Response to referee comments and suggestions on bg-2018-521 by N. Löbs et al.: "Microclimatic and ecophysiological conditions experienced by epiphytic bryophytes in an Amazonian rain forest"

Manuscript format description:
Black text shows the original referee comment, and blue text shows the response of the authors and the explicit changes in the text. The figure and table numbers refer to the revised manuscript.

Maaike Bader as Referee submitted the comments RC1 and RC3

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Maaike Bader RC1:

General Referee comment:

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 behavior 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 author response:

We would like to thank Maaike Bader for the very constructive review and the helpful comments, which helped us to identify the critical aspects and to improve our manuscript.

Referee comment 1:

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!

Author comment 1:

We fully agree that it would be preferable to install sensors on several different trees in order to have fully independent true replicates. However, as you stated correctly, it is a great effort to install and run microclimate measurements in such a rainforest environment. Thus, we installed several sensors at each height in order to cover the variability at least to some extent. We added some more information about the incomplete replications in the methods section to explain the limitation of the measurement setup.

Author changes in the text 1:

P 4 L22: " At each height level, six water content, two temperature, and two light sensors (except for 1.5 m with only one light sensor) were installed in different bryophyte species (Table S1a). Due to constraints in accessibility, all sensors had to be installed on one tree. Thus, we expect that the replicate sensors at each height are not be fully independent and thus the variability could only partly be covered by our current setup. "

Referee comment 2:

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 µmol/m2/s, the total A and Rd times would be x and x% of the time, whereas a LCP of 1 µmol/m2/s would allow 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.

Author comment 2:

Thank you very much for this helpful comment! It is correct that the data collected at montane rainforest sites are not suited for a comparison and thus we now refrain to the lowland forest data (location BT) given in Wagner et al. (2013).
For the light compensation point (LCP) we include another reference for bryophytes in lowland bamboo forests with values of 3-12 µmol $s^2 s^{-1}$ for the LCP (Lösch et al., 1994).
Furthermore, for the estimation of the duration of NP and DR we assume different values for the compensation points of the organisms at different height levels and we now also distinguish between mosses and liverworts, as we find that these organism groups respond differently. For the current manuscript we omitted the information on saturation points, as we found that the current data are not well-suited for an inclusion of these data.
Author changes in the text 2:
All tables, figures, and the values in the text have been adapted according to the revised calculations.

Referee comment 3:
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.
Author comment 3:
The data were extracted in the following way:
P9 L31-32: The lowest Topt for tropical bryophytes of 16.1°C was reported in Wagner et al. 2013 in Tab. 3, while the highest Topt for tropical bryophytes of 27.3°C was reported in the Supplement of Wagner et al. 2013. Unfortunately, we just now found out that the species listed in Tab 3 and the ones listed in the Supplement are not containing data for the same elevation levels.
As described above, we now only consider the BT site located at sea level (see Tab. 3 and Tab S3 below). Accordingly, we cite the optimum temperature range with 24°C limiting the lower and 27°C limiting the upper end of the range as reported in Tab. 3 of Wagner et al. (2013).
The lower and upper temperature compensation points "TCP" of 30.0 and 36 °C were reported in Wagner et al. 2013 Tab. 3 as the temperatures when "$T_{NP=DR}$" for the site BT at sea level
In Table S2, we found a typo where the higher TCP value was accidentally written as 33 instead of 36°C.
Data on the water compensation point were extracted from Fig. 1 in Wagner et al. 2013.
Author changes in the text 3:
The text and tables (Tab 3, Tab, S3) were adapted accordingly.

Referee comment 4:

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 eco-physiological 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.

Author comment 4:

Thank you for your comment; we are aware that lichens and bryophytes do not behave identically in all respects, although there are quite some similarities. Thus, in the revised version we stress whenever we use information on lichens. When comparing our results with those from other studies, we specify the rainforest habitat and organism group.

Referee comment 5:

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”

Author comment 5:

We agree with your comment on the ‘ecophysiological conditions’. Your recommended change of the title is a good solution and thus we adapted it.

Author changes in the text 5:

Title: “Microclimatic conditions and water content fluctuations experienced by epiphytic bryophytes in an Amazonian rain forest”

Referee comment 6:

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.

Author comment 6:

Thank you for your comment. Yes, we indeed do not have CO_2 gas exchange data in this study. Nevertheless, we think that already the microclimate data by themselves and the calculations on potential activity patterns support our statement that water contents are highly relevant whereas temperatures are of minor importance for physiological activity. Nevertheless, we try to clarify these issues in the revised version of the manuscript.

Author change in the text 6:

Changes were made in the abstract and the discussion sections to clarify these issues.

Referee comment 7:

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.

Author comment 7:

We agree to delete some information on the study site (P4 L 25-27), on the neighboring forest types (P4 L 30 - P5 L2), and the styrodur brand (P7 L2). On the other hand, we would like to keep the setup information on the logger enclosure (P5 L27-28), as we consider it as relevant for the reader. Regarding the equations 5-8, as the calibration was restructured, the Eq. 8 was now moved to the supplemental section.

Referee comment 8:

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.

Author comment 8:

We agree with the comment, there was not enough information regarding statistical analyses. This information will be provided in more detail in section 2.6 and in all tables presenting test results.

Author changes in the text 8:

P10 L11:" For all the statistical tests 5- or 30-minute averages of the data have been considered, as indicated in the respective results sections. Furthermore, for the average of height levels the data of the individual sensors were pooled. For the statistical tests of the diel maxima, minima, and amplitudes, the daily values of all sensors at the given height level were pooled for the season to be tested."

Referee comment 9:

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…

Author comment 9:

Thank you very much for that good advice. We thought about this test again and figured that one probably always obtains statistically significant differences if the 5-minute-values of two subsequent years are compared (as these are never the same in two subsequent years) and a comparison of the annual mean values does not make sense as we only have two years. Thus we decided to skip this test here."

Referee comment 10:

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

Author comment 10:

More details (that have been included in the Supplement material) were now included in the text, section "2.2 Microclimatic conditions within epiphytic habitat". Furthermore we now add a Figure S2 to the Supplement showing the distribution of all the sensors along the vertical gradient.

Author change in the text 10:

P4 L32: "Generally, the water content sensors have been placed in four different bryophyte types being heterogeneously distributed along the four height levels. At the height level of 1.5 m the water content sensors were installed in the moss species *Sematophyllum subsimplex* (5 sensors) and *Leucobryum martianum* (1 sensor), at 8 m in the species *Octoblepharum cocuiense* (3 sensors) and cf. *Symbiezidium barbiflorum* (3 sensors), at 18 and 23 m in the species cf. *Symbiezidium barbiflorum* (all 6 sensors; Fig. S2 and Fig. S3). The temperature sensors were installed in the same species at each height, and the light sensors were installed just next to the measured species. For the higher levels at 18 and 23 m, one species identification could not be verified (Tab. S1a, S1b).

Supplement: Figure S2 and Figure S3 (see below) were added to the supplement.

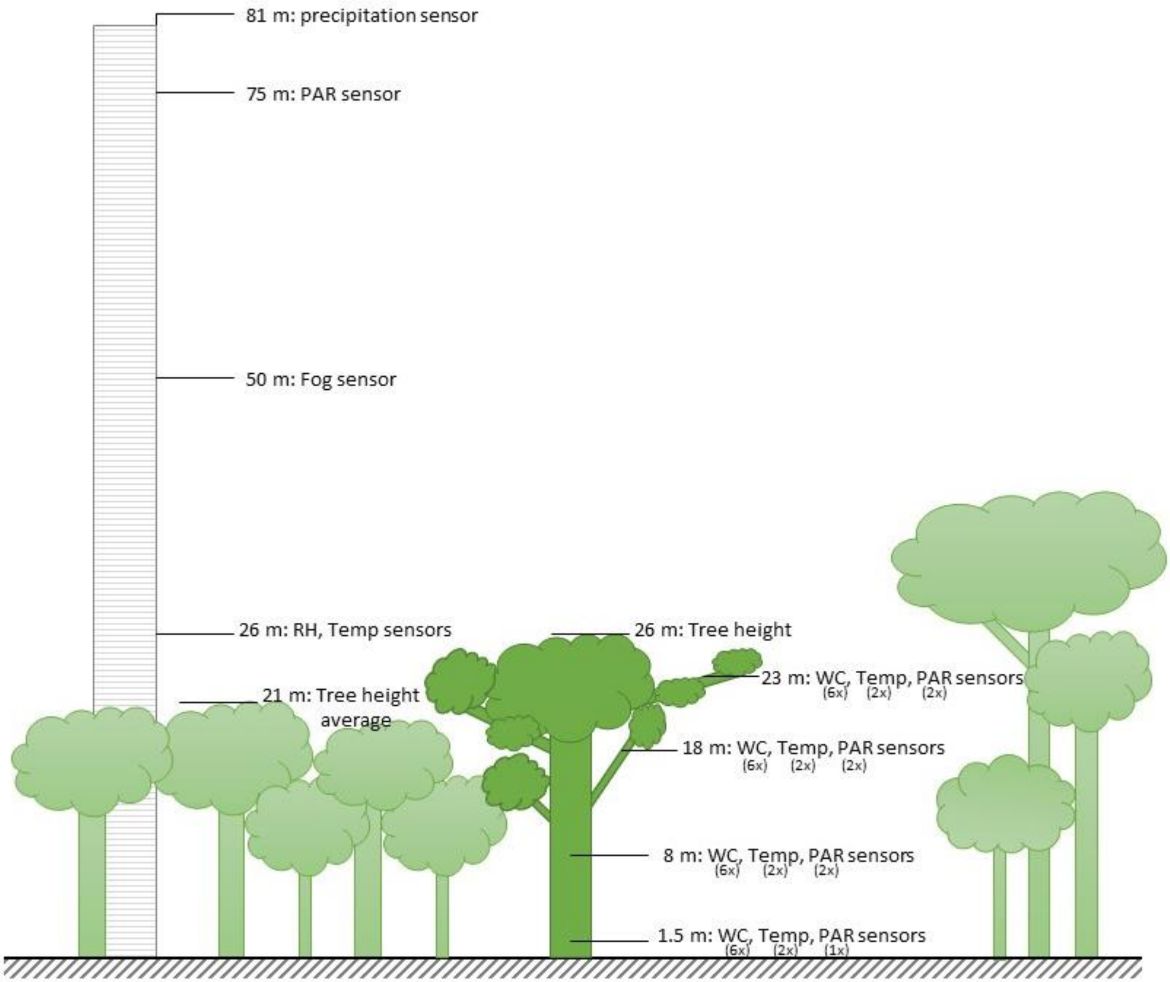

Figure S2: Schematic overview on the sensors installed at different height levels below, within, and above the canopy. The parameters water content (WC) and temperature (Temp) were measured within the bryophyte samples, the light sensors (PAR) were installed directly on top of the thalli. The average tree height of 21 m was determined for the plateau forest in general.

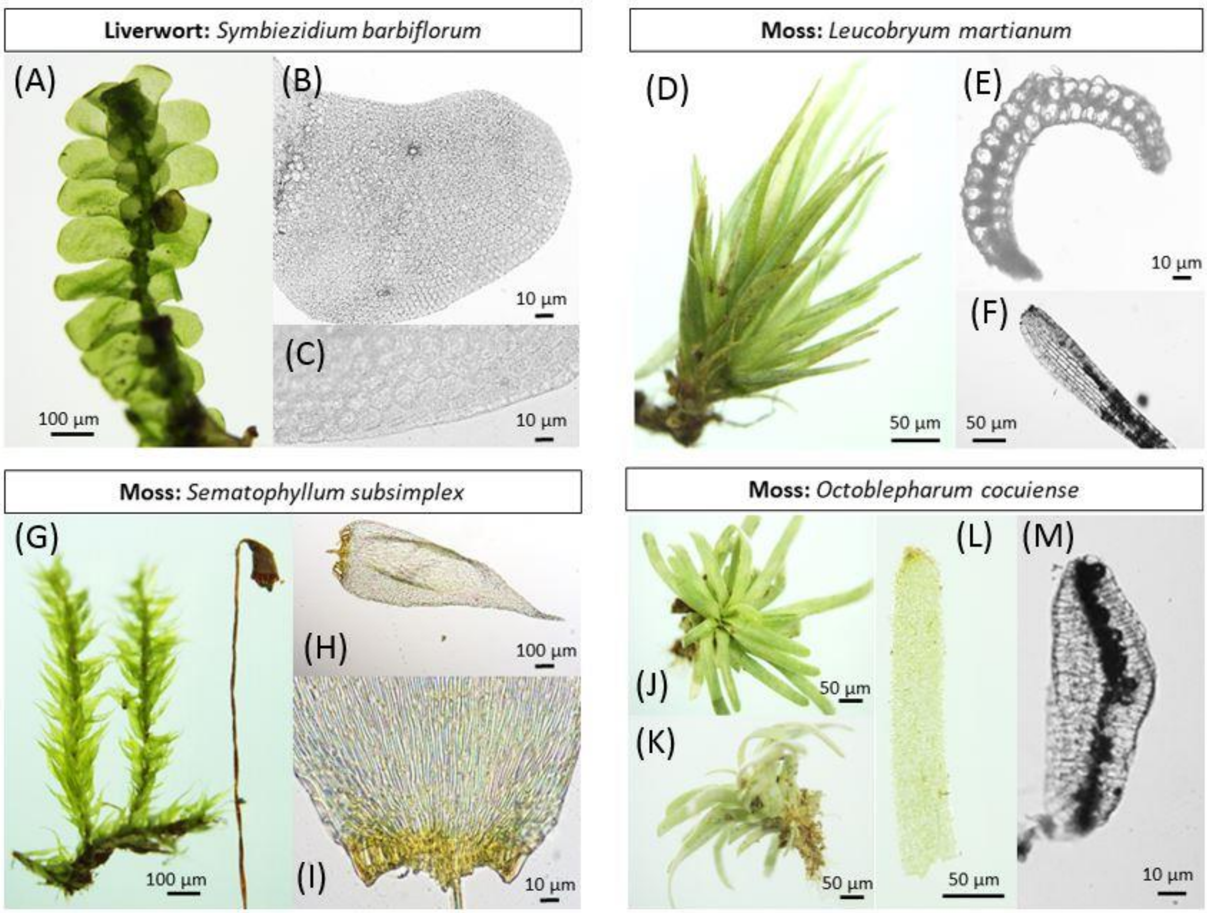

Figure S3: The four bryophyte species being used for installation of the sensors of the microclimate station. (A, D, G, J, K) overview, (B, H, L) leaf, (C, F I) cell form, and (E, M) cross section of a leaf.

<u>Referee comment 11:</u>

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.

<u>Author comment 11:</u>

For a calculation of A and Rd, the 5-minute data were used, as written in Table 3. We additionally provide this information in the methods section on P9 L28.

<u>Author change in the text 11:</u>

P9 L28:“Based on the literature values, we utilized the *5-minute data to calculate the* ranges of timespans when these cardinal points were passed. “

Referee comment 12:

You mention that the conductivity showed ‘short-time oscillations’ - could these be explained physically? Were they regular fluctuations or just general instability?

Author comment 12:

The oscillations of the sensors represent a general instability of the system, as the measured values oscillated around the actual values. Accordingly, the 5-minute data set was only used for an estimation of the physiological activity, as here the information on short-term events (as e.g. light flecks) is needed. For all other calculations, the 30-minute averages were used.

Author changes in the text 12:

P8 L5: “The measured electrical conductivity showed short-time oscillations, which could be removed with a 30-minute smoothing algorithm (Fig. S4). *Thus, for all calculations the 30-minute averages have been considered, except for the estimates of physiological activity*.”

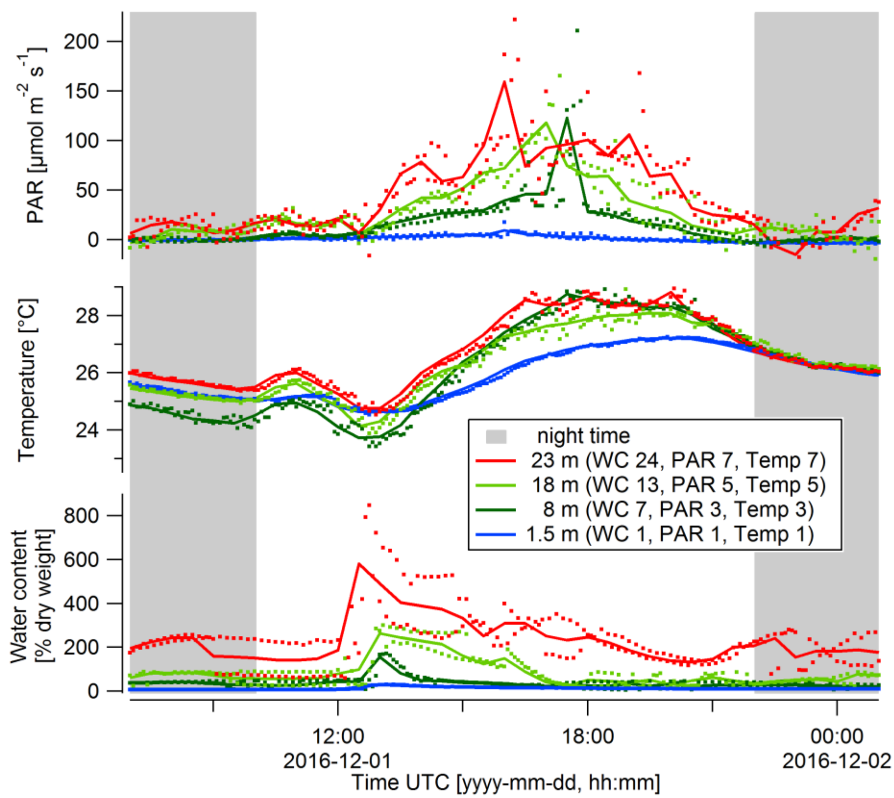

Figure S4: Comparison of 5-minute (dots) and 30-minute (lines) averages of exemplary sensors at each height level over a period of approx. one day in December 2016.

<u>Referee comment 13:</u>

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.

The calibrations were conducted for the samples starting with full water saturation and values were recorded every 60 seconds until weight constancy was reached, indicating that the sample were dry, as explained on P7 L4-6 ("In the beginning of each calibration the sample was wetted to the water holding capacity and during drying of the sample, the values of the balance and sensor were recorded at 60-second-intervals."). In the case of *Leucobryum* and *Sematophyllum* the values of conductivity measured in the laboratory unfortunately were not been higher at full water saturation.

We are aware of the fact, that this represents a problem and we thought a lot about this issue. These measurements illustrate that there are large differences between bryophyte specimens and even within one specimen we observed differences between measurements. This is probably caused by the fact, that the structure of the thalli and e.g. dead material collected within them strongly influences the calibration values. However, as measurements continued after this study, the measured specimens could not be removed from the tree for the lab calibrations. Thus, also based on your reviewer comment 3, we decided to test and use an <u>alternative approach</u> for the calibration of the water content. For this, the maximum and minimum values of electrical conductivity reached in the field were assumed to be assessed at the maximum and minimum water contents reached by the samples, whereas the amplitude of the water contents was determined based on the laboratory measurements. In the new approach we assume, that the maximum electrical conductivity in the field is achieved at the maximum water content, as determined in the laboratory. The measurements of the electrical conductivity in the laboratory are kept in the supplement to discuss their drawbacks.

Author change in the text 13:

P7 L21: “The calibration of the water content was performed, based on the maximum and minimum values of electrical conductivity reached in the field and the amplitude of the water contents reached in the laboratory measurements. We assume, that the samples at least once got fully wetted in the field and thus maximum electrical conductivity values achieved in the field were reached at the maximum water content, which has been determined in the laboratory (where the samples were wetted to full saturation). We also assume (and are quite sure), that the samples dried out at least once in the field, giving the minimum values of electrical conductivity. Accordingly, the water content (WC) was calculated as follows:

$$WC\ [\%\ DW] = \frac{(EC_i - EC_{min})}{(EC_{max} - EC_{\mathrm{min}})} * (WC_{max} - WC_{min}) \qquad \text{Eq. (4)}$$

with EC_i as electrical conductivity, EC_{min} as minimum electrical conductivity, EC_{max} as maximum electrical conductivity, WC_{max} as the maximum water content as determined in the laboratory, and WC_{min} as the minimal water content in the laboratory.

The measurements of the electrical conductivity in the laboratory will be kept in the supplement to evaluate the quality of the calibration curves, by the linear fitting of the individual measurements of the different samples resulting a R^2 ranging from 0.79 to 0.89 and a root mean square error (RMSE) ranging from 68 to 229 % DW (Eq. S1, Fig. S5, Tab. S1b). As a consequence, the calculated values for the WC should not be considered as absolute but as approximate values. Due to this uncertainty, no statistical tests have been performed on the bryophyte WC.”

Equation S1: The calculation of the root mean square error (RMSE)

$$RMSE = \sqrt{\frac{\sum (WCobs - WCpred)^2}{N}} \qquad \text{(Eq. S1)}$$

where WCobs is the observed water content, *WCpred* is the predicted water content, and *N* is the sample size.

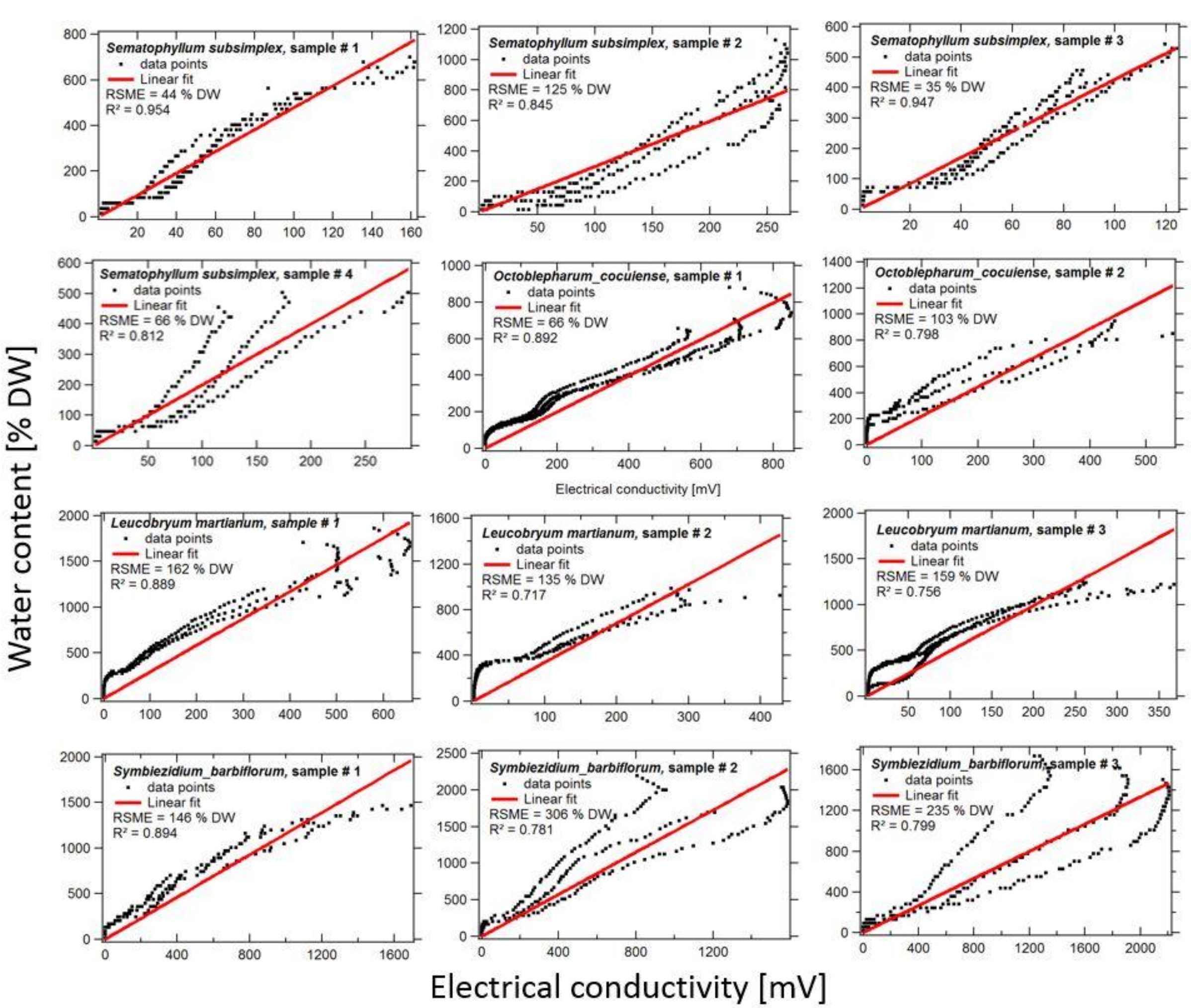

Figure S5: Calibration curves of water content sensors installed within different bryophyte species. The water content [% DW] is plotted against the electrical conductivity [mV] for the species Sematophyllum subsimplex (four replicates), Octoblepharum cocuiense (two replicates), Leucobryum martianum (tree replicates), and Symbiezidium barbiflorum (three replicates). Of each sample three subsequent wetting and drying cycles were measured. The dots show the measured data points, the lines represent the linear fit. For each fit the RMSE and the R^2 value is given in the graphic.

Table S1a: Height of installation and minimum and maximum values of the individual sensors of the microclimate station measuring water content, temperature, and light (PAR). For the water content sensors, also the bryophyte species are given. Based on 30-minute integrals.*) The species name cannot be verified without any doubts. The information of the calibration done with this species was considered for further data analysis, due to morphological similarity.

Water content	**Height [m]**	**WC [% DW] min**	**WC [% DW] max**	**Bryophyte species**
Sensor 01	1.5	0	598	*Sematophyllum subsimplex*
Sensor 02	1.5	0	598	*Sematophyllum subsimplex*
Sensor 03	1.5	0	598	*Sematophyllum subsimplex*
Sensor 04	1.5	0	1202	*Leucobryum martianum*
Sensor 05	1.5	0	598	*Sematophyllum subsimplex*
Sensor 06	1.5	0	598	*Sematophyllum subsimplex*
Sensor 07	8	0	1725	*Symbiezidium barbiflorum**
Sensor 08	8	0	834	*Octoblepharum cocuiense*
Sensor 09	8	0	834	*Octoblepharum cocuiense*
Sensor 10	8	0	834	*Octoblepharum cocuiense*
Sensor 11	8	0	1725	*Symbiezidium barbiflorum**
Sensor 12	8	0	1725	*Symbiezidium barbiflorum**
Sensor 13	18	0	1725	*Symbiezidium barbiflorum**
Sensor 14	18	0	1725	*Symbiezidium barbiflorum**
Sensor 15	18	0	1725	*Symbiezidium barbiflorum**
Sensor 16	18	0	1725	*Symbiezidium barbiflorum**
Sensor 17	18	0	1725	*Symbiezidium barbiflorum**
Sensor 18	18	0	1725	*Symbiezidium barbiflorum**
Sensor 19	23	0	1725	*Symbiezidium barbiflorum**
Sensor 20	23	--	--	*Symbiezidium barbiflorum**
Sensor 21	23	0	1725	*Symbiezidium barbiflorum**
Sensor 22	23	0	1725	*Symbiezidium barbiflorum**
Sensor 23	23	0	1725	*Symbiezidium barbiflorum**
Sensor 24	23	0	1725	*Symbiezidium barbiflorum**

Temperature	**Height [m]**	**Temperature [°C] min**	**Temperature [°C] max**
Sensor 01	1.5	21.1	36.3
Sensor 02	1.5	21.4	39.4
Sensor 03	8	21.6	34.7
Sensor 04	8	20.9	46.3
Sensor 05	18	20.3	38.0
Sensor 06	18	20.3	37.5
Sensor 07	23	20.8	41.2
Sensor 08	23	20.3	48.7

Light	**Height [m]**	**PAR [µmol m^{-2} s^{-1}] min**	**PAR [µmol m^{-2} s^{-1}] max**
Sensor 01	1.5	0	1546
Sensor 02	8	0	1461
Sensor 03	8	0	1502
Sensor 04	18	0	1386
Sensor 05	18	0	1080
Sensor 06	23	0	1326
Sensor 07	23	0	1351

Table S1b: Bryophyte species and calibration data of the water content sensors. Listed are the bryophyte species with their division (moss or liverwort), their height and height zone of installation, the root mean square error (RMSE), and the determination coefficient R^2. *) The species name cannot be verified without any doubts. The information of the calibration done with this species was considered for further data analysis, due to morphological similarity.

Bryophyte species	Division	Height [m]	Height zone	RMSE [% DW]	R^2
Sematophyllum subsimplex	Moss			44	0.95
Sematophyllum subsimplex	Moss			125	0.84
Sematophyllum subsimplex	Moss			35	0.95
Sematophyllum subsimplex	Moss			66	0.81
Octoblepharum cocuiense	Moss			66	0.89
Octoblepharum cocuiense	Moss			103	0.8
Leucobryum martianum	Moss			162	0.89
Leucobryum martianum	Moss			135	0.72
Leucobryum martianum	Moss			159	0.76
*Symbiezidium barbiflorum**	Liverwort			146	0.89
*Symbiezidium barbiflorum**	Liverwort			306	0.78
*Symbiezidium barbiflorum**	Liverwort			235	0.8
Sematophyllum subsimplex	**Moss**	**1.5**	**1**	**68**	**0.89**
Octoblepharum cocuiense	**Moss**	**8**	**2**	**85**	**0.84**
Leucobryum martianum	**Moss**	**1.5**	**1**	**152**	**0.79**
Symbiezidium barbiflorum*	**Liverwort**	**8, 18, 23**	**2, 3, 4**	**229**	**0.82**

Referee comment 14:

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…

Author comment 14:

You are right, according to the original calibration the WSP of 349 % WC was almost never reached in the canopy (at 23, 18, and 8 m), whereas it was reached during 22% of the time at 1.5 m height. As described above, the original calibration has been replaced and we also restrict the calculations to the compensation points, which are more relevant than the saturation points in the current context.

Referee comment 15:

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!

Author comment 15:

The sensors were installed in bryophytes morphologically similar to *Symbiezidium barbiflorum*. However, as the sensors were installed by a climber, it could not be completely reassured that always the same species was used. Nevertheless, we know that *Symbiezidium barbiflorum* was the most dominant species in the canopy of this tree, and from all the information we have for each sensor, the identification of these samples should be correct. This was corrected accordingly in the text.

Author change in the text 15:

P6 L24: "Thus, the bryophyte taxon with *a high abundance in the canopy of this tree*, i.e., the liverwort Symbiezidium sp. was *considered for all the further calculations in the course of the calibration, due to its morphological characteristics."*

Table S1a and Table S1b: *"*) The species name cannot be verified without any doubts. The information of the calibration done with this genus was considered for further data analysis, due to morphological similarity.*"

Referee comment 16:

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 specialized 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, indicating 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.

Author comment 16:

Indeed, we mixed up the replicate numbers in the Supplement Figure S5, and has been corrected. Regarding the behavior of different species during the drying process, this section was extended with more information.

Author change in the text 16:

P5 L8: "*As the morphology of different species affects their overall WC, different maximum WC and patterns of the drying process were observed. Whereas the individual samples of Octoblepharum c. reached quite similar maximum WC values (706-1109 % DW), the individual samples of cf. Symbiezidium* sp., *Leucobryum m., and Sematophyllum s. reached rather different values (1313-2383, 845-1862 and 428-1128 % DW; Fig. S5).*"

Supplement Figure S5: "The water content [% DW] is plotted against the electrical conductivity [mV] for the species *Sematophyllum subsimplex* (four replicates), *Octoblepharum cocuiense* (two replicates), *Leucobryum martianum* (three replicates), and cf. *Symbiezidium barbiflorum (three replicates*. Of each sample three subsequent wetting and drying cycles were measured."

Referee comment 17:

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.

Author comment 17:

This indeed is a relevant aspect, and we also considered if we could calculated these values. However, in order to calculate this we would need the temperature below the bark. Due to the lack of this information we cannot calculate dew formation.

Referee comment 18:

It would also be cool if you could detect relationship between cryptogam activity patterns 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?

Author comment 18:

Yes indeed, different trace gases are monitored at this study site and investigations on this are planned for the near future. This, however, is beyond the scope of the current manuscript.

Referee comment 19:

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 ~ 30 and ~ 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. 6, 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, *S. 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 characteristic 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. 6, 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.

Author comment 19:

Regarding P3 L 15-16, (Now P3 L16): reference was removed

Regarding P8 L30-31, (Now P10 L11-13): references exchanged; the repeated sentence was removed from P13 L24-26

Regarding P9 L5-6, (Now P 9 L23-25): The references of montane cloud forests (Romero et al. 2006, Zotz et al. 1997, Zotz et al. 2003) were removed. Only the information related to a research site at sea level in Wagner et al.

2013 was considered. The information for the WCP there is was extracted from figure 1. Perhaps we could obtain the exact value from the reviewer.

Regarding P6 L10-12, (Now P6 L23-28): Indeed, this sentence in its final version was wrong. Initially, this sentence intended to tell that in general in lowland rainforests liverworts are more abundant in the canopy than in the understory, which was observed by Pardow et al. 2012. However, due to internal revisions and changes in the text, the sense of this sentence changed, unfortunately resulting in a wrong statement. The sentence was changed to its initial meaning and the references for montane sites were removed.

Regarding P3 L12-13, (Now P3 L14-15): We corrected the reference according to your advice.

Author change in the text 19:

P9 L4: "The physiological activity of bryophytes – and of cryptogams in general – is primarily controlled by water and light, whereas temperature plays a secondary role – at least in the environment of the central Amazon (Lösch et al., 1994; Wagner et al., 2013)."

P9 L17: "For tropical species, values in the range between ~30 and *~80 %* have been determined (Wagner et al., 2013; Table S3)."

P6 L 24: "Thus, the bryophyte taxon with a high abundance in the canopy of this tree, i.e., the liverwort *Symbiezidium barbiflorum* was considered for all further calculations in the course of the calibration, due to its morphological similarity. Overall, for tropical lowland rain forests in Panama and French Guyana it was shown that liverworts have a higher abundance and higher biomass at the upper trunk and in the canopy than in the understory."

P3 L2: "By 2013, 800 species of mosses and liverworts...have been reported for the Amazon region (*Gradstein et al. 2001*)."

Referee comment 20:

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

Author comment 20:

Yes, this is a long term monitoring project and the database on the water content, temperature, and light conditions of epiphytes is uploaded to the ATTO data portal (www.attoproject.org/).The data thus are maintained, obtain a doi and can be retrieved from that site.

Referee comment 21:

General: rather than 'mesoclimate', 'above-canopy climate' would be a more intuitive name for those measurements.

Author comment 21:

We agree with your advice to rename the "mesoclimate" to "above-canopy climate". Accordingly, this expression was changed throughout the text. Furthermore, the expression "ambient" was changed into "above-canopy".

Referee comment 22:

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.

Author comment 22:

We agree to substitute "These" by "Such".

Author change in the text 22:

P2 L26: "*Such* communities can colonize different substrates, such as soil,…"

Referee comment 23:

P3 L 21: careful, not all bryophytes are desiccation tolerant, even if they are poikilohydric

Author comment 23:

Yes, we agree on that and added the expression "most species" and "for many species".

Author change in the text 23:

P 3 L14: "In a dry state most species can outlast extreme weather conditions, being reactivated by water (Oliver et al., 2005; Proctor, 2000; Proctor et al., 2007; Seel et al., 1992), and for many of them even fog and dew can serve as a source of water (Lancaster et al., 1984; Lange et al., 2006; Lange and Kilian, 1985; Reiter et al., 2008)."

Referee comment 24:

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.

Author comment 24:

That is right, that most of the fluxes were detected for soil communities. However, the information on VOC and aldehydes was performed on epiphytic lichens as well (Kesselmeier et al., 1999; Kuhn et al., 2000; Kuhn and Kesselmeier, 2000; Wilske and Kesselmeier, 1999).

This information was omit in the meantime, due to reorganization of the whole section.

Referee comment 25:

P4 L 8: seasonal variation in what?

Author comment 25:

…the seasonal variation of climatic conditions.

But the whole section was revised, and this sentence was removed.

Referee comment 26:

P5 L2: why ‘ecophysiological’ water content? What other water content is there?

Author comment 26:

It is the “normal” water content of bryophytes, thus the word “ecophysiological” can be deleted.

Author change in the text 26:

P4 L19: “The parameters temperature and light within/on top of the bryophytes *and the water content* of bryophytes are being measured with a microclimate station installed in September 2014 (Fig. S1).”

Referee comment 27:

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)

Author comment 27:

We agree on that and changed these the tenses accordingly.

Author change in the text 27:

P4 L20: “The sensors *were* placed along a vertical gradient…”

P5 L18: “Since the installation, automatic measurements at 5-minute intervals *were taken* with a data logger…”

Referee comment 28:

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.

Author comment 28:

Done accordingly.

Author change in the text 28:

P4 L21: “…, corresponding to the zones 1 to 4 *used by* Mota de Oliveira and ter Steege (2015).”

Referee comment 29:

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’.

Author comment 29:

Done.

Author change in the text 29:

P5 L15: “…, while the light sensors were fixed on ~ 5 cm long sticks and installed next to the bryophyte *samples* (Fig. S1).”

Referee comment 30:

P5 L 19: what do you mean with ‘fluctuations’?

Author comment 30:

With “fluctuation” we meant to describe the oscillations of the measurement. This was changed accordingly.

Author change in the text 30:

P5 L27: “The WC values are *oscillating, causing an inaccuracy of* approximately 15 % dry weight (DW).”

Referee comment 31:

P6 L17: are nutrient content and temperature species-specific?

Author comment 31:

Yes, the nutrient content is species-specific. But the temperature cannot be actively regulated by the species, thus it is not species-specific, but dependent on the environment. However, both parameters influence the measurements of electrical conductivity, hence it is recommended to include an assessment of the species-specific nutrient contents and to do a correction by temperature, to receive the most accurate values.

Referee comment 32:

P7 L1: what is the sensor weight?

Author comment 32:

During the calibration the sensor is fixed in the bryophyte sample and both are lying on the balance. Accordingly, the balance always reads and logs the total weight of ‘sample plus sensor’. But as the weight of the sensor varies slightly depending on the tension of its wire, the weight of the sensor is not the same for each ‘set of calibration measurements’ the same. Thus, the weight of the dry’ sample plus sensor’ is recorded at the end of the measurement, as soon as weight consistence is given. Afterwards the sensor with its wire is removed from the sample and then only the weight of the sample can be recorded.

Referee comment 33:

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.

Author comment 33:

Due to recalculation of the WC the fits are omitted from the new version of the manuscript. A discussion on the inaccuracy of the measurements and the uncertainty of the resulting values is included in the material and methods section.

Author changes in the text 33:

Equations of fits are moved to the supplement, as already described in comment 7.

P7 L30: "The electrical conductivity measurements conducted in the laboratory were used to evaluate their variability. A linear fit was placed through the individual measurements of the different samples resulted in an R^2 ranging from 0.79 to 0.89 and a root mean square error (RMSE) ranging from 68 to 229 % DW (Eq. S1, Fig. S5, Tab. S1b). These results demonstrate the rather high variability of the water content during measurement of different samples or repeated measurements of the same one. As a consequence, the calculated values for the WC should not be considered as absolute but as approximate values. Because of this, no statistical tests have been performed on the bryophyte WCs."

Referee comment 34:

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…

Author comment 34:

Yes, you are right, the rainfall amount should be summed up for certain time periods. However, sometimes rain detection was interrupted and data of short time gaps were missing. For these time periods, we decided to integrate the data not to underestimate the amount to much. We are aware of the fact, that these data gaps and the subsequent calculations may be a source of over- or underestimation. However, this to our knowledge, is the best way to deal with this problem.

Referee comment 35:

P8 L26: explain 'UTC values'; and where are such times presented, and why not always use local time?

Author comment 35:

The UTC is the abbreviation of the universal coordinated time and it is used throughout this study. It allows the synchronization with other data sets. The local time (LT= UTC-4) is only used for the calculation and presentation of diurnal cycles, where it is explicitly marked. The UTC time is used for long data ranges, as monthly and seasonal data.

Author change in the text 35:

P9 L1:" The time for seasonal periods is always taken and presented as UTC *(universal coordinated time)*, except for diurnal cycles, where local time (LT, i.e., UTC-4) is shown, as labeled in the figures."

Referee comment 36:

P9 L23: 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.

Author comment 36:

Here indeed was a mistake. With the WCPl we wanted to explain the point, when net photosynthesis equals respiration, due to limited water availability. This was now changed in the text.

Author change in the text 36:

P9 L16: "The lower water compensation point (WCP_l) presents the minimum WC above which positive *net photosynthesis is reached*."

Referee comment 37:

P9 L28: with 'we found' you mean 'we assumed'?

Author comment 37:

Yes, indeed. We changed the text according to this.

Author change in the text 37:

P10 L2:" The compensation points for the different parameters are also to some extent interrelated, e.g., the water compensation point of lichens has been shown to slightly increase with increasing temperature (Lange, 1980), but *we assumed* that this can be neglected in such a first qualitative approach."

Referee comment 38:

P10 L17: report the statistical results (test and test statistics)! This goes for all 'significant' (or non-significant) results.

Author comment 38:

Indeed, we missed to provide the information of the statistical test result. Now they are provided throughout the text.

Referee comment 39:

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?

Author comment 39:

This section deals with the 'annual fluctuations of monthly mean values'. Accordingly the RH should be understood as the monthly mean.

Author change in the text 39:

P11 L13: "*The monthly average of the above-canopy RH was* characterized by a similar behavior,…"

Referee comment 40:

P12 L25: word missing

Author comment 40:

Done.

Author change in the text 40:

P13 L13: “At 23 m height, the daily amplitudes tended to be higher during the dry compared to the wet seasons, whereas for the mosses at the lowest height levels the amplitudes tended to be higher during the wet season. For the bryophytes at the other height levels the difference of *amplitudes between seasons* was less clear.

Referee comment 41:

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.

Author comment 41: Your assumption indeed is right. We included Figure S9 showing the relation of temperature and water content at different heights along the tree and mentioned the temp/WC relation in the text.

Author change in the text 41:

P14 L1: “Overall, the highest temperatures were reached, when the bryophytes were rather dry (Fig. S9).”

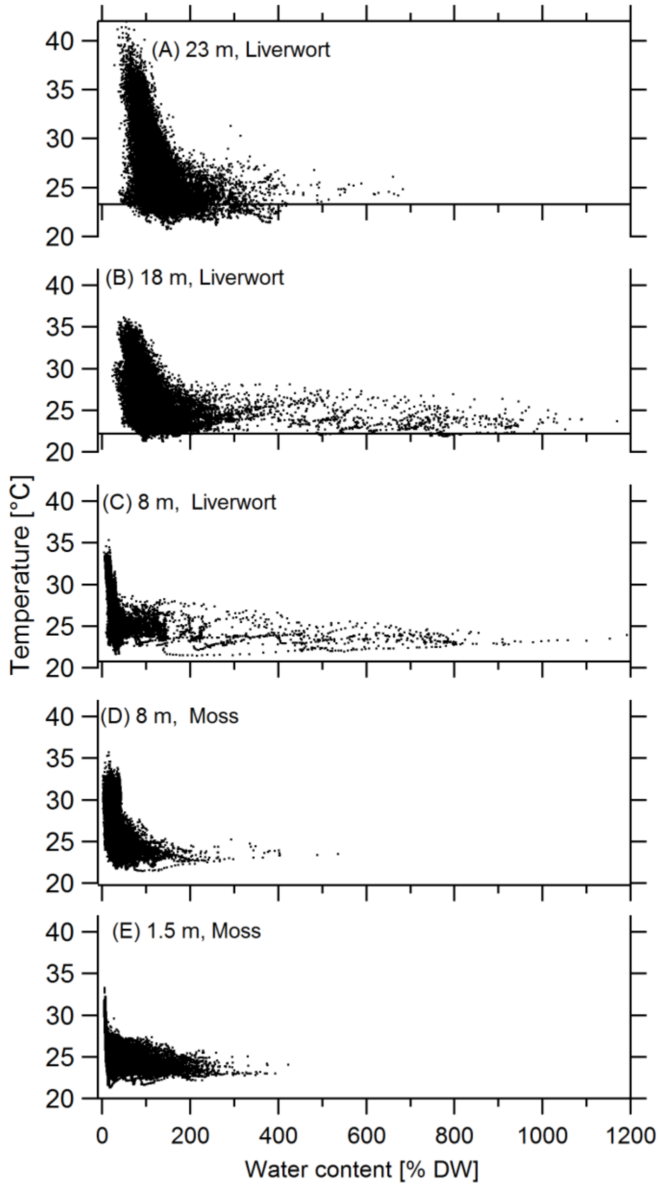

Figure S9: Temperature condition of bryophytes related to their water content. The temperature weremeasured in bryophytes at different height levels along the tree. Data presented as 30-minute averages.

<u>Referee comment 42:</u>

P13 L 27: I guess you mean the LOWER end of the WCPl range?

<u>Author comment 42:</u>

Yes, indeed.

Author change in the text 42:

P17 L25:" As the *lower* end of the WCP_l range (30 % DW) is reached during 100% of the time for liverworts…

Referee comment 43:

P14 L6: you mean 'height', not 'altitude' here.

Author comment 43:

Yes, it should mean 'height'.

Author change in the text 43:

P14 L10: "The microclimatic conditions experienced by bryophytes along *a height* gradient at the ATTO site follow…"

Referee comment 44:

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…

Author comment 44:

This sentence was restructured by adding the missing information.

Author change in the text 44:

P14 L10: "The microclimatic conditions experienced by bryophytes along a height gradient at the ATTO site follow the *seasonal and diel characteristics of the meteorological parameters* to some extent…"

Referee comment 45:

P14 L15-17: mention in methods

Author comment 45:

It is a good idea to provide this information in the methods section. We also rephrased this sentence in the discussion part.

Author change in the text 45:

P5 L11: "*Furthermore, the sensors were installed at the following orientation: at 1.5 and 8 m vertically along the trunk, at 18 m at the upper side of a slightly sloped branch, and at 23 m at the upper side of a vertical branch.*."

P14 L18: "This was most probably an effect of the canopy structure and cushion orientation, as the sensors were installed *vertically along the trunk at 1.5 and 8 m, at the upper side of a slightly sloped branch at 18 m, and at the upper side of a vertical branch at 23 m*."

Referee comment 46:

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)

Author comment 46:

As the sensors were located at 8, 18, and 36 m height, they were out of direct sight for us. One would have needed to install cameras to explore this over time. Maybe it is better to use the expression “could” instead “can”.

Author change in the text 46:

P 14 L22: “This *could* explain lower monthly PAR_{avg} values…”

Referee comment 47:

P14 L20: was PARavg not the monthly average? Do you mean the monthly averages of the daily patterns?

Author comment 47:

We intended to differentiate between *PARavg* and *PARmax*. While PARavg is the average of a certain period, could be month or hour, the PARmax is the maximum PAR value reached per day. In the cited context it is the hourly average presented for the diel cycle.

Referee comment 48:

P15 L9-15: this could indeed be expected and is not very exciting. Your contribution here should be discussing the differences in temperature fluctuations quantitatively.

Author comment 48:

Yes, this is a good point and was considered by insertion of some more detail on this difference.

Author change in the text 48:

P15 L17: “*The daily amplitude of the temperature was about twice as large in the canopy as compared to the understory (Tab. S8).”*

Referee comment 49:

P15 L17-18: mention this reinstallation in the methods too.

Author comment 49:

The reinstallation is mentioned in the methods part according to your advice.

Author changes in the text 49:

P5 L23: “*However, from time to time the water content and temperature sensors fell out from the moss samples, which required a reinstallation. Accordingly, the WC sensor number 6 (1.5 m) was repositioned in 01/2015, WC sensor number 1 (1.5 m) in 11/2015, WC sensor number 1, number 6 to24 and all temperature sensors in 11/2016. The data of the periods when the sensors were not in the bryophyte samples were excluded from further calculations.”*

Referee comment 50:

P15 L21-22: mention and discuss this earlier on.

Author comment 50:

This is a good point, and thus, a description of the uncertainty of the WC is now already provided in the material section.

Author change in the text 50:

P5 L28: "*Furthermore, besides the specific position in the substrate the water content depends on the texture of the sample material, its ion concentration, and the temperature. As all these factors modify the sensor readings, the provided values of the water content should rather be considered as the best possible estimate than as exact values.*"

Referee comment 51:

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 good moss cover. Interesting if this happened to be the west side if this side receives less moisture. Can you explain this?

Author comment 51:

Yes, the west side indeed was chosen as we found the best bryophyte cover there. Although the wind intensity is weaker inside the canopy, we still experienced wind below the canopy and think that this could have an impact on water and habitat conditions at the different expositions, which then could have an effect on the differential growth of bryophytes.

Author change in the text 51:

P16 L6: "At 8 m height, the bryophytes *measured were oriented in western direction. Although wind intensities are clearly weaker below the canopy, we still expect that winds*, predominantly originating from north and north-eastern directions during the wet season (Pöhlker et al., 2018), could have an impact on water and habitat conditions at the different expositions."

Referee comment 52:

P16 L11: why does a light rain facilitate drying??

Author comment 52:

Maybe we expressed it in the wrong way. But we intended to say, that after a light rain event the bryophyte samples dried quicker again, as they got not completely saturated with water. We rephrased this sentences for clarification.

Author changes in the text 52:

P16 L15: "Most rain events in the Central Amazon occur in the early afternoon (12:00 – 14:00 LT) and more than 75 % of them are weak events of less than 10 mm (Cuartas et al., 2007), *which induce only a partial water saturation of the bryophytes. Consequently, the organisms likely dry within a shorter time span as compared to strong rain event, which causes full saturation of the thalli*."

Referee comment 53:

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?

Author comment 53:

This means, it was estimated that approximately 4 % of the total precipitation will reach the ground as stemflow water. Thus, 4 % of the rain water is directly available for epiphytic organisms. By our calculations, this means that 68 to 75 mm per year are available as stemflow water. We rephrased this sentence accordingly.

Author change in the text 53:

P16 L23: "It has been *estimated* that in tropical forests stemflow water could provide up to 4 % of the annual rainfall amount (van Stan and Gordon, 2018), corresponding to values of 68 and 75 mm for the years 2015 and 2016 at the ATTO site."

Referee comment 54:

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.

Author comment 54:

Indeed we did not measure the water holding capacity or only indirectly during the calibrations. Instead, we found high water contents over prolonged times, which we wanted to describe here. We rephrased this sentence for clarification.

Author change in the text 54:

P16 L28: "The high WC of the organisms in the understory might be partly explained by the different water holding capacity of different bryophyte species growing (and measured) there, as understory species of lichen and bryophytes are known to be adapted to long-term water storage (Lakatos et al., 2006; Romero et al., 2006; Williams and Flanagan, 1996)."

Referee comment 55:

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.

Author comment 55:

We meant to say that understory species reach higher net photosynthesis rates at low light conditions. We changed this sentence accordingly.

Author change in the text 55:

P18 L4: "…and it has been reported that understory mosses and lichens indeed show higher rates of net photosynthesis *at low light conditions* than canopy species."

Referee comment 56:

P17 L19-20: words missing

Author comment 56:

Yes indeed. The two sentences were rephrased and linked for clarification.

Author change in the text 56:

P18 L9: "Since net photosynthesis is the sum of simultaneously occurring photosynthesis and respiration processes, positive net photosynthesis rates may still be reached at higher temperatures in the light, as long as the photosynthetic capacity is high enough, whereas during the night, high temperatures could cause a major loss of carbon due to high respiration rates (Lange et al., 2000).

Referee comment 57:

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.

Author comment 57:

Indeed, this type of information can be added to the text.

Author change in the text 57:

P18 L14:"*However, Wagner et al. (2013) explained that the respiration losses themselves do not explain the low bryophyte growth in tropical lowlands, as respiration rates are adapted to the prevailing temperature conditions. At higher elevations they observed higher respiration rates at the same temperatures but still also higher biomass values.* "

Referee comment 58:

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!

Author comment 58:

Yes, indeed the study of Zotz et al. 1997 was performed in a lower montane forest with an altitude of 1100 m, thus we decided to omit the comparison of the WC within the same season.

Referee comment 59:

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??

Author comment 59:

Thanks for this comment, the text was revised accordingly.

Author change in the text 59:

P19 L3: “*In the understory, the WC of cryptogams seems to be predominantly regulated by rain events and the vegetation reduces the evaporation by its shadowing effect. An increased RH mostly slows down the drying in the understory, whereas in the canopy a nightly increase of RH also causes an increase of the bryophytes WC (Fig. 2). The effect of fog events is hard to distinguish from the influence of a high RH, as fog occurs when already a high RH persists. However, some events indicate an increase of WC upon fog (Fig. S8).”*

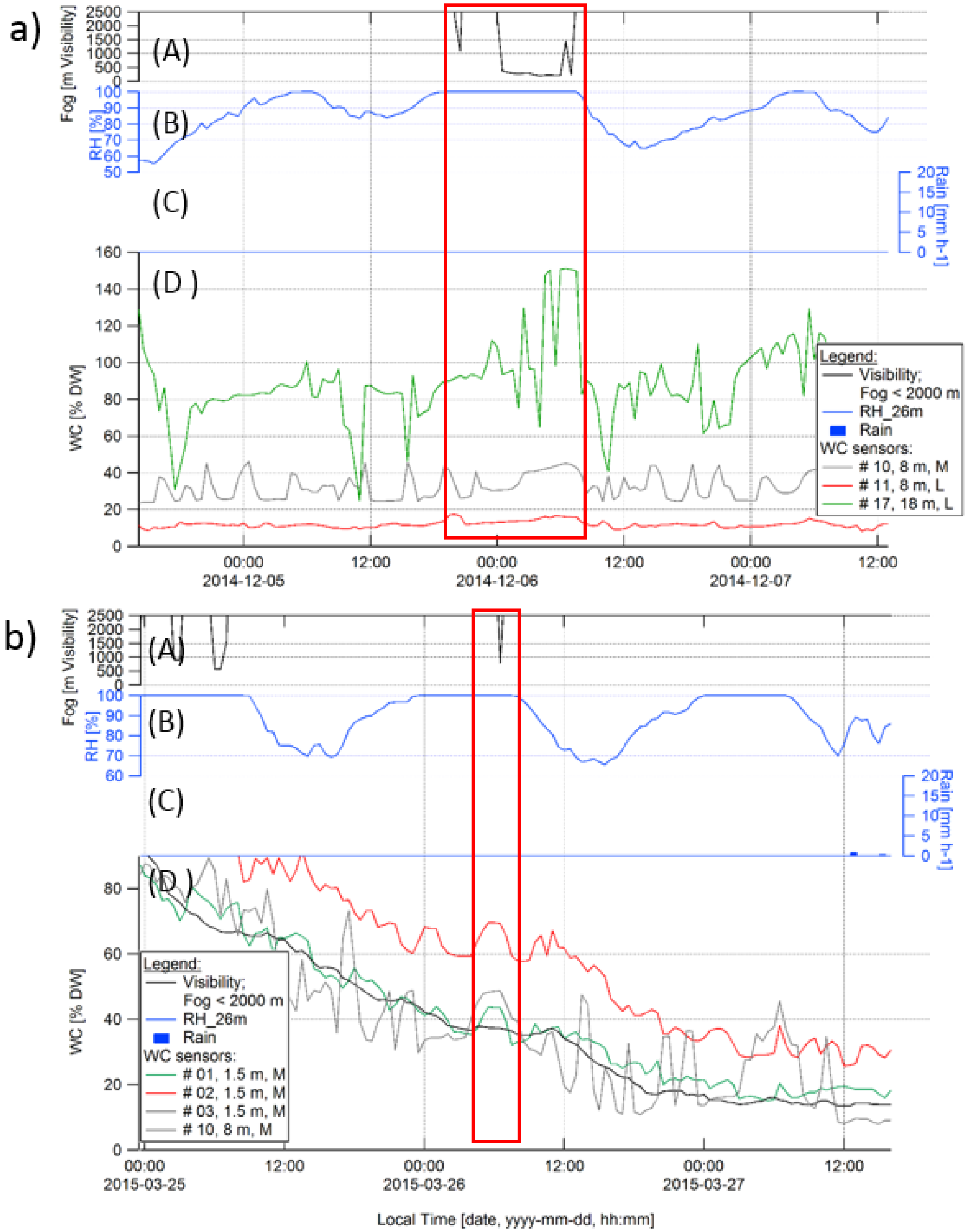

Figure S8: Two exemplary fog events and the reaction of the moisture sensors of the bryophytes (a and b). Each panel presents (A) a fog event defined by a visibility < 2000 m, (B) relative air humidity (RH), (C) rain, and (D) the water content (WC) of the bryophytes. In each panel, the fog event of interest is marked by a red box. For the WC sensors the number, height of installation, and division (M = Moss, L = Liverwort) are given.

Referee comment 60:

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

Author comment60:

“Which” refers to the observation of Pardow and Lakatos (2013), where they describe that understory species are more sensitive to drought than canopy species.

However, the sentences was already omitted, due to reorganization of the section.

Referee comment 61:

Figure S2: in what way are these integrals? Do you mean interpolations?

Author comment 61:

The data with 30-minute time intervals are the average values of six 5-minute grid data. It indeed is better to say “*average*” instead of “integral”.

Referee comment 62:

Supplement: P4 L7: looks like 2 replicates for *Octoblepharum*

Author comment 62:

Supplement Figure S5: Yes, indeed there were two replicates for *Octoblepharum*. This was already mentioned in your comment 16 and was corrected accordingly.

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