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Geoscientific Model Development Discussions

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Interactive comment on "Cutting out the middleman: Calibrating and validating a dynamic vegetation model (ED2-PROSPECT5) using remotely sensed surface reflectance" *by* Alexey N. Shiklomanov et al.

Tristan Quaife (Referee)

t.l.quaife@reading.ac.uk

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The manuscript "Cutting out the middleman: Calibrating and validating a dynamic vegetation model (ED2-PROSPECT5) using remotely sensed surface reflectance" by Shiklomanov et al. illustrates the assimilation of reflectance data observed from aircraft into a component of the ED2 model. In many respects this is an excellent paper. The Bayesian approach taken is state-of-the-art and the use of forward modelling of reflectance for assimilation purposes is desirable for many reasons (and yet surprisingly little progress has been made in this area for land surface studies, making this paper

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Discussion paper

especially welcome). The text is also well written on the whole.

Unfortunately I do have one rather significant concern about this paper, which may at first seem like a subtlety, but really is not. The authors are not “cutting out the middleman” so much as choosing to ignore them. I am concerned that the take home message of the paper for people less familiar with the underlying physics will be that it’s OK to take this approach.

1. Assimilation of BRF and HDRFs.

[Note: I am using the definitions of reflectance quantities from Schaepman-Strub et al. (2006; hereafter S06), which the authors have also done.]

My major concern with this paper is the authors’ misuse of different reflectance quantities. They are not comparing like with like and I do not agree that it is OK to assimilate quantities that are not physically consistent with those being modelled. The Sellers two-stream model predicts reflectances (BHR and DHR), whereas the AVRIS data are reflectance *factors* (BRF and HDRF). Although they are both unitless they are fundamentally different quantities and have different scales.

The authors do devote a paragraph to discussing this, but it is misleading and I am not convinced by the arguments they make. The statement that the ED radiative transfer model predicts BHR is only partly correct. It also simulates DHR and the predicted reflectance is a weighted mean of the two. The authors go on to state the AVRIS observations are most related to HDRFs. This would only be true for overcast skies. AVRIS observations are best represented as a mix of HDRFs and BRFs (although the reality is more complex of course as the down-welling diffuse flux is rarely isotropic). I argue that for most cases the AVRIS data will be most closely related to BRFs as most flight campaigns are under relatively clear skies.

I think part of the problem here arises because S06 define anything with more than 0

Using the Hogan et al (2018) paper to defend this position is disingenuous. One of the

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things that paper shows quite clearly is that solar geometry effects are not well dealt with by classic two-stream formulations for complex canopies. The adjustments to the two-stream scheme made in that paper are required to make the model predicted DHR match the reference 3D model.

I am prepared to accept that in the specific experiment in this paper the limited angular sampling of AVRIS may mean that the overall effect will be small. However, when the authors make statements such as the one at Line 300 (“We therefore conclude that additional computational and conceptual challenges (as well as parameter uncertainties) associated with treatment of angular effects in similar models are unwarranted”) it is very misleading: the take home message is that it is, in general, OK to take this approach. It is most definitely not.

On a related note I also don’t accept the statements on lines 269 and 299 that seem to imply that it’s necessary to have additional parameters to predict directional reflectances. This is not the case and hence also not a valid defence of the approach taken. The set of parameters required to define a two stream model can be used also to define a BRDF model derived from the same underlying assumptions (i.e. semi-infinite, plane parallel turbid media with point scatterers).

I propose the following modifications to address these issues:

a) Change the title to remove “cutting out the middleman.”

b) Modify the discussion around this point, including stating clearly that the reflectances and reflectance factors are physically different things and that, in general, it is not appropriate to do assimilate one into a model that predicts the other.

c) Quantify the differences between the BRFs and the modelled BHRs. I noticed, poking around in the github repository, that the SAIL model is included and hence, presumably, it is trivial to take representative posterior parameters values and model both BRF (from SAIL) and BHRs. If the authors’ assertion that it shouldn’t make any difference is correct then this will help to defend that. The observed reflectance factors should also be compared against SAIL predictions. I would be happy to iterate on the experimental procedure with the authors.

2. Is ED2 actually used in this paper? It seems that it is only a relatively small component of the model (i.e. the canopy radiative transfer scheme) that has been extracted. I think the title is slightly misleading in this respect. More important, I am not sure calling the code ED2-PROSPECT5 is appropriate. Perhaps EDR-PROSPECT5 would be better?

3. How are correlations between spectral channels dealt with? Do the authors use all of the AVRIS observations in the domain 400-1300nm? The errors in spectrally adjacent bands will be very highly correlated and the overall information content will be much lower the same number of independent observations.

Minor comments:

L27 The MacBean et al. (2018) paper referenced here does not assimilate a derived product in the sense the authors mean it. The assimilated data in that paper is solar induced fluorescence which, whilst it is "derived" in the sense that it is not what is being measured by the sensor, is no more derived that the surface reflectance data used in the manuscript under review. I suggest finding another reference here.

L35 "Meanwhile, the estimating" -> "Meanwhile, estimating"

L61 Surface reflectance is not assimilated in Zobbitz et al., (2014). The title is misleading (and I regret not standing my ground on that issue when we submitted that paper!); in fact fAPAR data is assimilated.

L85 Not sure this line should finish with a colon.

Eqn 3 What is meant by r_{n+1} (and other variables with that subscript)? If I have understood correctly this is there are n layers, so what is the reflectance of the $n+1$ layer? (I am sure I have just missed something here, but it was not obvious).

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L102 “tau” should presumably be the Greek symbol τ.

Eqn 7 I am confused by this, why is *forward* scattering defined by the sum of R+T? This is just the total scattering isn’t it?

L188 This a different definition of X from earlier in the paper. I suggest finding a different symbol.

L273 “DALEC-predicted foliar biomass, which required introducing an additional fixed parameter (grams of leaf carbon per leaf area) present in neither model.” This statement is incorrect – that parameter already existed DALEC.

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