

Interactive comment on “Annual net primary productivity of a cyanobacteria dominated biological soil crust in the Gulf savanna, Queensland, Australia” by Burkhard Büdel et al.

Anonymous Referee #3

Received and published: 13 October 2017

Review report on the manuscript bg-2017-374, authored by Burkhard Büdel et al., titled “Annual net primary productivity of a cyanobacteria dominated biological soil crust in the Gulf savanna, Queensland, Australia”, submitted to the journal Biogeosciences, for the Special Issue: Biological soil crusts and their role in biogeochemical processes and cycling

General comments This work deals with the metabolic activity and gas exchange of a cyanobacterial biocrust in the Boodjamulla National Park of Australia. The authors have identified the main species in the biocrust, have carried out lab measurements of net photosynthesis (NP) and dark respiration (DR) as related with variable conditions of

[Printer-friendly version](#)

[Discussion paper](#)



light, temperature and water content (WC), and have carried out one-year monitoring of NP and DR under field conditions, recording also a series of micro-climatic data. The work is very interesting, in the cutting edge of knowledge, fully matches with the scope of the BG special issue, and is well made and well written with almost the only exception of some aspects in the methods section, which has consequences in part of the results. However, I think all my comments can be resolved with a few changes. The results includes the time duration of metabolic activity of the biocrust, the time proportion of NP vs DR, and an explanation of the timing of these processes. It should be noted that the authors provide with annual net CO₂ flow data. They also provide evidence of strong seasonality of these biocrust-atmosphere gas exchange processes, and of the positive annual balance of NP despite the relatively short time during which NP is achieved, showing that the net incoming CO₂ flux produced by NP is larger than the flux due to DR, since the duration of NP seems shorter. The positive annual balance of CO₂ is important because it is not obvious, having into account the low frequency of the conditions in which positive NP can be observed in situ in biocrusts in most of the sites. Discussion and Conclusion are based in the results, include substantial contributions to knowledge and, along with the Introduction, show the experience and up-to-date knowledge of the authors.

Specific comments **Methods** The sampling is a bit confusing. How many samples/replicates were used for the experiment of CO₂ exchange under controlled conditions in lab? It seems that three replicates were used but the sentence in lines 12 and 13 of page 4 introduces some doubts. It is unclear what '9 different samples' are. If I have understood well the main text, I suggest rewriting that sentence, for example: "For every independent variable (light, temperature and water content), a different set of three samples/replicates was used". About the samples used for the one-year monitoring, taking some replicates in each measurement time would have been better. I think that it would been possible with only one cuvette (measurements in a series of replicates can be done in a short enough period to avoid that daily gas exchange variation had a significant effect in part of the replicates with regard to the others).

BGD

[Interactive
comment](#)

[Printer-friendly version](#)

[Discussion paper](#)



However, Fig 3 suggests (showing the time overlaps among bars of different colours) that sometimes through the year only one sample was measured, whereas in others, two, three, four or even five samples were measured. Were there any replicates at certain times of the year? If this is so, I think that this diversity in number of replicates along the year requires some comment. If this is not the case, the Fig 3 should be corrected or explained. Independently from the number of replicates during the monitoring period, the use of several different samples throughout the year would have been probably necessary, because the cyanobacterial biocrust samples have a limited resistance to handling and, after a series of measurements, they should be replaced. (Due to this fact, this is not properly a case of repeated measures over time). But again Fig 3 shows how the duration of the different samples is very different; in some cases the same sample appears to have been used repeatedly during even five or six weeks, but in others, only once. Is this related with the difficult to understand the last sentence of section 2.2 (page 4, lines 20 - 21)? Please, explain better how, “for the one-year monitoring”, you used 21 samples (page 4, line 16) and, though from those 21 used samples, only 11 were selected “for the long-term monitoring” (page 4, lines 20-21). Do you mean that those selected 11 samples were used repeatedly over time while the other 10 were used only once? Why?. By the way, the caption of the Fig 3 should be completed; a caption must be self-explanatory. I think we can assume that replacing the samples along the year for the monitoring is acceptable, apart from possibly necessary, since the monitoring was made only on carefully selected samples of the brown cyanobacterial community dominated by *Symplocastrum purpurascens*, and each sample includes probably billions of cyanobacterial individuals, being a good representation of the whole community. Besides, the microbiota at a certain sampling point could change enough along the year, which decreases the importance of always sampling at the same point. On the other hand, it would be advisable to state explicitly the number of times in which measurements were taken during the monitoring (and when), avoiding the reader having to speculate or discover this from the figures. For example, writing, “twenty-five measurements were taken between November and April,

[Printer-friendly version](#)[Discussion paper](#)

once per week, on the dates shown in the Fig 3". The sentence (page 4, lines 24-25) "The response of ... NP and ... DR to... WC was determined for light, temperature and WC" could be better written, to avoid the expression 'the response to WC was determined for... WC'. I am not sure I have understood this paragraph, particularly after seeing Fig 4. According lines 30-31 of page 4, the temperature-related NP and DR were determined by varying temperatures while keeping constant both light and WC; whereas the WC-related NP and DR were determined (lines 1-2 of page 5) at constant light and different temperatures (in addition to different WC, it is supposed). However, according the Fig 4, it seems that temperatures and WC were not crossed. On the other hand, the first step of the procedure was determining the effect on NP and DR of light in every level of WC, for constant optimal temperature. I wonder whether the determination of the effect on NP and DR of temperature by itself (for constant optimum conditions of light and of WC) was the procedure for establish that optimal temperature. If so, then this should be explicit and constitute the first step. If not, why is studying the effect of temperature by itself important since the effect on NP and DR of WC was determined for every temperature (keeping constant light)? I do not think these experiments were badly done, only that this paragraph is difficult to understand and raises doubts. Since to test the effect of each of these independent variables, at least one of them remained constant, the design is not fully factorial. Probably the triple interaction is significant and, in such a case it would be interesting to understand the biocrust functioning under natural conditions, to study the NP and DR response to that triple interaction, rather than the responses to every independent variables more or less separately. Nevertheless, meanwhile, this work provides very valuable information. In page 4, line 31, is 1500 $\mu\text{mol photons m}^2 \text{ s}^{-1}$ a saturating light? And, how is "optimal WC" defined? A (very short) definition appears only much after, in the line 15 of page 6. On the other hand, is the whole procedure described in the last sentence of this paragraph (lines 2-4 of page 5) repeated for every temperature? This is almost obvious but I think that to say it explicitly would be better.

Results The lines in Fig 4 b are not attributed to any WC levels. A series of lines

similar to those of graph from Fig 4a are expected here, if I understood adequately the methods. Caption of Fig 4 does not help to understand this; in the part referred to graph 4b, any reference to the WC levels is missing. If the graph from Fig 4b refers to the effect on NP and DR of the temperature while keeping constant both light and WC, a value of (optimal) WC is lacking in the graph. What are the lines of Fig 4c, has each sample one line of NP and one line of DR? How? Where are the six different temperatures, since in the graph 4b temperatures and WC are not crossed?. Why the graph 4c shows 47°C as constant? experimental temperature whereas, according the main text (page 5, lines 1-2), six temperature were crossed with different WC levels?. Why was the graphed experiment made at 47°C instead of at the optimal temperature (32°C)?. Perhaps the authors plotted a graph for every temperature and only show the last one; but, in such a case, what are the lines of Fig 4c?. The wording of Methods and/or Results should be a bit improved.

Conclusion In page 10, line 29, the sentence “three months having a negative balance probably due to regrowth of the biocrust” is hard to understand since, by default, ‘regrowth’ implies growth, and growth requires net CO₂ assimilation. Besides, I think that this sentence about the regrowth requires an explanation, defining what exactly means ‘regrowth’ in this case, since this is closely related with the hypothesis presented at the end of the introduction (page 3, lines 12-14). Perhaps the authors used here the word ‘regrowth’ to refer to the recovery of metabolic activity after the latent-life span of the dry season. It would be also advisable a better definition of that hypothesis in the Introduction

Technical corrections Page 4, line 2; the expression ‘(factorial design)’ would be better than ‘(factorial analysis)’. Indeed, the experiment was factorial (although not fully factorial); but, no statistical analysis is explicit in the Method section. Page 4, line 19: ‘NP’ and ‘DR’ appear in that line, whereas they are defined after, in lines 24 and 25 of that page 4 Page 5, line 27: “2” is lacking after ‘Fig’ Page 6, line 15: A dot or a semicolon seems advisable just before the last word of that line.

[Printer-friendly version](#)[Discussion paper](#)

Printer-friendly version

Discussion paper

