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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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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 accounts 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.

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.

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.”)

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 measure-

ments (see above).

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.

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.

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?

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.

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