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

Maaike Bader (Referee)

maaike.bader@uni-marburg.de

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1. Does the paper address relevant scientific questions within the scope of BG? yes

2. Does the paper present novel concepts, ideas, tools, or data? Yes (data)

3. Are substantial conclusions reached? No, at least no supported ones.

4. Are the scientific methods and assumptions valid and clearly outlined? They may be valid but should be outlined better (uncertainty evaluation, parameter selection, statistical methods)

5. Are the results sufficient to support the interpretations and conclusions? No, conclusions needs to be downtuned.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Of the experiments yes, of the calculations (statistics) no.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Literature citations can be improved.

8. Does the title clearly reflect the contents of the paper? Almost. . .

9. Does the abstract provide a concise and complete summary? Yes.

10. Is the overall presentation well structured and clear? Yes.

11. Is the language fluent and precise? Fluent yes, could be more precise.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes, but some are unnecessary.

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

14. Are the number and quality of references appropriate? Yes, but some need to be cited differently in the text.

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

Dear authors,

The manuscript "Microclimatic and ecophysiological conditions experienced by epiphytic bryophytes in an Amazonian rain forest" presents interesting data about the microclimate experienced by epiphytic bryophytes in a tropical rainforest, as well as unique measurements of the time these organisms stay wet. Such data is indeed very valuable for understanding the distribution and ecophysiological behaviour of such mosses and liverworts. The data are well-presented graphically at different time scales, showing seasonal and diel patterns. There are some issues about the presentation of the interpretation that need addressing though, as explained below.

General issues:

It is clear that is a great effort to measure such data in a rain forest environment and the difficulty of canopy access. Because of this, and because of the absence of comparable data, the lack of replication (all samples were located close together on one stem or branch section per height on the tree) can be ′forgiven′, but it should be mentioned and evaluated in the text!

I am also very aware of the almost complete lack of basic ecophysiological data on gas exchange in tropical lowland bryophytes, data being available for only 6 species, presented in Wagner et al 2013. However, I do not think that this justifies using data from tropical montane forest species, especially not for temperature responses, which differ along elevation (as shown in the cited paper by Wagner et al), but also not for water content responses, because montane species experience very different water regimes and are likely to employ different strategies concerning the preservation and use of their water contents – that is to say, the ′community weighted mean′ of the strategies is likely to be different. I do think that it is a valuable exercise to estimate activity times for net photosynthesis and net respiration, but I think the lack of physiological data to base this estimation on needs to be dealt with differently. Some of the cited parameters (which are from montane species) are so unlikely (like a lower activity level for water content of 225%...) or uncertain (note that in Wagner et al it is explicitly mentioned that the absolute carbon exchange values should be treated with caution because of uncertainty in the absolute carbon exchange rates measured. This is not a problem for the optimum ranges (T and WC), but it is a problem for the compensation points, to which your calculation is highly sensitive. I would recommend to use only the lowland data and to use these data more loosely, using them combined with your common sense to estimate (or select) likely parameter values and presenting only theoretical calculations like " if we assume that the LCP is 6 umol/m2/s, the total A and Rd times

would be x and x% of the time, whereas a LCP of 1 umol/m2/s would aloe net A x% of the time". This is not fundamentally different from your current presentation, but you could avoid having to present estimations of 0-100%, which are not very helpful, and it would acknowledge the fact that gas exchange data for lowland species are simply not sufficiently available to really allow the type of estimates you would like to make at this point.

- Considering my previous point this one may be obsolete now, but it is not clear how the parameters in table 3 and S2 or those presented in L17-18 P9 were selected from Wagner et al 2013. Also, a ′water content compensation point′ was not presented in Wagner et al although the paper is cited for it.

- Also, a lot of the statements about ′tropical bryophytes′ are supported by literature from montane forests, and a lot of the statements about ′epiphytic cryptogams′ are based on literature on lichens. This is not wrong but it is a bit deceiving. There would be nothing wrong with emphasizing, not only at the end of the discussion but right up front, that very little data is available for tropical lowland bryophytes and that therefore you need to rely on quite a bit of rough guessing and extrapolation of results from other areas and other organisms. As long as you make clear what your limitations are, they can be dealt with.

- So: make clear what literature is about lichens and what is about mosses – although these organisms have ecophysiological similarities, they are not the same in all respects! For example, enthanolic fermentation and bioaerosols have been observed for lichens but not for bryophytes, or am I wrong?

- And: be very careful, and be explicit about it, with using parameters and process knowledge based on montane forests and on lichens.

- Water content can hardly be called ′ecophysiological conditions′, I would recommend removing this term from the title. To make sure that the innovative data on water content are in the title, you could consider changing it to "Microclimatic conditions and water-content fluctuations experienced by epiphytic bryophytes in an Amazonian rain forest"

- The statement "Our data suggest that water contents are decisive for overall physiological activity, and light intensities determine whether net photosynthesis or dark respiration occurs, whereas temperature variations are only of minor relevance in this environment." In the abstract, and the statement that ′water content has turned out to be key′ is not justified by your results. It is probably the case, but this is not suggested by your data – it could not be and was not addressed in your study, as realistic data about gas exchange is missing.

- There is a lot of information in the methods section that is superfluous or irrelevant, whereas other information is missing. Superfluous/irrelevant: P4 L 24-26, 29-32; P5 L13-15; Equations 5-8; P6 L20 brand name of styrodur.

- There is basically no information about the statistical analyses other than in what software they were performed. . . Please explain what was tested, what were your units of replications, etc.

- I am a bit afraid that you have used days as replications to compare climatic variables between years – is 26.6° really different from 26.4 °C, or even 25.8° is different from 25.8° (Table 1)?? With enough (pseudo)replication any tiny difference can become ′significant′, but that does not make it real. . .

- Please present your experimental design (what species, what positions, justification for the pseudoreplication), preferably early in the methods section.

- It was not clear whether you used the 5-minute resolution data for calculating the times for A and Rd, or whether you only used the half-hour smoothed data. The smoothed data are fine for studying seasonal differences, but for the activity times and for quantifying the frequency of sun flecks (which would be interesting to do!) I would recommend using the 5-minute data. You mention that the conductivity showed ′short-time oscillations′ - could these be explained physically? Were they regular fluctuations or

just general instability?

- Limitations should not only be acknowledged for the availability of gas-exchange parameters, but also, and early in the manuscript, for the measurements themselves. In particular, the quality of the WC calibration curves could be a problem. The calibration graphs show that there is indeed great variation between samples and between measurements, and that the models do not reflect the water contents very well even for the calibration data. As an example for the variability, the curves show that a conductivity of 800 mV (why is conductivity expressed in mV?? Should this not be in Ohm?) in Symbiezidium could be caused by a water content anywhere between 300 and 1700 %. What is the effect of this uncertainty on your results? For Octoblepharum the model underestimates the WC over much of the range (can this explain the low WC at 8 m?). For Sematophyllum the maximum conductivity measured in the field greatly surpasses the maximum values measured during calibration, which will, by the looks of it, results in a very high estimated water content even with the exponential correction. Why are these models not drawn for the whole range of measured conductivities? For example, the quadratic function for Leucobryum would mean that a very high conductivity, like the 1000 observed in the field, would indicate a lower WC than intermediate values. If you do not draw the whole curve, this potential artifact cannot be evaluated well.

- Also, the observation that water saturation was never reached at the 3 higher levels seems to suggest that something was wrong either with your WC measurements or the literature parameters used. . . BUT, this statement (P13, L24) cannot be true based on your data, because Symbiezidium is present only in these three higher levels, and in the calibration curves you show that observed values go up to 1500% WC, which is well above the WSPs cited. . ..

- It was unclear to me what "upper three height levels the bryophyte taxa could not be securely determined. Thus, the bryophyte taxon with the highest abundance in the canopy communities, i.e., the liverwort Symbiezidium barbiflorum was used" means exactly. Did you install sensors only in this species, or did you do the calibration curve only for this species and then use if for all the different (unidentified) species sampled at the higher height levels? This should be made clearer. I could imagine that you installed sensors in other liverworts looking similar to Symbiezidium and then assumed that the relationship between electrical conductivity and water content should not be more different between species than within species, due to the similar life form. This seems a reasonable assumption, but should be made explicit, and in table S1b the species should not be named if you do not know the real name. Indicating if it was a moss or a liverwort, or the family it belongs to, would be useful though!

- The use of different species at the different heights is a problem that also needs to be discussed earlier and more prominently and included in the analysis. It reads all through the manuscript as though differences in water content between height zones were caused by microclimatic differences, but of course a Leucobryum (cushion moss with specialised water-holding cells) is going to have very different water content dynamics that a Symbiziedium (prostrate leafy liverwort), even under the same environmental conditions. This is also obvious from your own data in the calibration curves, the points for Leucobryum being much closer together, inciating that the drying was much slower than e.g. for Symbiezidium. For Octoblepharum the two (! Looks like they were only two though you write they were three) samples dried at quite different speeds, it looks like the slow sample was denser and thus had higher conductivity at similar water contents. At the moment, the whole manuscript reads a bit as though you consider all cryptogams are expected to respond more or less the same, but we know that there are big differences between species, in particular in terms of water-content dynamics as well as the responses to this water content. Although you do mention this briefly, I think it deserves a few more words at least.

- It would be really cool if you could detect a dew signal in the WC data, did you look for this? Mention this in the discussion to but the dew remarks into the context of your data.

- It would also be cool if you could detect relationship between cryptogam activity pat-

terns and measured trace gas emissions – this tall canopy site would be one of the few places in the world where the needed data might be available, assuming that trace gases above the canopy are also monitored?

- The literature cited needs to be revised! Only few bryophyte papers are cited and often they are not the correct ones (see below)! Some examples:

p. 3, lines 15-16: Zotz et al 1997 is cited a lot but refers to a montane forest, and not to nutrient cycling, as suggested on this occasion.

p.8, lines 30-31: ′at least in the environment of the central Amazon′ is followed by references out of which none are from the central Amazon, most are from cloud forest. . . (by the way, this sentence is more or less repeated on page 12, L 29-31)

p. 9, lines 5-6: ′For tropical species, values (of WCPl) in the range 5 between $\sim$ 30 and $\sim$ 225 % have been determined (Romero et al., 2006; Wagner et al., 2013; Zotz et al., 1997, 2003)′ Again, these references are all from montane species or do not mention WCPl values at all.

p. 3, lines 10 - 12: . . .."Thus, the bryophyte taxon with the highest abundance in the canopy communities, i.e., the liverwort Symbiezidium barbiflorum was used (Gradstein and Allen, 1992; Mota de Oliveira et al., 2009; Mota de Oliveira and ter Steege, 2015; Pardow et al., 2012; Romanski et al., 2011; Sporn et al., 2010)." Of the 6 references cited here, B. barbiflorum is only mentioned in Gradstein and Allen (1992), the other 5 references do not cite this species at all! (one of the papers cited, Sporn et al. 2010, even deals with Asia even though S. barbiflorum does not occur there, being restricted to America. . .). Interestingly, Gradstein and Allen (1992) state that S. barbiflorum is a characteristc shade epiphyte of forest understory communities, not canopy communities. Not-cited more recent publications on the habitat of S. barbiflorum, however, show that the species also occurs in the forest canopy (Gradstein et al. 2001, Gradstein 2006, Gradstein & Ilkiu-Borges 2009, Gehrig et al. 2013). These recent papers show that S. barbiflorum is actually an ecological generalist, occurring in understory communities as well as in canopy communities. None of these non-cited papers document highest abundance of the species in canopy communities. Thus, the sentence on p. 3, lines 10-12, is rather wrong.

p. 3, line 12-13: "In 2013, 800 species of mosses and liverworts ..,. . . have been reported for the Amazon region" (. . ... Mota de Oliveira & ter Steege 2013). The reference cited here is quite wrong, Mota de Oliveira & ter Steege did not provide this number at all, instead they took it from Gradstein et al. (2001; correctly cited by Mota de Oliveira & ter Steege) who calculated 800 species in the Amazon region in their book based on a full-scale analysis of the bryophyte flora of the Neotropics. Thus, the correct reference here is Gradstein et al. (2001) and not Mota de Oliveira & ter Steege.

Data availability: does this local database assure future data maintenance and retrieval? Please provide more details.

Detailed suggestions, including a few technical but also many conceptual points:

General: rather than ′mesoclimate′, ′above-canopy climate′ would be a more intuitive name for those measurements. P3 L 9: instead of ′these′ write ′such′ (this is an example of the confusing mix of literature and statements about cryptogam communities in general (often based on soil crusts..) and on tropical lowland epiphytes. P3 L 21: careful, not all bryophytes are desiccation tolerant, even if they are poikilohydric P4 L4-6 Add that most of this info is based on data from soil crusts and from temperate zones and that very little is known about biomass and functions of epiphytic cryptogam in tropical forests, especially in the lowlands. P4 L 8: seasonal variation in what? P5 L2: why ′ecophysiological′ water content? What other water content is there? P5 L3: use ′were′ rather than ′are being′, even if the measurements are continuing, because you are here presenting results of a specific period in the past. Same for P5 L 11: were taken (not have been taken) P5 L 5: instead of ′described by′ use ′used by′, because ′described′ suggests that these zones were the output of a study, but it was the sampling design. P5 L8: a cushion is a specific bryophyte life form, seeing your species

the samples probably were not cushions in most cases. . . You could use ′bryophyte samples′ P5 L 19: what do you mean with ′fluctuations′? P6 L17: are nutrient content and temperature species-specific? P7 L1: what is the sensor weight? P7 L12: rather that presenting the models, which are very standard (except maybe for the exponential correction; if you want you could show the models in the appendix), a discussion about uncertainty propagation would be fitting here. P8 L16: rainfall amounts would usually not be calculated by integration but by adding the rain amount (e.g. number of tipping events) per time period. . . L8 L26: explain ′UTC values′; and where are such times presented, and why not always use local tiem? P9 L4-5: this WCPl is not what you describe it to be (this would not be a compensation point), it is the point below which the WC is so low that photosynthesis cannot compensate respiration, respiration ceasing at lower WCs than photosynthesis. P9 L28: with ′we found′ you mean ′we assumed′? P10 L17: report the statistical results (test and test statistics)! This goes for all ′significant′ (or non-significant) results. P11 L1: ′The RH..′What RH? It is generally not always clear in the text what parameter you are talking about: daily means, monthly means, something else? P12 L25: word missing P13 L16-18: it would be relevant to mention whether such high temperatures were ever reached in wet bryophytes; I would expect that they would only occur while samples were dry. P13 L 27: I guess you mean the LOWER end of the WCPl range? P14 L6: you mean ′height′, not ′altitude′ here. P14 L6-7: ′The microclimatic conditions experienced by bryophytes along an altitudinal gradient at the ATTO site follow the meteorological characteristics to some extent′ - this needs some reference to time. . . P14 L15-17: mention in methods P14 L18: ‘ may have periodically shaded the organisms′: it seems to me that you can have observed whether this was the case or not: were any leaves situated close to these sensors? (Same for P16 L7-8) P14 L20: was PARavg not the monthly average? Do you mean the monthly averages of the daily patterns? P15 L9-15: is could indeed be expected and is not very exciting. Your contribution here should be discussing the differences in temperature fluctuations quantitatively. P15 L17-18: mention this reinstallation in the methods too. P15 L21-22: mention and discuss this earlier on. P15 L33-34: Is the canopy so open that the wind direction is noticed at 8 m height? Why did you choose the west side, I would expect you to select a side with goo moss cover. Interesting if this happened to be the west side if this side receives less moisture. Can you explain this? P16 L11: why does a light rain facilitate drying?? P16 L17: this has at best been estimated, and please specify what you mean by 4%: 4% of water input for bryophytes (or other epiphytes?), or just comprising (thus not ′providing′) 4% of total precipitation? P16 L22: the water holding capacity is not what you have been measuring... Otherwise, this sentence is very true: the high water contents may be due to the high water-holding capacities of these species. P17 L13-14: be careful with your wording: understorey species are probably more efficient at low light (lower LCP), but it would be weird if they had a higher potential photosynthesis. P17 L19-20: words missing P17 L22: It may be worth mentioning that Wagner et al 2013 concluded that, although respiration losses may be high, this in itself does not explain low bryophyte growth in tropical lowlands, because respiration rates are adapted or acclimatized to the prevailing temperature conditions: in mosses growing at higher elevations the respiration rates are higher at the same temperatures, but still epiphytic bryophyte biomass is much higher here. P18 L4: another example of a mismatch between cited literature and interpretation: you suggest that it is relevant that water contents in Zotz et al 1997 were measured during the same time of the year, but as this was a different region and a very different forest type, this temporal coincidence has no meaning whatsoever! P18 L13-14 ‘ whereas in the canopy, rain events, fog, and condensation seem to be equally important water sources for cryptogams.′ What do you base this conclusion on?? P18 L16: what does ′which′ refer to? The reference seems strange here. (Figure 2: the wet season data are shown twice, the dry season data are missing! A legend is also missing.) → already corrected by authors Figure S2: in what way are these integrals? Do you mean interpolations? Supplement: P4 L7: looks like 2 replicates for Octoblepharum

References: Gradstein, S.R., Churchill, S.P. & Salazar A., N. 2001. Guide to the Bryophytes of Tropical America. Memoirs New York Bot. Garden 86: 1-577. Grad-

stein, S.R. 2006. The lowland cloud forest of French Guiana – a liverwort hotspot. Cryptogamie, Bryol. 27: 141-152. Gradstein, S.R. & Ilkiu-Borges, A.-L. 2009. Guide to the Plants of Central French Guiana. Part IV. Liverworts and Hornworts. Memoirs of the New York Botanical Garden 76, 4: 1- 140, 83 plates. Gehrig-Downie C., Obregón A., Bendix J., Gradstein S.R. 2013. Diversity and vertical distribution of epiphytic liverworts in lowland rain forest and lowland cloud forest of French Guiana. Journal of Bryology 35: 243-254.

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