Interactive comment on "Estimating global carbon uptake by lichens and bryophytes with a process-based model" *by* P. Porada et al.

Anonymous Referee #2

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General comments

This interesting manuscript describes, in somewhat extreme detail, the function of a process-based model that aims to estimate bryophyte carbon uptake at global scales. This is an extremely important subject, as such non-vascular plants and processes are rarely and/or poorly represented in global models, yet exert outsize effects on the global carbon cycle. The ms is generally well written, and the equations individually clear (if collectively overwhelming). The 'strategies' approach used here is interesting, and to my knowledge has not previously been applied to this subject.

There are some significant problems however.

First, the authors say, essentially, that detailed model validation is "beyond the scope

of this study" (p. 3751), and their efforts in this area are indeed minimal (Figure 7, about which see comments below). But this isn't acceptable; you're presenting global estimates and asserting that your numbers are consistent with observed data, without proving this in the least. (Certainly Figures 5 and 7, which show net carbon uptake peaking at 18 gC/m2/yr, don't give one confidence; how could northern peatlands ever form with such a low NPP?) Nor is there any comparison to other bryophyte models that have been used on, if not global, site- to regional bases (see e.g. Turetsky et al. 2012, 10.1111/j.1469-8137.2012.04254.x). Not even Clymo's classic work is cited!

Typically, these sorts of papers are split into two, one documenting the model (equations etc.) and one validating/testing it, and I strongly suggest you take this approach.

Second, most ground bryophytes (specifically, the highly productive and widely distributed Sphagnum spp.) are highly sensitive to very small-scale vertical differentiation: micro-topographical changes produce large changes in carbon status (see, among many examples, Nungesser 2003). Can a "process-based model" run on a two-degree grid just ignore this small-scale variability and still be accurate?

Finally, there are previous few details on the model itself, beyond equations. The reader has no idea of the model's timestep, for example.

In summary, the current ms has many points of interest, and I applaud the attempt to produce a global estimate of bryophyte carbon fluxes. The current ms is heavily weighed down in equations though, minimal in its validation, and lacking comparison to previous modeling efforts in this area. I really think it needs a fundamental re-thinking for clarity and rigor in these areas.

Specific comments

1. Page 3741, line 21: I had understood this is a distinctly nonlinear process–see for example Williams and Flanagan 1998 (10.1046/j.1365-3040.1998.00292.x). Is this not true? What kind of bias might be introduced by assuming it's linear?

2. P. 3748, l. 16-: it would be useful to give some details. What language is model written in? How long does it take to run? What's the timestep (yes, I see equation B32 but that's no help)? Is it open source and freely available, if so from where?

3. P. 3749, l. 26: highest values reached in forested areas in the boreal zone? This seems really problematic, as most field measurements of bryophyte NPP show the highest values in open peatlands

4. P. 3754, l. 18: Figure 7 doesn't seem to support your assertion of "good agreement". Presenting some statistics or other quantitative information is really necessary here

5. Table 1: by "net carbon uptake" do you mean NPP? Use a standard term, here and throughout. How were these studies selected? There are many others: Harden 1997, many R.K. Wieder papers, Bond-Lamberty 2004, Oechel, Schuur, Strack, Turetsky, etc.

6. Figure 7: this is a very poor way to present model agreement; the log scale makes it very difficult to compare points. How about a modeled:observed plot with 1:1 line?