

Interactive comment on “Microclimatic and ecophysiological conditions experienced by epiphytic bryophytes in an Amazonian rain forest” by Nina Löbs et al.

Anonymous Referee #3

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Dear Editor, dear authors

I have read with interest the manuscript entitled “Microclimatic and ecophysiological conditions experienced by epiphytic bryophytes in an Amazonian rain forest” by Löbs et al. submitted to Biogeosciences. Please find my comments related to it below:

I appreciate a strong point in this manuscript, that is to contribute to raise the data availability regarding cryptogamic covers functional performance in tropical regions, and going further, the lack of data available in Central and South America. It seems that almost all the literature regarding this issue has been focused in Polar Regions some years ago and in drylands at the present. I also appreciate the novelty and the

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effort made to provide microclimatic data sets at those heights at the tree trunks. If we want to understand properly the relevance of these organisms in global cycles and their response under environmental changes a huge and very different biome as the tropics can not be ignored. I think that authors do a complete revision of the literature available and try to contribute from there with their data. Mosses dominate cryptogamic covers in tropical regions in biodiversity, so the target organisms in the study seems to be quite correct

But, at the same time, my opinion is that this lack of data availability in the region is an intrinsic weakness of the manuscript. My point here is that the manuscript is based in a double assumption rather than in strictly measured data sets. The first assumption would be the water content of the bryophytes through conductivity sensors. I appreciate the effort made by the authors calibrating this methodology in the lab and this experimental testing gives higher credibility to the measurements. But then we see the big second assumption, that is to extrapolate data taken from the literature to understand the functional performance of the bryophytes in the altitudinal gradient. I think that it is likely that possible inaccuracies could arise in this sense. Data available in the literature is little, so, it must be difficult to find similar experimental designs that could help providing reliable extrapolations. I am not talking about finding same species with data available in the literature, but it would be interesting, in order to trust the ecophysiological data provided, to have data from a similar habitat following at least the light adaptation patterns of the species included in this work.

As I suppose that these data sets are very difficult to get, but I think that this manuscript is interesting and useful to the scientific community, I would make a proposal to the authors: What about to include in your manuscript a few gas exchange checkpoints in the lab including relevant species inside the gradient. For example, one representative species in the understory and another one at the closer point of the canopy could serve as cardinal points to calibrate authors' predictions about net photosynthesis availability, time and amount of respiration and possible C losses, light cardinal points, adaptation

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strategies. This would improve the discussion substantially from my point of view

I am not asking for a complete gas exchange profile of the species included in the study because I know how time consuming this technique is, just a few replicated checkpoints in the lab to see how close predictions are from reality. If they were far from each other, the real gas exchange parameters measured could work as a more reliable source of predictions than a very likely imprecise literature for the aim targeted. I would welcome further assumptions at this point, but based in some real measured values (I said in the lab because conditions are easier to control, but some field gas exchange data sound good for me also). I think that this could improve the manuscript and put it as a reference text in tropical epiphytic bryophytes functional performance due to the low amount of literature available

Some minor points also to comment:

INTRO:

-Page 3, Ls 20-25: I would focus in bryophytes functional properties rather than in general physiological features of cryptogamic covers because only bryophytes are included in the experimental design

METHODOLOGY:

-Section 2.5. Could you please explain in more detail why some meteorological parameters are measured at 26m and light is measured at 75m?

-Section 2.6. I would establish the possible ranges for each ecophysiological parameter analyzed focusing more in tropical epiphytic bryophytes functional performance

RESULTS:

-Section 3.1. 2 consecutive years of microclimatic data availability is a good and interesting output provided by authors

-All sections in general. I see that the headings do not correspond too much with what

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is written at each of the sections. Authors mix concepts in the same paragraphs such as microclimate, mesoclimate, water content, seasonal and daily analyses. ...Would it be possible to rethink the headings of the sections and write text more focused to each of the headings?

-Page 10 L9, I think that authors missed a word after "35%", maybe "lower"?

-How did authors compared climate statistically between years/seasons? Did you use a monthly basis? Daily basis?

-Page 10 Ls 25-26. If I understood ok, the idea is that the microclimatic T value at the moss level was higher than ambient T, and that this is a frequent pattern. What about the shading effect of the tree canopy over microclimatic T?

-Fig 1, legend. I would say estimated water content of the bryophytes rather than "ecophysiological conditions"

DISCUSSION:

-Page 14 Ls 22-24. I think that these patterns observed reinforces that measuring some gas exchange control points might be useful

-Page 17 Ls 19-23. I do not understand this point properly

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