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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Advances in Soil Moisture Retrieval from Multispectral Remote Sensing Using Unmanned Aircraft Systems and Machine Learning Techniques" *by* Samuel N. Araya et al.

Anonymous Referee #1

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The study presents results of soil moisture retrieval using unmanned aircraft system and four machine learning (ML) techniques. The authors conclude that BRT as a ML technique was better (3.8%) than RF (3.9%), ANN (4.3%), SVR (4.4%) and RVR (4.5%) even though the error bars (Figure 6) overlap. While I think the scope fits quite well within the scope of HESS, I think the authors need to improve the presentation and discussion of the results than is presented.

The differences in the methods in estimating soil moisture are infinitesimal to warrant such a claim considering that the error of TDR probes is 3%. The accuracy of the

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results would very much depend on the relationship of the training data set, presence of outliers as well as erroneous values in the training data. In the abstract it is claimed that the UAS was used to create a high digital elevation model as well as quantify relative vegetation photosynthetic status. Yet photosynthetic 'status' results are not provided. It is also not clear what this means.

TPI was observed to highly correlated with soil moisture. This would then mean that areas with more relief were well correlated with moisture. And by extension, NDVI (TTVI) would likely be well correlated with moisture in these areas. While vegetation indices were eliminated from list of variables used, how would vegetation index vary with moisture along the gradient of these convex and concave reliefs? (See: Ryan Engstrom, Allen Hope, Hyojung Kwon & Douglas Stow (2008) The Relationship Between Soil Moisture and NDVI Near Barrow, Alaska, Physical Geography, 29:1, 38-53, DOI: 10.2747/0272-3646.29.1.38)

The discussion of results misses out on comparing and contrasting the findings of the study with those existing in literature. The conclusion of the study is also very weak with no take home message. Most detail is given on limitation of the study and future prospectus of the application rather than study findings.

Figure 5 indicates that NDVI was at maximum at day 186 while moisture was low indicating previous moisture recharge was the likely cause of increased vegetation bloom. What was the role of lag time of vegetation indices (NDVI/TTVI) response to moisture recharge due to previous rainfall?

The BRT model prediction (Figure 8) shows under-estimation of high soil moisture content. The authors do not attempt to explain the reason for this underestimation especially for Julian day 186. Could this be because vegetation variables (NDVI/TTVI) were not included in the model?

It is indicated in paragraph 235 that 12000 images were acquired. Can this be broken down to how many acquisitions per sample site were taken for each band and used for

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each of the 6 days UAS was flown?

Figure S9: There is no PET at 15-day aggregation.

Figure S5: Inference is made of NTWI being linearly correlated with log of acc (flow accumulation) whereas in the main manuscript text it is TWI.

Use of TTVI index instead of NDVI is stated to eliminate negative values. Were there negative values of NDVI for the grassland dominated sample sites?

Calibration panel was used to record stability of the UAS sensor for every flight for the six days and to compute surface reflectance. What were the uncertainty (errors) for the different days?

Use of the word 'meteoric' and interchangeably with 'meteorological' variables when hydrological variable would be appropriate for the two variables used, precipitation and potential evapotranspiration. The article has so many grammatical errors and would benefit greatly if the paper was re-written and organised accordingly to HESS standards. More comments are highlighted in the attached manuscript.

Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2020-271/hess-2020-271-RC1-supplement.pdf

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