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? Yes, although the most important conclusion, realistically, is that not enough ecophysiolgical data about tropical bryophytes are available to draw strong conclusions.
4. Are the scientific methods and assumptions valid and clearly outlined? Mostly yes, but the water content results cannot be right and need to be recalculated.
5. Are the results sufficient to support the interpretations and conclusions? Mostly yes, the conclusions are more cautious now than in the first version. Some details still need to be presented more cautiously.
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 of water contents no.
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes.
8. Does the title clearly reflect the contents of the paper? Yes.
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? Mostly yes.
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.
15. Is the amount and quality of supplementary material appropriate? Yes.

The new version of your manuscript, now with the adjusted title “Microclimatic conditions and water content fluctuations experienced by epiphytic bryophytes in an Amazonian rain forest” has improved a lot, in particular in the more cautious discussion of the results. I now have only one major problem with the results, and that is in the very unlikely water content values.

I appreciate it that you tried out the method I suggested, and I still think that method makes sense, but the results are not very convincing as they are. It does not make any sense that the liverworts in the canopy should have a constantly high water content. Also, it does not make sense that the bryophytes in the understorey should never reach WCs above about 400% (Table S1, Fig S8 – even if this is for 30-min averages, during rain events the mosses could stay at their maximum capacity for half an hour), if their maximum WC is about 1500% for Leucobryum and 1000% for Sematophyllum (your data in Fig S3 in the previous version).

In any case, there must still be something wrong with the calculation. Perhaps it would help to not take the absolute min and max mV signal ever measured by the sensors, but something like the 5% and 95% Quartiles, to avoid using spurious signals. Supporting the assumption that this has happened is that it does not make sense that any of the mosses and liverworts should go down to values as low as 0 or 1% WC - this is what one would reach in a good drying oven, not at >70% RH… So the real minimum WC should be higher. Also, there may be a meaning in the fact that the maximum electrical conductivity measured in the field was much higher than those measured during

calibration for Leucobryum (Lm) and Sematophyllum (Ss) (Fig S3 of previous version). The result of this is that the new function used estimates much lower WC values than those estimated by the calibration curves. I agree that the calibration curves (not presented in this version of the paper as they were not used for the new WC calculations) were problematic due to the huge variability (especially for Symbiezidium, not so much for Lm). Still, they might be used to constrain the range of relationships between electrical conductivity and water contents that can be considered acceptable, and for Lm and Ss the current functions use would fall outside this range, with systematic underestimations of the water content. If the calibration curves have a meaning, I would expect electrical conductivity in the field to be usually between near-0 and 600 (Lm) or 300 (Ss). By plotting the histogram of the conductivity values it may be possible to identify a more realistic value corresponding to maximum moss wetness.

For Symbiezidium I suspect that the water content is, on the other hand, systematically over-estimated. It seems rather impossible to me that a canopy bryophyte would maintain a WC of around 100% all the time. It would be really good if the quantitative translation of sensor output to water content can be managed, because the data do show that there is a signal in the data, e.g. by the strong response of at least some of the sensors to rain events. I think there are some interesting patterns in the data, in particular the diel fluctuation of the WC in the upper canopy, apparently following RH fluctuations. It does make sense that this diel fluctuation is largest at the upper two heights. So it is mainly the determination of the absolute WC that is problematic, not so much the fluctuations.

There may be a way to deal with this though. Apart from trying not using the absolute minimum and maximum ever measured but the 5% / 95% quartiles, you may also need to use the 30-minute averages, so as not to use the lower end of the short-term fluctuations as a minimum and upper end of those fluctuations as a maximum. Judging from Figure S4 those fluctuations may be a problem especially at 23 m, which could explain the high mean values there if you use the 5-min data for the calibration. I could imagine that after rain, they may also become more pronounced at e.g. 1.5 m. I might have to take back the recommendation to use 5-min values for the estimation of activity times, at least for WC.

A second point: I wonder how interesting it is, in the context of predicting activity of the mosses, to put emphasis on the seasonal patterns in the mean values of the climatic variables. What could be more interesting is to analyse the bryophyte activity patterns separately for the seasons.

Some smaller points:

Even if you do not use the calibration any more in this version, please do describe the fact that variability was high in these experiments, so that the uncertainty of the WC values is expected to be very high. Discuss why the calibration curves could not be used, so why the current approach was chosen. Also, on the positive side, you can describe how single measurement series showed a more or less linear relationship between mV and WC (except at very high WC, where the measurement tended to became saturated and mV did not longer change), justifying your linear approach in calculating WC from mV.

It would be good to present the min and max WC values (from your calibration curves) to give readers more insight into the WC calculations.

The discussion could present a stronger line in the points being made. As it is, some paragraphs seem a bit lost, rather than incorporated into a story. The story could, for example, be centred on the activity patterns, presenting the microclimatic data in that context. At the moment, context is missing a bit for some sections.

Marks, R.A., B.D. Pike and D. Nicholas McLetchie, 2019. Water stress tolerance tracks environmental exposure and exhibits a fluctuating sexual dimorphism in a tropical liverwort. Oecologia. Available from https://www.ncbi.nlm.nih.gov/pubmed/31664577. DOI 10.1007/s00442-019-04538-2

Detailed suggestions:

Abstract (p2), L16 I would not call the diel fluctuations in WC ´frequent wetting and drying events´ (although my expectation would indeed be that they would go through more than in the understorey, but form your data this is not obvious), how about calling them diel fluctuations? It looks like for your samples, the lower ones actually lived through more wetting and drying events as they responded to rainfall more directly / consistently (would be nice to quantify ´consistently´..). In any case, the WC calculations need to be revised so this sentence may still change, although the fluctuations will probably be little affected by the recalculation of the absolute values.

P2 L7-8 why the quotation marks?

P2 L21: measurements of CO2 gas exchange would be necessary, but this study is not a starting point for such measurements. Maybe change to “supported by measurements of CO2 gas exchange”

P2 L30 I do not understand the ´equally´ here

P3 L13 Have been reported

P3 L17 Clarify what ´It´ is

P3 L25 The ´Thus´ does not fit well. You introduce ecosystem functions but present data on microclimate…

P3 L29 variations in climatic conditions

P4 L23 What exactly do you mean by the ´story structure´? Is this more than just the vertical structure?

P5 L1 Why ´Generally´?

P5 L10-11 not only….. and height.

P5 L21 were not installed

P5 L28-30 Did you check/correct for drifts in the measured light levels due to e.g. algal growth on the light sensors?

P6 L11 would the voltage not be proportional to the conductivity if the sensor pins did not have a fixed distance?

P6 L22-25 This calculation of the water content does not really need a formula or a citation. It is simply the water content expressed per dry mass.

P6 L27-30 Like explained above, I would try using not the absolute min and max but something like the 5% and 95% quartiles, or some other reasonable min and max based on a histogram of the mV signals.

P6 L5 remove 8

P6 L6 were considered

P6 L12-15 try using a parallel structure (why were some data collected, others measured and others assessed?

P6 L20 I am still not convinced the ´integration´ is what you mean here.

P8 L7 We have already shown that high respiration loss due to high temperatures are probably not so important, as respiration rates are adapted/acclimatized to the elevation at which bryophytes grow (Wagner et al 2013, Annals of Botany) This could already be acknowledged here. And also in P12 L3-4, and in P16 L12, rather than suggesting that there is still all reason to think that night T is important to then, surprise surprise, conclude that it is not..

P8 L15 WCs BELOW the WCP

P8 L23 if light intensity is above and temperature below the compensation point

P9 L16-19 I would suggest to make clear here already that these measurements may be strongly influenced by local canopy cover, thus not necessarily reflecting the conditions for that stratus of the forest in general, and should therefore be taken with caution.

P9 L24-26 why is this interesting?

P9 L27 similar patterns to what?

P10 L2 RH where?

P10 L3 I really do not belive this result (RH highest at 23 m)

P10 L8-9 Suggestion to not talk about this result as ´showing high WC´ but rather as ´showing high conductivity, but this could not be related to the WC because of reinstallation… Or if you have enough data you could adjust the function by taking a new max (95%) and min (5%) mV signal.

P10 L21 Temperatures showed (not reflected)

P10 L28 Any way of knowing whether the fog touched the canopy? That would

P11 L2-3 This is a very interesting observation. It would be great if you could show (calculate) how consistent this response is. The examples shown in Fig S7 are not necessarily convincing, given all the fluctuations in the WC…

P11 L10-11 these amplitudes are caused by fluctuations at different scales (rain events at 1.5 m, diel humidity fluctuations at 23-m, therefore I am not convinced that it is useful to compare them. Also, this sentence seems repeated in L 27-30.

P11 L21 what ´mean´ are your referring to?

P11 L9 make clear here whether ´reported time´ considers 24-h or only daytime

P12 L12 Instead of ´microclimatic temperatures´ I would use ´temperatures inside the moss stands´

P13 Because of the reasons described in L12-13 and L17-18, I would not present the differences in light levels between seasons (L6-8) in that context, though it is probably worth discussing the artefacts because of the use of these data for estimating activity patterns.

P13 L31-34 What is the interest in knowing the difference in mean temperature, which is physiologically meaningless..?

P14 L1 This sentence can be removed

P14 L5-8 Example of a ´lost´ paragraph with no clear function in the story

P14 L9 it is not the response that changes between seasons, but the conditions.

P15 L4 What is ´stepwise´ about this drying?

P15 L5-7 You know what the water holding capacity (WHC) of your species is, and you even have information about their drying speed (both from your calibration curves), so there is no need to speculate here. Unfortunately, I think the WHCs of your species do not explain the pattern at all, if anything, they would cause a reverse pattern, with Leucobryum staying moist longer than Symbiezidium…

P15 L16 I agree with this, but it could be elaborated upon a bit more, and I think you need to aim at making these values indeed approximate but no longer biased.

P15 L21-22 It is not correct to equal acclimation processes to intraspecific variation. Intraspecific variation can also be a result of adaptation, see e.g. Marks et al 2019. The references cited here also do not refer to acclimation.

P15 L24-25 What is the function of this sentence? It breaks up the flow of the story.

P16 L1-4 reacting rapidly and efficiently to light flecks is not at all the same as being efficient at low light levels, this sentence thus does not make much sense:

P16 first paragraph. The ´so what´ of this paragraph is unclear.

P16 L7-8 is photosynthesis not a metabolic process?

P16 L17 I do not agree that this has been shown at all, I think this sentence and those that follow will be removed after revising the WC method.

P16 L24-25 Really? What would be the mechanisms for this?

P17 L3 was “exceeded” during.. Except that it was not ;-)

P17 L13 I agree! Except from questioning the generality of the published compensation points, this would also be a good place to critically evaluate your microclimatic data!

P18 L1-2 revise after recalculations. And I would rise the diurnal variation in WC in the upper levels to the conclusion section. This could be very relevant, but this depends on the exact WC values and the exact WCPs, neither of which, unfortunately, we know…

P18 L4 … minor variation relative to the physiological tolerances of the mosses, as far as these are known, …

Tables

Table 3 It is not totally clear what is meant by ´are reached´. It looks like you mean ´exceeded´, which has the problem that, depending whether it is a lower or upper CP, the shown values can be the time net photosynthesis could be positive (LCP, WCP) or the time that it could not (TCP)… While for Topt it is not so clear.

Table3 : why a column for showing the ´conditions´ which are the same everywhere?

P29 L4 occur are listed

Figures

P30 L5 remove ´epiphytic´ or ´the´

Fig 2: it is hard to see all the information here, could it be printed larger?

Fig 3 match order in caption to order in graph.

Fig 3b: The green for WC at 18m is a different colour than the rest

Supplementary material

Fig S4 This figure is cool it shows nicely how the T at 23m is most variable and mostly higher than at lower heights. The model for the 23-m data does not seem to fit well, though this is hard to see well because of the superposition of the points… Maybe also provide some panels with the data and models per height separately?

& Digits in R2…, Above canopy AT 26 m

Table S1 The 0 and 1% minimum WC values are probably just values due to some unexplained fluctuations in the sensors, so I would not report these extremes but rather the 5% quartile or something like that. The same goes for the ´maximum´.

Table S3 Explain what low and high mean

Table S7 Is showing PARmin really necessary?

References

Marks, R.A., B.D. Pike and D. Nicholas McLetchie, 2019. Water stress tolerance tracks environmental exposure and exhibits a fluctuating sexual dimorphism in a tropical liverwort. Oecologia. Available from https://www.ncbi.nlm.nih.gov/pubmed/31664577. DOI 10.1007/s00442-019-04538-2