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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 #2

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Review for "Estimating global nitrous oxide emissions by lichens and bryophytes with a process-based model" by Porada et al.

General comments:

The authors present a new approach to estimate global N2O emissions from lichens and bryophytes. In this approach they use empirical relationships between N2O and respiration to derive N2O emissions from simulated respiration fluxes. With this combination of modelling and empirical relationship they can represent the effect of climatic conditions on N2O emissions. Relating N2O emissions to climatic conditions is of course particularly interested in light of climate change. They highlight this, while they do not discuss that the sensitives in their N2O fluxes reflects the sensitivity of respi-

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ration. A more detailed discussion on potential differences in climatic sensitivities of N2O emissions vs respiration and related uncertainties is necessary. They discuss the advantage of their new approach vs previous estimates based on NPP and they also discuss shortcomings and general uncertainties related to N2O emissions by lichens and bryophytes. They state that their model does not simulate nitrification and denitrification, however, it does not get clear if the model is capable of simulating N fixation and N deposition. Those fluxes would have a more direct functional link to N2O emissions as compared to respiration. So in addition to referring to an alternative approach of using NPP, it would be beneficial to refer to other alternatives and related advantages or disadvantages of their approach. Another aspect still missing in the discussion is the general uncertainty related to estimates of the global abundance of lichens and bryophytes. With this extension of the discussion and the more specific comments below, I recommend the study for publication.

Specific comments:

Page 1:

Line 2 and 3: “This finding relies on . . . which are combined with . . .”: It gets not very clear what the authors mean by “combined”; this is explained better later in the paper, but this sentence sounds too vague, please rephrase more clearly

Line 21: “In a first ecosystem-based upscaling approach”: is this approach based on modelling or measuring on the ecosystem level? So is the alternative approach by Porada et al. (2013) different because they use a model (vs. observations) or because they model at global scale (vs. at ecosystem scale)?

Page 2:

Line 6: how can they influence weathering by their demand for phosphorous?

General remark: for those organisms fixing N, would it not make sense to link N2O emissions to fixed N? Or in general to N taken up, including fixed N; maybe this ap-

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proach is not feasible in your case because of technical or modelling issues, but it would still be worth noting why you use respiration and not a N-related flux;

Line 8: are uptake into microbial biomass and leaching the only processes? Later you also mention gaseous losses, and your paper is about N2O emissions, so I guess you can expand this list. And is the uptake of fixed N relevant enough for the study for being dedicated one paragraph?

Line 11: how likely is it that nitrification and denitrification occur? As you derive global N2O emissions, do you distinguish between microbial communities that are and those that are not capable of nitrification or denitrification? If not, this fact should be discussed.

Line 12: what is meant by "surrounding atmosphere"? I suggest to delete "surrounding"

Line 13: ammonia is not formed during nitrification or denitrification

Line 17-19: who used those data?

Line 19: N2O is not in general the main ozone depleting substance, but the main ozone depleting substance that is still emitted; Other ozone depleting substances are not emitted any more, but still more destructive for ozone than N2O

Line 22 ff: in this paragraph you focus on denitrification, what about nitrification?

Line 22 ff: Regarding the upscaling of N2O emitted by lichens: how uncertain are estimates on global lichen and bryophyte occurrence?

Line 25: relation between N2O and fixation rate seems to be available from the study by Barger et al. 2013, why not using this relationship instead of linking N2O to respiration?

Page 3:

Figure 1: Figure 1 shows nitrification and denitrification, and the dependence of N2O emissions to NH4 and NO3 concentrations; It also shows that NH4 and NO3 depend

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on fixation and deposition; In contrast to Figure 1, you derive N2O emissions from respiration; Is there a link between respiration and other N fluxes such as fixation? Is it pure coincidence that respiration and N2O fluxes show an empirical relationship?

Page 4

Line 8 ff: ; is the relationship between N2O and respiration not driven by temperature change? Also moisture dependency of respiration might be different to N2O, especially as nitrification and denitrification have different optimum ranges; respiration differs between species... does N2O/respiration not differ across species? From what I found in cited literature, moisture dependency of respiration stays 1 for moisture values exceeding an upper limit; this is not true for N2O, as under very anoxic conditions, N2O is reduced further to N2: so here, the sensitivity of N2O on moisture differs from the one of respiration! This needs to be discussed at least.

Line 22: "...variation in climatic conditions": in the approach used in this study, the sensitivity of N2O emissions on climatic conditions mirrors the sensitivity of respiration; the authors do not discuss potential differences in sensitivities and arising uncertainties in their results, please add this to the discussion

Line 25: the variations in N2O emissions simulated in the study mirrors the variation in respiration; hence, claiming that their study helps to assess the variation in N2O emissions is a bit of a long shot; some clarification on this, and also on how reliable the linear relationship they are using is under different climatic conditions would be necessary

Page 5:

Line 4: "Since it is assumed in LiBry that lichens and bryophytes cannot grow together with crops, growth is low in these regions...": why do they grow at all, if it is stated that they cannot grow together with crops?

Figure 2: d) Tropical Forest Canopy: It seems like the small values come mainly from

very few grid cells at the edge of the tropics; if those few grid cells were excluded, range, and average value would look different; maybe I get this impression only due to the chosen color range, but I still think it wold be worth checking

Figure 2: what is the difference between organisms growing on ground or on leaves and how is this represented in the model? Here, that distinction comes up for the first time, if it is important to distinguish those two groups, then please add more explanation on it already in the introduction

Figure 2: values for desert regions are presented, while the Sahara is grey: please explain

Page 9:

The authors showed the ratio between respiration and NPP, however, they do not explain in how far respiration is dependent on NPP in the model; as N2O is somehow calculated from respiration, the link between N2O and NPP does not get clear; given this, the authors have a rather large focus on the NPP evaluation while it is not obvious how NPP affects N2O emissions in their approach

Page 10:

Line 9 ff: Diversity of estimated N2O emissions driven respiration, please add notes and discussions on that

Line 15: "functional diversity of lichens": I guess there are many kind of functional diversities and not all are related to N2O. . .. phrasing is a bit vague

Line 16: "considers the most important sources of variation. . .": this might be true for respiration, but you do not explicitly calculate N2O emissions, they mirror the sensitivity of respiration

Line 23 ff: one option to assess the uncertainty regarding wfps for N2O anyway could be to add a sensitivity of the linear relationship between N2O and respiration on water

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content and test different ranges

Line 32: I assume not the measurements suffer from uncertainties, but that rather the results presented to not provide any information regarding uncertainties

Page 11:

Line 2: another shortcoming of manual chamber measurements is the limited temporal resolution which can make a huge difference in cumulated fluxes (Barton et al. 2015, Sampling frequency affects estimated of annual nitrous oxide fluxes, Scientific Reports)

Line 4: I don't really understand this sentence. How is water, temperature and nutrient conditions influenced by experimental setup? N2O emissions are driven by those factors, so it is quite logic that N2O emissions show a similar heterogeneity, independent of the experimental setup

Line 6: This sounds as if you refer to experiments with for instance application of fertilizer, that would in fact influence nutrient conditions by the experimental setup; if so then please phrase it more clearly

Conclusions: There are hardly any conclusions in the conclusion section; The first three sentences are a short summary of the study, the last sentence emphasizes vaguely how additional measurements could be beneficial; In my opinion you can draw more conclusions from your study, so please take a bit more care about this section. It is the last thing people read, and the way it reads now, it leaves at least me with an unsatisfied feeling about what actually your main conclusions are

Technical comments:

Line 8: units Tg N2O yr-1 or Tg N2O-N yr-1 ? – please specify the units regarding N2O emissions throughout the manuscript

Line 19: units: Gt C yr-1 or Gt CO2 yr-1 ?

Page 2:

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Line 5: citation style

Line 17: N_2O is already explained in line 13

Page 5:

Figure 2: units: change from [g m-2 yr-1] to [g C m-2 yr-1]

Page 9:

Line 15: again unit: Tg N or Tg N_2O

Line 17: add blank after 25 %

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