Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-324-RC2, 2018

Interactive comment on “Global NO and HONO emissions of biological soil crusts estimated by a process-based non-vascular vegetation model” *by* Philipp Porada et al.

Anonymous Referee #2

Received and published: 21 December 2018

The Porada et al. article uses a process-based modeling approach with the LiBry model to determine the global areal coverage of biocrust types responsible for emitting NO and HONO, which are important atmospheric chemical constituents for OH. They then relate biocrust type to water saturation to determine emissions of these two constituents. The paper is well written, has sufficient validation and sensitivity analysis, and concludes with values similar to an empirical upscaling study, but for a more restrained area, so that, in fact, the emissions are actually even larger. The wetting event validation comes from four sites in a single South African location, so would be good if there was an attempt for more global validation of this variable, as it is key to how emission are modeled. There are quite a few things that can be revised to help clarify

the paper, as suggested below.

1. The four biocrust types are light and dark cyanobacteria, chlorolichen, and mosses, yet the LiBry model is Lichen and Bryophyte. At times bryophyte is used instead of mosses (p. 20 line 8), so should just specify that up front and then use “mosses” consistently throughout. 2. P. 2, line 19 – 100% of N2O emissions in dryland regions is what percent of total N2O emissions? 3. More explanation of physiological strategies in LiBry. A few of them are used to partition between the four types as in Figure 2. But in section 2.4 (first sentence) it is mentioned that there 3000 physiological strategies, so it is not clear what all these strategies are referring to. Also, some further explanation of how photosynthetic capacity, height, and CO2 diffusivity are used in the model would be helpful. 4. At end of Introduction there are two main extensions of LiBry mentioned – assigning physiological strategies to the four types (so, is this really just defining new types by physiological strategies – would make it clearer to describe it in this way); the dynamic surface cover model described in Figure 3; and how about adding determining emissions of NO and HONO to the model? 5. The fundamental assumptions from Figure 1 that underlie the emissions model are pretty simplistic. So, how about further discussion of what controls water saturation – not just in terms of water balance, but how important is the uptake of water by the photosynthetic biocrusts? 6. NO and HONO emission are based on water saturation and Q10 – is it simply these two terms multiplied? Include an equation that explicitly states how it is calculated. 7. Since the effect of nitrogen cycling is not included (p. 5, lines 16-17), there should be some discussion of that in the Discussion as to how that may change things. 8. Figure 3: I suggest changing the green colors so they grade from darker to lighter with height, rather than the lightest one in the middle. 9. Need reference for the WATCH data (p. 8, line 13) 10. I would like to see more explanation of how disturbance is applied (p. 9, line 6) – although the Porada reference is given, one or two sentences here would be helpful. 11. Table A2: list units as %. 12. Sensitivity to model parameters (end of Methods) is based on those that affect total biocrust cover, relative cover, temperature-dependence of NO and

HONO emissions, but what about those that affect water saturation? There ought to be some test of a key parameter here, as that affects NO and HONO emissions. 13. P. 11, line 2 – why are chlorolichen-dominated crusts larger fractions of the Sahara and Arabian deserts – does not look that way from the figure? 14. From Figure 4e, I would conclude the light cyanobacteria are the outlier under low precipitation, rather than lumping the dark cyanobacteria together with them, which is done throughout the text. 15. Appendix figures are out of order – A6 is discussed first, then A2-A5, then A1, and finally A7. Please put these in order. Also, from Figure 9 and Figure A2-A5 I am unclear which site (1 - 4) is being referred to, so there should be some way of distinguishing between the four sites. Furthermore, why are the results from only one of these sites shown in the main text? 16. In section 4.1, there should be better attribution of these explanations as to what actually occurs in the model, to distinguish from general arguments. For example, why is there a competitive advantage due to height given that shading is not an issue? How does the discussion of moss vs lichen in dry/wet conditions pertain to the model design? Also, p. 20 line 4 – not sure how this follows previous sentence. 17. P. 20, line 34 – it is ok for process-based models to determine parameters based on optimization from field results – do not need to just use values based on the literature. 18. P. 21, line 5: Are the results not sensitive to climate or is it just that at this one site the match is good? 19. P. 21, line 23: Would changing the albedo throw off the other seasons, or is it a change that only occurs in the warm season? 20. P. 22, line 23: The conclusion of correct wetting events is from only the one location in South Africa, at least as presented in this paper. There really needs to be a more broad-scale analysis globally of this in order to make this conclusion. Is that possible to do – as that is really key to getting the emissions correct? 21. I would consider putting the 1.1 (which is listed as 1.04 in the abstract?) and 0.6 Tg/yr into the Conclusions and adding the 20% in the Abstract.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-324/bg-2018-324-RC2-

supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-324, 2018.