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Biogeosciences Discussions

Interactive comment on "Microbiotic crusts on soil, rock and plants: neglected major players in the global cycles of carbon and nitrogen?" *by* W. Elbert et al.

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Referee General Comment:

The scientific question concerning the role of microbiotic crusts in the global C and N exchange addressed in the paper is a very challenging task, and is well within the scope of Biogeosciences. The authors state that microbiotic crusts are likely to play a major role in the global biogeochemical cycles of carbon and the biological nitrogen fixation. They have screened literature data on exchange rates and extrapolated them to the globe. According to the presented results, the global biomass of crusts - in terms of carbon - corresponds to 2% of the global biomass of terrestrial vegetation, and ac-

counts for 6% of the respective NPP, meaning that microbiotic crusts are 3 times more efficient in NPP as the vegetation. The role of microbiotic crusts in the global biological nitrogen fixation (N2, to prevent any misunderstanding by agronomist) is estimated to be even more important (ca. 40% of the total global biological N fixation). Personally, I like the idea of making strong statements, to attract fellow scientists to account for something important that might have been overlooked so far, especially in a field where only few data are available yet. Having said this, I have to emphasize that there are severe concerns about the approach used in the extrapolation procedure, and hence about the representativeness of the output data. The authors present a combination of statistical results (medians) using trace gas exchange protocols that, to my understanding, were not established to allow this sort of rigorous global extrapolation. The main scientific work needed to make this an "educated guess" would be to thoroughly discuss (on a sound scientific basis) the representativeness of the exchange and crust coverage data used.

Response:

We thank the referee (U. Kuhn) for reviewing our manuscript. The stimulating comments are very welcome, and the suggestions for improvement will be implemented upon revision as far as possible. Detailed responses to the individual comments are given below.

We agree that the availability of measurement data is very limited. In the course of manuscript revision we were able to include some more data from various locations and habitats, and we considered the statistical distribution of the available data to improve the uncertainty estimates as detailed below and in the revised manuscript.

Moreover, we include additional data of land surface coverage by BSC and BRC (Table 10). The available information is not sufficient to resolve the global coverage of land surfaces by BSC on the level of ecosystems and regions. The same applies for the coverage of plant surfaces by EPC (Table 6). Nevertheless, the available information

shows that the use of median values is not unrealistic, because there are no clear indications for systematic deviations between different types of regions, climatic zones and ecosystems: high and low values are scattered over arid, temperate, tropical, mid-latitude and high-latitude regions.

Thus, we see currently not better way than using median values of the available data to arrive at a first global estimate. More measurement data will be needed for more "educated guesses" and regionally resolved modeling approaches, respectively. If it were already possible to do much better, one would have expected that a first global estimate had already been presented earlier.

With regard to the available gas exchange data, we are aware that the underlying measurement techniques may not be perfect. For clarification we intend to reference a recent study dealing with this issue (Bader et al., 2010). Nevertheless, we are confident that it makes sense to use a rough first estimate for orientation, and that our exploratory study may help to trigger and target further investigations.

Referee Comment 1:

Maximum photosynthetic rates, partly measured in the laboratory for short periods of optimal conditions (e.g. Lange et al. 1997; I didn't screen them all) can not easily be extrapolated to the whole year in the real world. In contrast, the metabolic activity of the poikilohydric organisms are characterized by quick and drastic changes in moisture availability and long periods of drought, hence physiological inactivity, especially in arid and semi-arid regions (but not restricted to those). A typical diel course is characterized by nocturnal hydration, by fog and/or dew, followed by activated dark respiration of the crusts, and - after sunrise - followed by a short period of positive net photosynthesis that only continues until metabolic inactivation occurs from desiccation. Due to rapid desiccation, the combination of light saturation with optimal water content rarely occurs under field conditions (this mainly refers to the maximal photosynthesis data cited in Table 1 and 3), and may result in several month of inactivity in semiarid and arid

zones. Therefore the authors have to explicitly explain the "calculation background" used for yearly flux values ("NPmax values were scaled to estimated average ambient conditions"), e.g., what is the calculation basis of the "adopted balance" or how did they achieve the "mean values of balances" for the whole year in Table 1 and 3? In Table 9 there is not even any comment on that in the table. For poikilohydric organisms the accurate scaling from lab data to integral yearly rates (if at all possible) is the linchpin of the entire calculation exercise.

Response:

Following up on the above comment and suggestions, we intend to clarify the "calculation background" in the methods section of the revised manuscript as follows.

Unless mentioned otherwise, the tabulated data were taken directly from the cited references. In these studies the scaling necessary to account for limitations of photosynthetic activity, dark respiration and surface coverage has already been performed by the authors. For studies that had reported only maximal photosynthesis rates under optimal conditions (usually given in units of μmol m-2 s-1), we converted the reported rates into units of g m-2 a-1 (Tab. 1 and 3, column 4), and scaled these values by a factor of 1/72 which takes into account the following effects and characteristic parameters obtained from various studies (Lange, 2000; Zotz and Rottenberger, 2001; Lange, 2003; Lange and Green, 2004; Lange and Green, 2008):

1) The photosynthetic activity is usually limited to about one third of the maximal value (factor $\frac{1}{3}$) and about one quarter of a day (factor $\frac{1}{4}$).

2) Approximately half of the fixed carbon is lost by dark respiration (factor $\frac{1}{2}$).

3) About one third of the surfaces of (semi-)arid soils and rocks (Tab. 10) and of vascular plants (evergreen leaves, evergreen needles, and stems and branches of trees, Tab. 6) are typically covered by BSC, BRC or EPC, respectively (factor $\frac{1}{3}$).

Reported rates from studies that had already accounted for partial surface coverage but

not for the limitations of photosynthetic activity and dark respiration were scaled with a factor of 1/24 (Tab. 1). Reported rates from studies that had already accounted for dark respiration and partial surface coverage but not for the limitations of photosynthetic activity were scaled with a factor of 1/12 (Tab. 1). Reported rates from studies that had already accounted for the limitations of photosynthetic activity and dark respiration but not for partial surface coverage were scaled with a factor of 1/3 (Tab. 1).

In addition to the more detailed explanation in the methods section, we have reformatted several of the tables and included a detailed record of applied scaling factors together with explanatory footnotes.

Referee Comment 2:

From a statistical point of view, using the median instead of the arithmetic mean value is justified. But as long as the representativeness of the data can not be assessed, the uncertainty of this approach might still be huge (much more than the factor of 2 as stated by the authors). One quantitative indicator of the uncertainty of the approach is, e.g., the difference between using the median value for extrapolation of the global nitrogen fixation rate by epiphytic crusts (contributing the major share of the global crust estimate) of 0.35 g m-2 a-1 (Table 9), compared to the respective arithmetic mean value of 6.2 (standard deviation 14.6) g m-2 a-1 (Table 9). The difference is spanning more than an order of magnitude of the most basic term concerning the crusts N fixation. Using the arithmetic mean (instead of the median), the authors would come up with a global N fixation of ca. 600 Tg per year by epiphytic crusts alone, which is 6 times the amount of the current global estimate of total global biological N fixation (!). This fact should justify a note in the discussion. A minor issue in this context: I think the authors mixed up BSC and EPC in their global numbers of N fixation (". . . we obtain global estimates of 30 Tg a-1 and 15 Tg a-1 for nitrogen fixation by BSC and EPC.")

Response:

Following up on the above comment and suggestions, we intend to clarify the approach

and improve the uncertainty estimates in the methods section of the revised manuscript as follows.

For global upscaling we used median rather than arithmetic mean values in order to obtain conservative estimates. The median values were generally in fair agreement with the corresponding arithmetic mean values, and the relative standard errors of the mean values (relative standard deviation divided by the square root of the number of data points) ranged up to $\sim$80%. The 10th and 90th percentiles deviated from the median values by factors of 2-5 for carbon fixation by BSC and BRC, 3-20 for carbon fixation by EPC, and by factors of 3-4 for nitrogen fixation by BSC and EPC (Tables 1, 3, and 9). The percentile and standard error values indicate that the first global estimates presented here are uncertain by factors of 2-20. More long-term studies and detailed measurements of biological crust coverage, biomass, and uptake fluxes will be needed to reduce the uncertainties (Bader et al., 2010).

With regard to the uncertainties of N fixation by EPC, we agree that using the arithmetic mean would lead to seemingly unrealistic values. In our opinion, however, this just confirms that using the median is a better approach (as agreed by the referee) and does not warrant further discussion. Note that the estimate for the overall biological nitrogen fixation is also still highly uncertain (100-290 Tg a-1, (Galloway, 2005)). This information shall be added in the revised manuscript.

Referee comment 3:

Another critical issue are the global crust coverage data used. Scientist interested in the exchange characteristics of microbiotic crusts tend to go where crusts are most abundant (hot spots). It seems questionable to directly use the global coverage data cited (for dry areas the authors use 100% coverage if I got that right; and for epiphytic crusts on vegetated areas they use 35-50% in Table 7) in combination with the median data of the trace gas exchange measurements. Microbiotic crust coverage data are rather meant in terms of whether there is some occurrence, but not necessarily in an

amount comparable to those areas used for the cited trace gas exchange measurements (see above).

Response:

We agree with the assumption of the referee that scientists tend to go where crusts are most abundant. To ensure that we use a realistic coverage value for the estimation of global fluxes, we collected as many data from the literature as we could get, and will present these data in a new table added to the revised manuscript (Table 10). This overview includes 36 average values and 10 value ranges of soil surface coverage by BSC and BRC in various regions. These data should be fairly representative for the respective ecosystems and regions, assuming that the authors selected characteristic sites for their studies. From the data in this table and Table 6 we adopted a coverage value of 1/3 (33%) for the scaling of fluxes as detailed in the methods section of the revised manuscript.

Note that we had already accounted for partial surface coverage in the original manuscript and calculations, as indicated by a footnote in Table 1. We realize that the scaling procedures were not well described. This will be corrected in the revised manuscript as outlined above.

Referee Comment 4:

A minor issue, concerning the title: It should be emphasized that the CO2 exchange of microbiotic crusts is actually not really neglected in the current carbon flux inventories, but the crust's contribution is mostly (co-)measured as an integral component of the system under investigation (soil, leaf, branch, tree, ecosystem); at least when applying micrometeorological flux measurements. However, the triggering functions are totally different to, e.g., higher plants, which is why the crusts contribution is not correctly taken into accounted in present modelling exercises, e.g., due to the lack on stomatal control, the dependence on the ambient relative humidity and rain events necessary for their general physiological functioning. This way I totally agree with the authors that the

contribution of microbiotic crusts has to be specifically characterized and hence has to be separately accounted for in the inventories.

Response:

We agree that CO2 exchange of microbiotic crusts should be included in micrometeorological flux measurements. To our knowledge, however, carbon flux inventories are not always based on micrometeorological flux measurements. Even if the effects of biological crusts were implicitly included in all carbon flux inventories, their explicit consideration appears to be necessary for a full mechanistic understanding of the biogeochemical cycles of carbon and nitrogen. Thus we consider the formulation of our title as justified, especially with the question mark at the end.

Referee Comment 5:

The authors should state their idea where the photosynthesised carbon ultimately ends. Obviously in the arid zones there is not a lot of C accumulation within the soil layer (BSC depth do hardly extend more than a few cm of soil depth). I understand that most of the carbon will be allocated to the energy support of the heterotrophic fungal partner; a short discussion would be appropriate, just not to leave the reader alone with the potential misinterpretation that microbiotic crusts might also be responsible for comparable amounts of carbon sequestration.

Response:

As stated at the beginning of the results and discussion section of our manuscript, the net uptake values reported in our study should be considered as net primary production (NPP). (Chapin et al., 2006). Further discussion of carbon balance and flux terminology (NPP, net ecosystem production, ecosystem carbon accumulation, sequestration, etc.) would go beyond the scope of our study.

As outlined in our manuscript, biological crusts can release fixed carbon and nitrogen in the form of sugars, polyols, amino acids, nitrate, ammonium, etc. to the surrounding

ecosystem. The released compounds can be readily taken up by vascular plants or animals (Belnap, 2002; Belnap, 2003). Moreover, large amounts of crustal biomass can be removed by wind and storm events (Loris et al., 2009). These processes may explain why the biomass of biological crusts is relatively low compared to the calculated NPP values. Further investigations will be required to fully unravel and quantify these effects on regional and global scales.

Referee Comment 6:

This brings me to a rather academic question: in ecosystem studies the soil respiration (by microbacteria and fungi within the soil layer) is accounted for as heterotrophic respiration and is subtracted from the daytime net CO2 exchange to achieve NPP values. For microbiotic soil crusts the definition of NPP is hence difficult. In most cases the organisms are dominated by the fungal partners not being able to photosynthesise, but rather contributing a major share of respiration. I assume in this context the fungal respiration activity is accounted for as autotrophic respiration in terms of NPP?

Response:

Indeed, autotrophic respiration within the crusts has been accounted for (see above). The measured and reported net photosynthesis rates generally do account for photoautotrophic respiration. Dark respiration is taken into account in the applied scaling procedures, which will be clarified in the methods section of the revised manuscript.

Referee Comment 7:

Regarding the nitrogen fixation, the same critical issues as for the carbon data apply, i.e., concerning the representativeness of (i) the extrapolation from short-term data under optimized conditions to yearly averages, and (ii) the use of the global coverage data. Else: biological nitrogen fixation in soil crust is mainly allocated to cyanobacteria. But not all soil crusts contain this partner and not all cyanobacteria fix nitrogen (also being dependent on the environmental conditions of their micro-habitat). I understand the

manuscript as a rather provocative statement that there might be something that has been over-looked, and one should invest efforts in further investigations. The authors should better stress this point, i.e., the rather speculative nature of their approach. The main scientific work needed to be done is a thorough declaration of the calculation basis, with respective discussion on the representativeness of the data sets used. They should ultimately apply only those data that are justified to being extrapolated to yearly integrals, even for the field data. Even an upper limit estimate should not do without this consideration/discussion.

Response:

With regard to nitrogen data we have not performed any scaling, because the authors of the referenced studies had already reported annual fluxes. This had already been stated in the original manuscript but will be further clarified in the methods section of the revised manuscript as follows: With regard to nitrogen fixation, the tabulated flux data were taken directly from the referenced studies, in which the authors had already reported annual fluxes (Tab. 9).

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