

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

### **Anonymous Referee #1**

Received and published: 19 April 2013

The manuscript “Estimating global carbon uptake by lichens and bryophytes with a process-based model” aims at estimating the contribution of these organisms to terrestrial net carbon uptake using a process based model. There are few if any previous studies trying to incorporate these organisms into global C – models so in that respect this study is both *novel* and of interest to a broader audience. The major strength of this study is the modelling part *per se* in combination with the emphasis to also include those characteristics of lichens and bryophytes where they differ from higher plants. For the model the authors have chosen 15 “strategy parameters” of importance for a processed based net CO<sub>2</sub> uptake model. The majority of the parameters are similar to models for higher plants while others are more specific for lichens and bryophytes; i.e. parameters related to the uptake, retention and losses of water and the presence of a photosynthetic carbon concentrating mechanism (CCM). Since this

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is one of the very first attempts to estimate global C uptake of lichens and bryophytes one may argue that it is not possible to include all various details and peculiarities of these organisms already at this stage. However, I have found some weaknesses in the model and the manuscript that should be addressed or at least discussed already in this study.

1. Being poikilohydric the relationships between metabolic activity and environmental water supply is a key parameter in any model attempting to estimate productivity of lichens and bryophytes. The literature dealing with this is extensive (while very few are cited in the manuscript) and includes some later studies presenting both models and empirical data on the relationships between rain, air water potential, rates of uptake and loss of water, and the rate of metabolic activation (or deactivation) of lichen photosynthesis. These models may be too detailed to include in the present study but should be cited and commented, particularly the additional constraint of delayed activation of metabolism. See Jonsson et al (2008) *Oecologia* DOI 10.1007/s0442-008-0990-5; Lidén et al (2010) *PCE* DOI: 10.1111/j.1365-3040.2009.02111.x; Jonsson Cabrajic et al (2010) *PCE* doi: 10.1111/j.1365-3040.2009.02110.x. The presented model might therefore be improved by including a 16<sup>th</sup> strategic parameter accounting for metabolic activation kinetics. Apart from slower or faster activation of lichen metabolism after hydration another well know characteristic of lichens is the difference between those with a cyanobacterial photobiont *versus* a green algal where the former requires liquid water for activation. An additional strategy parameter (trait or characteristic) would then be to include whether liquid water would be required for metabolic activation.
2. The model also includes some trade-offs (Table B10), one being the increase in respiration with increased Rubisco (protein) concentration of the cells. However, lichens are also symbiotic organisms with a fungal component that does not have any photosynthesis (or Rubisco) but possibly contributes to quite a large extent

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to respiration. The here presented model has avoided any mentioning of the fungal component of the lichens. There might be a good rationale behind this but those arguments should be presented. Moreover, the model assumes a constant relationship between Rubisco and maintenance respiration across strategies (species), even though at least one study has shown that photosynthetic capacity increases more than maintenance respiration with increased nitrogen content across lichens, albeit also being dependent on morphology and photobiont taxa – see Palmqvist et al. (2002) *Oecologia* DOI 10.1007/s00442-002-1019-0.

3. The 14<sup>th</sup> strategy parameter incorporated in the model is the presence or absence of a CCM. The addition of this parameter was justified because of its assumed energetic demand and maybe because the CCM might partly compensate CO<sub>2</sub> limitation in water saturated thalli (although the latter is not clearly discussed by the authors). A CCM is indeed present in the green algal and lichenized genus *Trebouxia* and in the cyanobacterial genus *Nostoc*, but nothing is known regarding the energetic costs of the mechanism when present in lichenized photobionts. Instead, the authors have inferred this from what we know from their free-living relatives. But this is not very clear from the text and should be introduced better. Overall, we know very little about why lichenized photobionts possess this mechanism and it may well be that other characteristics of these algae and cyanobacteria have other traits that have made them a good choice for their fungal partner. The N<sub>2</sub>-fixation ability of cyanobacteria is one such trait which is even more expensive in terms of energy demand. Overall, the inclusion of the CCM in the model needs to be better described, particularly in view of the conclusion one can make from Fig. A2 c.
4. The structure of the manuscript and the separation of equations, tables and figures makes it quite difficult to follow how and with what data the model was parameterized. The extensive cross-references between the different sections, equations and tables is relatively confusing so one easily gets lost regarding

which data that was finally included and for what reasons. I would suggest that the Tables and Figures in the B-section are presented together with their corresponding equations – now in another B-section before the References - and that the references to data for these equations are presented together with their corresponding equations, tables and figures.

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Interactive comment on Biogeosciences Discuss., 10, 3735, 2013.

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10, C1114–C1117, 2013

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