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Interactive comment on "A process-based *Sphagnum* plant-functional-type model for implementation in the TRIFFID Dynamic Global Vegetation Model" *by* Richard Coppell et al.

Anonymous Referee #2

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This paper introduces into the TRIFFID model, a generically long-held ambition of many land surface and dynamic vegetation modeling groups - to treat the physiology of non-vascular plants (specifically spagnum moss), without stomatal control, in the same framework as that which is typically deployed to simulate the gas exchange of vascular plants. In that sense, this paper is a welcome addition to the literature and could, I imagine, form the basis of many developments that are based on top of these fundamental updates.

Data on the physiology of spagnum moss are difficult to come by, and an initial assessment of the validity of the model was made against available data.

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Discussion paper

In general, I found the discussion of the data sources quite difficult to follow through the paper, and also, the results section is missing a way of comparing the spagnum model to the standard photosynthesis scheme used in JULES-TRIFFID.

Further, I feel like the paper is not sufficiently 'polished' in tems of the clarity of the writing, nor the description of the model modifications, to merit publication at this stage. Most critically, the main text is missing a discussion of why and how the primary modifications to the photosynthesis scheme were made. It seems like the paper needs a few more iterations between coauthors before it reads clearly enough to those unfamiliar with the work or the surrounding literature.

Lastly, while the first author has gone to appropriate lengths to make the code for these modifications available, they are largely not useful without the full code infrastructure of the JULES model to place them in context. I appreciate very sincerely that this is not the fault of the authors, but it is in my view untenable to retai this state of affairs in 2020.

Specific Comments

P1 L10: I think it's not necessary to say 'more recent' DGVM, most models I know of have had sub daily physiology for at least 20 years.

P1 L13: The paper title says 'for implementation in' whereas this suggests it is already implemented in TRIFFID. Which is it?

P2 L1: in this paragraph the focus is on DGVMs, but these are a subset of land surface models that all could benefit from improved representation of mosses. i suggest making this less focused on DGVMs (given this is really a paper about representing physiology and not vegetation dynamics) and more on LSMs in general.

P2 L16: This section needs references expanding outside of the JULES/TRIFFID literature. P2 L31: A clear statement on the magnitude of how much carbon is derived from sphagnum would be useful hear. Earlier, you refer the half the carbon in Northern peatlands, but an estimate of their spatial extent and/or carbon store would help frame the importance of the representation of sphagnum better. P2 L31- P3 L5: This section is a little muddled and could do with reorganization.

P3 L10: This paragraph states that this is in fact an existing moss physiology module in JULES, and then later that there is not. Really, a more focused discussion is needed on the deficiencies of the existing sphagnum representation.

P4 L1: Do you mean NEP here (given previous references to soil?) P4 L8: The word 'assemblage' as used in this context is something I, for one, am not extremely familiar with. Can you define/explain it before using it here?

P4 L20: I do not think that the term 'PFT model' has any specific meaning here. P6 L23-29: This section on photosynthesis doesn't really tell us anything specific about moss, other than that it is a C3, which doesn't seem to need a whole paragraph?

P7 L2: Across what range does it vary between species?

P7 L6: And also dew?

P7 L7: Is there a point to add concerning the thermal insulation properties of the moss layer(s)? P7 L14: What -does- the model contain? That seems like an important thing to add before stating what it doesn't? Why not modify the canopy water storage terms to account for the moss properties? P7 L15: What is microform position?

P7 L21: Again, 'PFT model' is redundant here.

P7 L27: Indeed, but Prof. Cox is also the last author on the recent implementation of plant hydraulics in JULES (Eller et al. 2020) following from numerous other implementations of hydrodynamic schemes into other land surface models. I suggest you reframe the syncing with an explicit water uptake scheme as something to be incorporated into later versions, rather than digging any deeper into the defense of the previous empirical scheme.

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Discussion paper

P7 L28: Where was the sphagnum in question located? I feel like this needs a little more detail as it is the primary dataset employed here.

P8 L1: A fixed maximum value of what?

P8 L6: I'd say "for vascular plant in the TRIFFID model"

P8 L7: Did Williams and Flanagan use JULES/TRIFFID? Or is this a different approach altogether (in place of is a confusing word choice here).

P8 L9: Successful approaches to what? I'd be slightly more circumspect about attributing 'success' in the context of model elements of land surface models in the absence of very comprehensive and specific benchmarking of particular elements. Models have so many degrees of freedom that one can trivially get the right answer for the wrong reason a lot of the time.

P8 L25: A note on the potential pitfalls of used smoothed photosynthesis can be found in this recently published analysis by Walker et al. (2020)

P8 L20-27: This part is difficult to follow given that one has to skip between the table and the text. I'd recommend integrating the explanation of what you have done into the text and not using the table, which is a nce idea in principle but actually quite hard to follow, in particular wrt the mathematics of the solution of the photosynthesis/gs scheme. Why was it necessary to change the solution? I don't really understand from what is written here why simply adding a constant stomatal conductance does not suffice. Further, the references to Druel et al. and Dimitrov et al. need expansion here, given that this section really explains the key developments tht you have made.

P9 L3: How is LAI a model parameter? Do you mean LAImax? LAI is an output of TRIFFID, unless I'm very much mistaken? Also this paragraph could do with having the parameter table closer to it in the text, otherwise these names come rather out of the blue.

Table 2: Under the first heading "carbon gain and mass balance" is the description

of the Beta factor controlling stomatal responses to soil drying. This needs correcting and/or describing better. Table 2: “Photosynthesis Machine” should probably just be “Photosynthesis” Table 2; Why is respiration in this table when the modification of the parameters would more naturally exist in table 3?

Table 5: Why organize the information in this way? It requires one to shuttle backand forth between at least three sections at once to figure out what information is used where. I would reorganizt this in to section in the text by process, and perhaps keep table 3, (merged with 4?) but lose 2 and 5.

P 17 L 1: What criteria does a dataset need to meet to be useful for calibration and /or validation of this model?

P17 L8-10: I’m not sure what the ‘correction’ is that you are trying to describe here. P17 L 12: Is this ‘correction’ still trying to account for 20% non-sphagnum vegetation in the first dataset? This is pretty hard to follow in the absence of familiarity with these references (and given that representation of moss in DGVMs and LSM is essenttially a new topic, I think you can safely assume the majority of your readers will not be familiar with this literature).

P17 L20: This statement on NPP seems a little redundant.

Figure 1: It is generally customary to have model output as lines and field observations as points in these types of figures.

P18 L5: Reiterate what type of data are available fro Riutta et al.

P18 L6: It should not be especially hard to generate an RMSE, or R2 value? I appreciate that getting the general patterns of behaviour is a higher order concern, but it sees like it should be easier to just calculate some statistics than to argue why it is unnecessary to do so.

P18 L9: This section would be useful earlier on.

P19 L5: Which earlier data? This is making things unnecessarily hard on the reader. Also, LAI is not a parameter here, it is presumably an input, but I remain confused about why it is not an output of the fully TRIFFID carbon cycle physiology. In fact, if the model is taking LAI as an input rather than predicting it from some sort of allocation/turnover model that is downstream from the moss gas exchange model, then this needs to be much more dlearly delineated earlier on. Further, as you have discussed, TRIFFID is a dynamics vegetation model and as such predicts the distribution of moss as well as its physiology. Up to now, that element of model testing has been absent and therefore, I suspect, the scope of this model development needs greater elucidation in the methods and introduction sections.

P19 5-7: Also, I am not sure why LAI is described as a ‘curve fitting’ factor, when it has a clear biological meaning. Were no observations of LAI or FAPAR available? Is that a challenging aspect of working with moss?

P19 :L9-11: Is 1-3 the realistic rang eof LAI? Could GPP in principle be higher for optimal boundary conditions? I am not sure what to take from this description of the range.

P19 L 16: The ‘Thus’ in this sentence is redundant as it doesn’t really follow from the discussion in the previous paragraph. Further, before making this statement, you need to reference the relevant figure and analysis. Also, the discussion of the ‘residuals’ is incomplete. Residuals of what? This seems to me to be an extremely abbreviated description of the results, and I am not sure, given that, that it is necessary to have these appendices.

Figure 2; Here it would have been helpful to illustrate what the default TRIFFID model does in the absence of the moss physiology. How wrong does it get the shape of these curves?

P21 L3: Here, again ‘PFT model’ is uninformative. Further, at this point, it would also be good to actually escribe the scope of the model that you have developed, which I would describe as a ‘gas exchange’ model for non-vasculars plants.

P21 L6: On p19 L20, you state that you did in fact carry out ‘calibration’ and then here it is stated that no calibration was needed, before then in the next sentence again stating that Rg was calibrated, and then LAI? This obviously needs to be less contradictory. Also I think this is the first time you have discussed the literature on LAI in sphagnum?

P21 L12: There ARE insufficient test data. . ..

P22 L2: Simple climate correlation of what against what?

P22 L25: Many existing models have variable tissue Nitrogen concentrations, (see Davies Barnard et al. 2020) and other optimally adjust Vcmax as a function of environmental conditions. (see review in Franklin et al. 2020). In general,those types of model would be better equipped to deal with this issue (which is not particularly specific to mosses. . .) Again, the literature here needs to expand outside the JULES-TRIFFID domain.

P23 L9: The resolution of this issue, being the central development of the paper, does really need a better explanation in the main bulk of the text. Having read to here, but not the appendices, I am non-the-wiser about what was accomplish, nor why it was needed.

P23 L21: A DGCM does really imply that you are predicting the distribution of the individual PFTs. Have you tested that aspect of TRIFFID with spagnum, and if not, how did you test the model in the absence of competition with other vegetation types? The developments necessary to go from the gas exchange elements to a full biogeochemical and then dynamic vegetation scheme need to be at lest briefly discussed somewhere.

P32 L9: In my opinion you shouldn’t be in any way grateful to the Hadley Centre for only allowing a part of this development to be made public. Continuing to retain the JULES code as a closed repository increasingly contravenes journal guidelines. I have, on many occasions as an editor and reviewer, been forced to make exceptions to journal policies (to allow early career researchers to publish their hard work) because of this situation. For example, to get the JULES code here, I would need to break my reviewer anonymity and sign up for access, which violates the GMD policy as per:

“Where the authors cannot, for reasons beyond their control, publicly archive part or all of the code and data associated with a paper, they must clearly state the restrictions. They must also provide confidential access to the code and data for the editor and reviewers in order to enable peer review. The arrangements for this access must not compromise the anonymity of the reviewers. All manuscripts which do not make code and data available at this level are to be rejected. Where only part of the code or data is subject to these restrictions, the remaining code and/or data must still be publicly archived. In particular, authors must make every endeavour to publish any code whose development is described in the manuscript.”

I appreciate that the author of this paper does not have jurisdiction to change this situation and has in fact gone to appropriate lengths to make the relevant code available under trying circumstances, but I find this feature of putting the onus on PhD students and early career scientist to force exceptions to journal rules to be highly regrettable.

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