. The cutoff frequency is 24 hours. We have revised the sentence in L21 on P9. T_d refers to the deep soil temperature (°C) calculated by applying a low-pass filter to T_s with the cut-off frequency of 24 hours.

9. 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?

Reply: Thank you for your suggestion. Yes, the model needs the wind speed as inputs to calculate the aerodynamic resistance for estimating sensible heat fluxes. The accurate information about the wind speed is important for the model to estimate the aerodynamic resistance to the transfer of sensible heat flux. Wind speed, however, it is not used to estimate the transfer of vapor flux (evapotranspiration) as we used a Priestley-Taylor JPL equation. The PT-JPL model used the PT coefficient (α) with a fixed value to account for the ratio between aerodynamic term and radiation. Thus, the ET is not sensitive to wind speed in the model. The larger contribution to errors in H is actually from the soil, canopy, and air temperature (Chehbouni et al., 2001). After that, uncertainties in soil and canopy emissivity values, canopy height, and wind speed also have measurable effects on the accuracy of simulating H (Sánchez et al., 2008). In addition, the error in the sonic anemometer is very low. With traditional cup anemometers, a larger error, of about 10% of error in the wind speed will translate in an error in H of about 5-10% (depending on the temperature difference) for the type of vegetation in this paper. In SVEN the surface temperature estimates depend on the energy forcing which is constrained by three different energy variables (Rn, H, LE) and soil moisture, apart from the temperature from the previous time step. Therefore, errors in wind speed only affect H should not affect too much the temperature estimates. However, we also agree that without field measurements such as the sonic anemometer, the wind speed data could have large uncertainties from weather forecasting or climate reanalysis data. Applying the SVEN model to the large scale or other data-scarcity regions could have more uncertainties from wind speed data. Thus, we have added these discussions about the uncertainties from wind speed to model performance. Please see L28-29 on P23. Chehbouni, A., Nouvellon, Y., Lhomme, J. P., Watts, C., Boulet, G., Kerr, Y. H., ... & Goodrich, D. C. (2001). Estimation of surface sensible heat flux using dual angle observations of radiative surface temperature. Agricultural and Forest Meteorology, 108(1), 55-65. Sánchez, J. M., Kustas, W. P., Caselles, V., & Anderson, M. C. (2008). Modelling surface energy fluxes over maize using a two-source patch model and radiometric soil and canopy temperature observations. Remote sensing of Environment, 112(3), 1130-1143.

10. 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?

Reply: Thank you for your suggestion. We agree that PT-JPL works better in temperate than drier climate. We also agree that it is good to mention the limitation of this model. We have added this suggestion to L24-26 on P23. Compared to the Penman-Monteith approach, the Priestley–Taylor approach may need adjustment of the aerodynamic term, when extending the study from radiation controlled sites to arid climates (Tadesse et al., 2018; Xiaoying and Erda, 2005). Tadesse, H. K., Moriasi, D. N., Gowda, P. H., Marek, G., Steiner, J. L., Brauer, D., Talebizadeh, M., Nelson, A. and Starks, P.: Evaluating evapotranspiration estimation methods in APEX model for dryland cropping systems in a semi-arid region, Agric. Water Manag., doi:10.1016/j.agwat.2018.04.007, 2018. Xiaoying, L. and Erda, L.: Performance of the Priestley-Taylor equation in the semiarid climate of North China, Agric. Water Manag., doi:10.1016/j.agwat.2004.07.007, 2005.

11. Page 11, Line 27 should probably be "equation 29“ instead of "equation 28“ Reply: Thank you for your suggestion. We have revised it.

12. Page 12 Line 2 Soil water storage has different units here (m) and on page 9, Line 10 (mËĘ3). I think it is fine to stick with m. Reply: Thank you for your suggestion. We have revised all units for soil and canopy water storage to be m.

13. 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.

Reply: Thank you for your suggestion. theta_r and theta_s are from the look-up tables based on soil texture. I have revised Table 2 to include theta_r and theta_s. For details, please refer to Table 2 and Figure 4.

14. 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.

Reply: Thank you for your suggestion. We have revised Table 2 and added one column to indicate the source of parameter values (model calibration or look-up table). Furthermore, we have added Figure 4 to show the model implementation of this study.

15. 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.

Reply: Thank you for your suggestion. We agree that the root water uptake depth vary with time over the growing season. Our paper aims to propose a simple but operational model for interpolation of land surface states/fluxes. So we did not consider such variations of root water uptake depth. To address this limitation, we have added discussion on the shortage of this model into the discussion part. Please find L18-19 on P23. In addition, the soil layer depth refers to the maximum root water uptake depth, which can vary with time (Guderle and Hildebrandt, 2015), but SVEN simplified this soil depth parameter to keep it consistent. Guderle, M. and Hildebrandt, A.: Using measured soil water contents to estimate evapotranspiration and root water uptake profiles-a comparative study, Hydrol. Earth Syst. Sci., doi:10.5194/hess-19-409-2015, 2015.

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

Reply: Thank you for your suggestion. We have added the values for the initial conditions into Table S3.

Table S3. Information on model initial conditions Initial conditions Description Unit Initial value CWSin Initial canopy water storage m 0 SWSin Initial soil water storage m 0.5 Ts0 Initial surface temperature âĎČ Ta Td0 Initial deep soil temperature âĎČ Ta

17. 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?

Reply: Thank you for your suggestion. The values of calibrated parameters are shown in L25-26 on P16. But to make it clearer, we also added the calibrated values of parameters directly to the figure caption (L6 on P17). Regarding whether optimized parameter values are similar or different, we have added the analysis on the optimized parameter values in supplementary Figure S1. Cveg and SWSmax show low variation of coefficients (CVs), and this indicates the parsimony of the SVEN model. Meanwhile, Csat and b show relatively higher CVs. This may be due to equifinality between Csat and b, which relate to soil thermal properties (Eq. 8) and could compensate each other.

18. 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.

Reply: Thank you for your suggestion. We have streamlined these texts. Here we only put the performance regarding RMSDs in the text. Other statistic indices have been moved to Table 3 and Figure 8.

19. 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).

Reply: Thank you for your suggestion. We have revised Figure 7 to be the boxplot showing the simulation residuals and NDVI. We have also improved the interpretation of results. We agree that the model tends to overestimate Ts for most cases. For details, please refer to L14-16 on P17.

20. 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.

Reply: Thank you for your suggestion. We have revised Fig 6 (original Fig 5) by adding vertical lines to show the UAS observations. Please see L5 on P18 in the revised version.

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

Reply: Thank you for your suggestion. We have revised this sentence. Please see L16 on P19 in the revised version.

22. Page 18, Line 1 I think "be" should be erased

Reply: Thank you for your suggestion. We have erased "be" and revised this sentence.

23. 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?

Reply: Thank you for your suggestion. The red lines in Fig 6 refers to that the model simulation residuals are equal to 0. To make this clear, we have added detailed explanation to the caption. Please see L4 on P20. In addition, we have revised the original scatter plots to the boxplots, which could be clearer to identify how the model simulation performance changes with NDVI and radiation conditions.

24. Page 18, Line 6 Add "of" after enhancement

Reply: Thank you for your suggestion. We have revised this sentence to make it clear. Please see L7 on P20 in the revised version.

25. 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.

Reply: Thank you for your suggestion. To make figures clear, we have revised the figure to make fonts consistent. In addition, we have merged Fig 7 and Fig 5. Please see L1-6 on P21 in the revised version.

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