

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

## **Anonymous Referee #2**

Received and published: 15 February 2019

The authors provide a description of bryophyte occurrence and microclimate in a tropical forest canopy. These data are scarce and therefore crucial for a variety of applications that the authors list at various times in the manuscript. However, at present, the manuscript suffers from several issues. First amongst these are poor organization and a general lack of coherence. Facts about bryophytes (such as they are poikilohydric) are repeated often. No clear hypotheses or research questions are outlined. The introduction tells us that bryophytes are ‘cool’ and important to study but doesn’t do a good job of setting up the study itself. Until the end of the methods section, I didn’t realize that gas exchange measurements were not performed (something that is mentioned in abstract- If gas exchange in epiphytes is essential, why did the authors not make these

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measurements?).

While I am quite satisfied by the measurement protocols and methodology (and that the epiphyte wetness-drying data are novel and important) the study ends up being merely a data reporting exercise with conclusions that often seem unsubstantiated by the data that are presented. Other times conclusions are trivial. For instance, Pg 18, lines 18-23 it is suggested that it is dark in the understory and therefore photosynthesis is light limited. I do not think that today one needs to go to the Amazon to make this conclusion, as this has been known for decades (for e.g. read classic reviews by Chazdon and Fetcher, 1984; Mooney et al., 1984). I seem facetious here, but the authors could use the same data to build upon these earlier findings, and find some nuance and/or insights. What is the knowledge gap that you are trying to fill with your measurements?

I want to be clear that I do not think that this work is unpublishable, rather a considerable amount of work needs to be done, especially in the writing, to ensure that it is. The advantage of the study is that the authors have collected a vast amount of important data, and there are several questions that can be formulated and answered. For instance, Fig S.5 is very interesting, and one could speculate about the significance of Tair -TCryptogram relationship in different parts of the canopy, and its significance to physiology. Another question could be the importance of light flecks, since you have carefully measured PPFd within the canopy. Fog is also measured but these data seem largely ignored (I wonder if you had leaf wetness sensors, those data could bolster the study tremendously). I would recommend the corresponding author to read some of the classic literature on epiphyte distribution and abundance (e.g. Benzing, 1984). With some more data exploration and thought I think this could be a very significant contribution. In its current form however, the manuscript reads like an early draft of a thesis or a dissertation chapter, and I do not see it fit for publication in Biogeosciences, or a journal of similar repute.

Finally, authors should provide data access via a link with a doi to a data repository. I

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wonder If this is required of papers that are submitted to Biogeosciences.

Specific/Minor comments below:

The abstract is a bit long with too many technical or field specific terms that should be introduced (in the introduction), since it makes it difficult to comprehend for the general reader. An example is “While the monthly average mesoclimatic ambient light intensities above the canopy revealed only minor variations. . .” This is a well written but complicated sentence for the average reader. Please simplify.

Line 12: 1.5 m relative to what (i.e, please include canopy height). For the abstract something general, like ‘near-surface’ or ‘in the understory’ is more appropriate.

Line 13: instead of saying “low, exceeding less than 8%. . .” you could say low, remaining below 5 umol. . . more than 92% of the time.

Lines 18-19: Dark respiration should occur independent of light (and unless temperatures are very low, which seems unlikely at your site). The references to photosynthesis and respiration are repeatedly incorrect. Photosynthesis and respiration are co-occurring biological processes (in the light), and therefore one may dominate over the other.

Introduction

The first paragraph is a well written introduction to tropical forests, but has little do with the study. Either you should reframe it in the context of epiphytes or omit. Overall, the introduction does not set up the study satisfactorily.

Pg 3 Line 13: ‘By’ not ‘In’ 2013.

Pg 4 Line 5: Update references to carbonyl sulfide: (Gimeno et al., 2017; Rastogi et al., 2018).

Methods

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Sec. 2.1. A greater description of the site is required. I would recommended starting with site characteristic and then describe the tower and measurements, not the other way around.

Pg 4 line 13: Remove “The”.

Pg 5 line 2: “were measured” not “are being measured”

Line 5: this seems important for your study and you should describe why sensors were placed where they were placed, in addition to citing the Mota de Oliveira (2013) study.

Pg 6: line 30: Why 60 C? Is this a temperature that these communities experience?

Sec 2.5. Again, some information (a figure ideally) describing the vertical profile of the forest is necessary. That helps put the various sensor heights in perspective.

Lines 15-21: Why were rainfall values gap-filled? Also, isn't the sensor at 1.5 m the least well placed to record rain event. For instance, a small rain event might not even be recorded at 1.5m, as interception must be high in a high LAI forest. Alternatively, there could be time lags between when rainfall occurs at the canopy top and when it is measured by the 1.5 m ht rain gauge.

Line 32- Pg 9, line 1: Rephrase sentence. Light intensity regulates the balance i.e. the Net exchange between photosynthesis and respiration.

Line 14: Respiration takes place at all light levels. IT IS NOT A LIGHT DEPENDANT PROCESS (there can be significant inhibition of respiration at high light levels). Please check this basic tenet of biology.

Line 21: Based on literature, not literature data. Also, please cite the relevant literature.

Results: Overall, I do not have issues with the content per se, but as I have stated before this section needs to be majorly revised/expanded. Some minor comments below.

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Pg 10

Line 8: Micromet did not depend on years but varied amongst years.

Line 18: please define mesoclimate the first time you use this term.

Pg 11

Line 30-31: What does this mean? Please elaborate.

Pg 12 Sec 3.1.3 header: remove parenthesis

Line 13: which 'organisms. Please specify.

Sec. 3.2. The scope of your inference is limited since you do not have replicates on different trees. I do not see this as limitation, but somewhere (probably in the discussion) you need to talk about heterogeneity in the microclimatic environment.

Lines 13-14. This has been mentioned previously (in Sec. 3.1.).

Lines 25 to Pg 14 line 3: This is a well written paragraph but belongs in the discussion.

Discussion: I am going to stop commenting here, since I think this section regurgitates a lot of information that is already presented. It is well-written, but I think that based on my previous comments, I anticipate this section to be revised extensively.

Tables:

Table 1: Why are these annual means presented? Why are light levels higher at 23 m than at 18m (again discuss heterogeneity)? I still don't have a good grasp of the canopy structure. I do not understand the sensor placement with respect to the canopy structure. Is the 23 m sensor above the canopy top (~ 21m)? Probably not, since light levels seem too low. Also, there seems to be some confusion between relative humidity and water content.

Table 2. There are no significant differences between seasons for some variables (for e.g., temperature), even though this is alluded to in the results (Pg. 11, line 24).

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Figures: Generally, the figures need to be clearer, and larger, since you have several subplots.

Fig 2. PAR and Temperature at different heights are very hard to see. Either summarize differently, or show a mean in this figure and direct to a figure in the supplemental with data from all heights.

Figure 3. This also has too many sub-panels crammed in one figure. In the caption, why do you say ecophysiological, micrometeorological and ambient parameters (the same is actually true of Fig. 1 as well). Which ones are which? Why are they called parameters? What are you trying to parametrize? I make a point about this, because this is one of several instances where words are not chosen carefully. Was humidity not measured at all heights?

Figure 4. The histograms are informative but the information provided in the various shaded regions is extremely hard to follow. In the end, I do not understand what the authors are trying to convey. Why is the y-axis broken in the histograms in the left most panel?

References mentioned in the review:

Benzing, D. H.: Epiphytic vegetation: A profile and suggestions for future inquiries, in *Physiological ecology of plants of the wet tropics*, pp. 155–171, Springer., 1984.

Chazdon, R. L. and Fetcher, N.: Light environments of tropical forests, in *Physiological ecology of plants of the wet tropics*, pp. 27–36, Springer., 1984.

Gimeno, T. E., Ogée, J., Royles, J., Gibon, Y., West, J. B., Burrett, R., Jones, S. P., Sauze, J., Wohl, S., Benard, C., Genty, B. and Wingate, L.: Bryophyte gas-exchange dynamics along varying hydration status reveal a significant carbonyl sulphide (COS) sink in the dark and COS source in the light, *New Phytol.*, 215(3), 965–976, doi:10.1111/nph.14584, 2017.

Mooney, H. A., Field, C. and Vazquez-Yanes, C.: Photosynthetic characteristics of wet

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tropical forest plants, in *Physiological ecology of plants of the wet tropics*, pp. 113–128, Springer., 1984.

Rastogi, B., Berkelhammer, M., Wharton, S., Whelan, M. E., Itter, M. S., Leen, J. B., Gupta, M. X., Noone, D. and Still, C. J.: Large uptake of atmospheric OCS observed at a moist old growth forest: Controls and implications for carbon cycle applications, *J. Geophys. Res. Biogeosciences*, 0(ja), doi:10.1029/2018JG004430, 2018.

Referee rubric evaluation:

1. Does the paper address relevant scientific questions within the scope of BG?: Not currently, but with significant revision.
2. Does the paper present novel concepts, ideas, tools, or data? Data yes, ideas and tools or concepts: no.
3. Are substantial conclusions reached? No.
4. Are the scientific methods and assumptions valid and clearly outlined? Yes.
5. Are the results sufficient to support the interpretations and conclusions? No.
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes.
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Literature needs to be expanded.
8. Does the title clearly reflect the contents of the paper? Yes.
9. Does the abstract provide a concise and complete summary? No.
10. Is the overall presentation well-structured and clear? No.
11. Is the language fluent and precise? No.
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined

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and used? Yes.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes (see detailed comments).

14. Are the number and quality of references appropriate? No.

15. Is the amount and quality of supplementary material appropriate? Yes.

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-521>, 2019.

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