

# ***Interactive comment on “Estimating global nitrous oxide emissions by lichens and bryophytes with a process-based productivity model” by Philipp Porada et al.***

## **Anonymous Referee #1**

Received and published: 15 November 2016

Dear Dr. Porada and colleagues,

I have reviewed your manuscript “Estimating global nitrous oxide emissions by lichens and bryophytes with a process-based productivity model”. In this manuscript, the updated model LiBry is used to estimate global respiration of lichens and bryophytes. Then global nitrous oxide emissions from lichens and mosses are derived from the simulated respiration amounts using a conversion factor. This is an important study, as the role of lichens and bryophytes in global biogeochemical cycles has been understudied. This is especially true for nitrous oxide, as exemplified by the fact that this paper is one of two global estimates for of N<sub>2</sub>O emissions for lichens and bryophytes. The model seems sound to me, and I appreciate the valid points the authors make

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about the limitations of their emission estimates. However, there are some issues that need to be addressed within the manuscript.

## General Comments

1. Conflation of mosses & bryophytes, biological soil crusts and microbial surface communities, and other terms- Please clarify if this paper is about one of these, all of these, or some of these. These terms are not interchangeable. The first paragraph of the introduction begins by talking about microbial surface communities (specifically biocrusts) in a dryland setting. However, the author's study is seeking to address global N<sub>2</sub>O emissions of lichens and mosses, as declared in the title. While lichens and mosses occur in biocrusts, the initial focus on biocrusts takes away from the global implications of the study and the potential importance of lichens and mosses to N<sub>2</sub>O emissions in other ecosystems (as the data later goes on to suggest). What is needed is less conflation of biocrusts with distinct units of lichens and mosses throughout the manuscript. This association of biocrusts with lichens and mosses is true for drylands, but the conflation breaks down very quickly in different ecosystems.

2. Clarify the players in N-cycling processes and the mechanisms early –This paper focuses on emissions of N<sub>2</sub>O actually sourced from the mosses and lichens themselves, not from nitrifiers or denitrifier microbes. Readers should be better introduced to this idea early on, so they are not confused. In the third, fourth, and fifth paragraph of the introduction, the focus is almost entirely on the fixation of N in microbial communities. These paragraphs are not entirely relevant to your study, and serve to confuse the reader.

Mechanisms for how microbial compounds release gaseous nitrogen are included, but there is no mention of the mechanisms for lichens and mosses until Page 9 Line 20. The process should be highlighted in the introduction. As follows, Figure 1 with its focus on the microbial communities mechanisms for N<sub>2</sub>O emissions is largely irrelevant to this study, and could be replaced by an example of lichen and mosses emission and

fixation pathways.

3. Make introduction global in scope - The results indicate that nitrous oxide emissions by lichens and bryophytes are highest in humid tropics and subtropics and yet, these regions are not even mentioned in the introduction. There is a lot of text spent on the N-dynamics of drylands, but I think it is more important to broaden the scope of the introduction and address the N-dynamics of the ecosystems that end up being most significant to global N<sub>2</sub>O emissions of lichens and mosses.

4. Expand the discussion: the discussion and conclusion focuses almost entirely on comparisons and short-comings of the model, while the introduction focuses heavily on N-cycling and mentions implications of N<sub>2</sub>O emissions. A paragraph tying the discussion back into the topics covered about lichens and mosses in the introduction, and our increased understanding of N<sub>2</sub>O emissions based on this study would be more satisfying to the reader. The authors begin to do this on page 9 line 15-18, but expanding on it or emphasizing it at the end of the manuscript would make for a stronger overall narrative.

#### Specific Comments

1. Page 4 line 8-16 Clarification of methods for relating N<sub>2</sub>O emissions with respiration –The explanation of how N<sub>2</sub>O emissions are derived from respiration states that they were converted from values determined experimentally from N<sub>2</sub>O emissions by microbial surface communities. It is important to note that lichens and mosses are not microbial surface communities. Mosses are plants! Neither are microscopic. I see later that it is stated Lenhart did measure samples of lichens and bryophytes. However, I had to read Lenhart et al to find that these measurements occurred when the lichens and mosses had their substrate (and therefore soil microbial communities) removed. Both of these points (1. Measurements were taken on lichens and bryophytes, not microbial surface communities and 2. removal of substrate during measurement) need to be made abundantly clear.

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Also the morphological range of lichens and bryophytes used to get this conversion factor should be briefly mentioned. For instance, is respiration and N<sub>2</sub>O emissions as tightly coupled with rock lichen and epiphytes?

2. Page 9, line 12 The Elbert paper that is being cited includes cyanobacteria in its carbon estimates while this one does not. I would have guessed that the inclusion of cyanobacteria should make carbon estimates higher than carbon estimates from LiBry that focuses on just lichens and mosses. Please address this point.

3. Page 10 line 30-35. This paragraph is again conflating microbial surface communities with lichens and mosses. If the end goal is to assess model-based estimates of N<sub>2</sub>O emissions by microbial surface communities that contain lichens and mosses, then this paragraph is appropriate. However, that needs to be stated clearly.

Technical Points:

Page 6, line 12 “cannot not be simulated, yet” change to “cannot yet be simulated”

Page 10, line 17-18 Sentence fragment. Do you mean that the uncertainty you need to discuss involves the methods you used for estimating respiration and deriving N<sub>2</sub>O emissions from those respiration rates? If so, please state that more clearly.

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