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Biogeosciences Discussions

Interactive comment on "Modeling the variability in annual carbon fluxes related to biological soil crusts in a Mediterranean shrubland" *by* B. Wilske et al.

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We thank the referee for the critical comments and corrections as well as for acknowledging the importance of modeling BSC-related carbon fluxes. We like to emphasize that (1) to our knowledge this paper represents the first estimate of annual BSC-related carbon fluxes based on field measurements of undisturbed soil-dwelling BSC within the BSC-soil continuum. This estimate may include some uncertainty but both the measurements and analysis methodology were sound enough to provide this estimate. (2) Similarly, our paper presents the first attempt to model BSC-related CO_2 fluxes in drylands, and hence, the accuracy of PdAM cannot be compared to other studies. While better measurements and/or models may become eventually available, at present we are surprised by the standards the referee applies, e.g., regarding CO_2 diffusivity and the activity trigger of the model. Below we respond to the specific points.

Anonymous Referee #2: General comments:

This paper describes a modeling effort focused on determining the annual carbon exchange of biological soil crusts (BSCs) in the Negev Desert of Israel. CO2 exchange of BSCs is extremely difficult to measure over long time periods, and the exchange is very dependent on moisture. The topic is appropriate for the journal, and the authors are aware of and cite the appropriate literature. The paper is for the most part well-written with some exceptions describing the modeling as noted below. Modeling is an important approach which, if done well, could lead to important information regarding the role of BSCs in soil carbon and nutrient cycling. For example, there have been some recent papers (highlighted by the authors in this paper) that claim very large C uptake in arid regions, and one hypotheses for the uptake that has been proposed involves BSCs. This modeling effort, if the results can be believed, suggest that BSCs are not likely to gain enough C to account for the large reports of Wohlfahrt et al. 2008 and Xie et al. 2008.

Anonymous Referee #2: General comments (continued): I have some concerns about the methodology used for the measurements. First, CO2 exchange was measured in a differential IRGA mode from chambers with intact BSC compared to chambers with the BSC removed, and this differential measurement was used to assess BSC exchange. The idea is clever, but soil gas physical transport will be entirely different in the two treatments. The diffusivity is a function of soil physical properties (texture) as well as environmental ones (moisture and temperature). The presence of a BSC, with mucilaginous sheaths of the cyanobacteria, presence of fungal hyphae, aggregates, etc., will certainly alter the diffusivity relative to bare soil. This is likely to be especially important when the soils are wet. A simple comparison of CO2 exchange in the two treatments is not the same as measuring the fluxes from the BSCs as the authors assume.

Authors reply: We understand the concerns of the referee regarding the disturbance of the upper millimeters in the reference samples. We agree that the diffusivity within these millimeters would be changed by the treatment; however, we think that the effect of the treatment should not be overrated. We mentioned in our previous paper, that (1) net BSC-related CO_2fluxes were not significantly different form results of another study that used samples from the same site, and (2) soil CO_2efflux was comparable to results from other dryland soils. The referee mentions that soil physical properties (texture) and environmental ones (moisture and temperature) exert influence on gas diffusion in soils. We like to consider three points to explain why we used the method and think that it did not change the fluxes significantly.

(1) We can assume the production rate in the undisturbed soil column was not altered through the treatment, and therefore nor the overall net flux from the soil column below the 10 mm of our treatment. The reference collars were refilled with soil from the same site; hence, the substrate for the respiration in the upper 10 mm was not changed.

(2) Other studies have shown that mainly macropores or larger gas-filled spaces influenced gas movement in soils because the relative gas diffusion coefficient near a macropore may be one order of magnitude higher than in regions without macropores (Allaire et al., 2008; Moldrup et al., 2000). We described in the methods (Wilske et al., 2008, page 1413, section 2.3.1.) that the reference samples were once flooded with a small amount of distilled water to recover the settled structure of the (sandy loam to loam) soil. Thus, we think the final step in the treatment has avoided magnitudinal changes in the diffusion within the upper millimeters of the soil, and below these millimeters, the soil column remained unchanged. If we consider the soil column as a series of resistances that affect diffusivity, then the upper 10 mm will be only one resistance among others and may not change significantly the overall soil CO_2 efflux as long as the diffusivity in this upper layer is not strikingly changed.

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(3) Furthermore, there is actually no disagreement between the authors and the referee regarding "The presence of a BSC, with mucilaginous sheaths of the cyanobacteria, presence of fungal hyphae, aggregates, etc., will certainly alter the diffusivity relative to bare soil." Soil CO_2 production creates a CO_2 concentration gradient in the soil column that is dependent on the resistance to diffusive transport. Given that CO_2 production was unchanged, the soil CO_2 concentration profile may indeed be different between a bare soil and a soil with BSC because a lower soil-atmosphere gradient develops in the absence of a BSC layer resistance. However, this is not believed to affect the net flux. Fully aware of the BSC-soil continuum and that we cannot (and need not) to distinguish between BSC and BSC-soil interactions, we use the term "BSC-related CO_2 fluxes" throughout the paper. We have mentioned the same context in our previous paper (Wilske et al., 2008, page 1414, last paragraph of section 2.3.3. CO_2 exchange measurements). To avoid misunderstanding, we suggest repeating the last sentence of the same paragraph in the discussion of our present paper.

As for the effects of temperature and moisture, we think that the referee is well aware that cracks appear in soils under dry and hot conditions. These cracks appeared in both the reference and BSC samples similar to the surrounding surfaces. While these cracks may influence summer soil CO_2 efflux, we did not observe CO_2 deposition (uptake) under dry conditions, which suggests that there was no difference between the samples regarding this efflux.

Finally, we like to point to studies that investigated the soil emission of other gas species (Feig et al., 2008; Otter et al., 1999; van Dijk et al., 2002) and partly used laboratory experiments to model emissions for the respective sample areas.

Anonymous Referee #2: General comments (continued):

Second, a clear chamber closed in the sunshine of the Negev Desert for 15 mins will most certainly have a large internal temperature increase during some times of the year, which will have all kinds of biological and physical influences. These make the

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measurements very suspect in my mind. However, the measurements are not the subject of the present manuscript – they have already been published. If one takes them at face value, then our role here is to evaluate the modeling effort.

Authors reply: We were aware of the heating effects that come with chamber measurements. We wrote in our previous paper "The general enclosure time of a sample was 15 min, but protocols were also adapted to fluctuations of light, temperature, and moisture, looking for the best tradeoff between data acquisition and keeping samples open for natural exchange of heat and moisture" (Wilske et al, 2008, page 1414 end of left column). We increased the number of enclosure rotations (and shortened the time of enclosure) during rain events, day time, and higher insolation whenever BSC were moist (see also our reply to referee 1). BSC were active, with few exceptions, in the winter at low sun angle and lower radiation flux. A clear chamber closed in the sunshine of the Negev Desert for 15 minutes showed a large internal temperature increase. However, during those periods the BSC were usually dry and inactive - or in other words - in a state in which they outlast periods of extreme heat.

Anonymous Referee #2: General comments (continued): I personally would not try to take 10 short periods of data with variable quality of model results compared to observations (Figure 1) and try to extrapolate that to 3 years of annual carbon gain – this is extremely weak. Try to imagine measuring air temperature during 10 different 3-day periods, then predicting what the total sum of annual air temperature would be for an entire year. You're almost certain to be wrong. How can one possibly get something as complicated as BSC carbon exchange right with this approach?

Authors reply: The depiction of the referee is not correct. The foremost items are: (1) We are not extrapolating but use a model. Data were obtained at particular times (dates) after BSC were activated by natural precipitation events in order to simulate the activation and to run the model simulation based on an available long term climatic data set. Such an approach is of course not an extrapolation. (2) We are not predicting air temperature for a year or even for one hour. The model runs with 15-min means of climate data from a station at a stone-throw distance. (3) We are sure the referee is aware that the model is not required to simulate the exact climate conditions for a period of 365 days, but mainly for the short season when moisture is available. We had collected evidence (references, personal observations and thorough communications with scientists who know the ILTER site for many years), that dew on the soil surface is rare. Thus, we can assume that moisture availability for the bulk of BSC-related CO_2 fluxes is closely linked with rain events and subsequent periods of high soil moisture. For instance, the climate station recorded PPT at 60 individual days in the period 2002-2003, of which 12 days showed a PPT$\leq$ 0.2 mm. We suggest expanding Table 4 and breaking the annual PPT down into such benchmarks.

(4) We regard it scientifically disputable that the carbon exchange of BSC is more complicated than the one of vascular vegetation, and the latter one is subject to extensive modeling.

Anonymous Referee #2: General comments (continued): ... For example, Lange in many papers has shown that the moisture-activity relationships of BSCs and lichens vary with temperature (e.g., Fig 3 of Lange et al. (1998) Functional Ecology 12:195). This tremendously important functional relationship is missing in your approach if you don't have measurements which show such variability to train the model.

Authors reply: We have shown the effect of the moisture-temperature relationship on the investigated BSC in our previous paper (Wilske et al., 2008, Figure 5b). Thus, we have data that show the variability to train a model, for instance one that would operate with the input from the continuous readings of a soil-surface moisture sensor. However, the combination of a time- and amplitude regulation in adaptation to the algorithm for the gas exchange did not make it meaningful to use a finer-scale calibration than the levels we have introduced. The referee seems to have unrealistically high expectations relative to modeling the temperature-moisture relationship, or in general, how accurately models should be capable of representing natural processes. Furthermore we like to emphasize that studies on the CO_2 exchange of BSCs and lichens as dependent

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on water content (WC), light and temperature (e.g., Lange et al., 1998) were conducted with individual species. Our samples encompassed a community of species (mosses, lichens, cyanobacteria) within each sample collar. Although the samples were selected to include about the same area of each contribution, this standard may be regarded as too low for aiming at a fine-scale calibration similar to studies that involved individual species and laboratory conditions. In other words, we think it is a valid assumption that the sum of spatially mixed compositions of BSC attains intermediate sensitivities to the temperature- moisture relationship. A differentiation into sensitivities of individual species would only make sense if their individual contribution would be exactly known (and not change over time). From our point of view, further diversification of activity levels in mixed BSC is presently not appropriate (other than the ones used in the model: optimum, 50% of the optimum, and the similar dividing intermediate fractions).

Anonymous Referee #2: General comments (continued): ... The A minimum first step would be to try to train the model with a subset of the data then see how it performs to predict other periods of observation. A Monte Carlo approach could be used at least, perhaps use 6 days to train the model, and predict the other 4, and repeat this thousands of times, each time adding up the total C exchange for the unknown days. Look at the variability of results for the unknown days in all the simulations, and you get a sense for uncertainty. I would have a hard time believing (or not believing) the results presented in Table 4 even after such an analysis. **Authors reply:** We regret that the review of our paper does not indicate, whether the referee regards the PdAM output as to high or to low, and which part of the model the referee regards as the main cause for the outputs being unbelievable (the activation switch or the algorithm). Instead, the referee suggests using a Monte Carlo approach. We discussed the review beyond the circle of authors and we are now confident that not all experts would reject our approach. A Monte Carlo approach sounds scientifically correct. A random sampling of measurement days could have -in the worst case- suggested to try modeling BSC-related CO_2 fluxes with data that include no or little activity. Conversely, we selected the highest diversity in daily moisture conditions that our data could provide

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(see BGD 6 Table 2). Thereby, our approach of modeling gave the highest priority to the issue that must have foremost importance relative to simulating the activity (and carbon exchange) of poikilohydric vegetation: To approximate the amount of time that BSC actively participate in ecosystem carbon fluxes. Almost in agreement with both referees, we started modeling with a subset of data, i.e., the first year of data. Tests of the second-year data required some adjustments and thus the second year could not be used for validation.

We used climate data, which were collected and defined for the whole period of the simulation. We modeled BSC-related CO_2 flux quasi as an interpolation between points of known flux measurements based on response functions derived from the range of measurement conditions. We still think there are other ways to establish the uncertainty of the model and produce sufficient confidence that the model can reproduce the bulk of BSC-related CO_2 flux. We suggest adding a sensitivity analysis based on realistic limits of the response functions, or in other words, run the model with alternative settings concerning the activation switch and or changes in the algorithm. We consider expanding Table 4 by including results from flanking switch settings showing root square mean errors and changes in the carbon deposition relative to measured days, and the resulting changes in the overall carbon deposition for a year. This will provide high transparency to the reader in how far smaller deviations in the model setting may affect the final carbon deposition. As a matter of fact, our discussion paper had already used this approach by showing not only the results of the final model but also the differences with regard to using individual contributions of RH and SM to the activation switch.

Anonymous Referee #2: Despite my concerns about the conclusions, the discussion is well-written and informative, as is the introduction.

Authors reply: The referee considers that most parts of the paper are well-written and informative. We think that we can come up with an improved model description, which fits better with the other parts of the paper. Specifically, we suggest changing

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the following items:

(1) We will remove the section on the “extrapolation of mean exchange rates”, because it distracts from the modeling approach. The improved chapter 3 (Assessment of BSC-related carbon deposition) will start instead with a section 3.1. Basic assumptions to a BSC activity model. (2) We will try to improve our flow chart to allow better orientation along the description of the individual model components and their interaction. (3) We will increase the transparency of model outputs by informing about changes related with alternative model settings.

Anonymous Referee #2: Specific comments: Pg 7296 line 25: (Stone 2008) – this is not peer reviewed, it's a news article

Authors reply: The paper by Stone (2008), published in the journal Science as “news of the week”, has initiated very controversial discussions. In accordance with the journal, we think it is worthwhile to include a contribution that raised that much interest within the science community. Future research will show whether it is right or wrong.

Anonymous Referee #2: Pg 7299, line 16: your grammar is incorrect

Authors reply: We will correct the grammar of the sentence “The species compositions contributing to BSC in Israel's Negev Desert was compiled by Friedmann and Galun (1974) and Lange et al. (1992)” by writing “Friedmann and Galun (1974) and Lange et al. (1992) compiled the species that contribute to the composition of BSC in the Negev Desert.”

Anonymous Referee #2:7301, line 1: photosynthetically active radiation?

Authors reply: We will change **“**photosynthetic active radiation” to “photosynthetically active radiation”.

Anonymous Referee #2:7302, line 8: the means are terribly vague – over what time interval? Over differing precipitation amounts? Etc.

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Authors reply: The mean exchange rates were calculated as the mean of measured exchange rates during days when BSC where active. To obtain a reasonable mean, we interpolated data to close longer gaps and avoid an imbalance in the number of data points throughout the days. We did not calculate the mean of interpolated daily carbon gains, but used the average of day and night flux to be accumulated by moisture records in 15-min intervals. However, as also mentioned in our reply to referee 1, we consider removing completely the rough estimate based on mean exchange rates, because it obviously distracts from the main message of the paper: BSC-related carbon fluxes can be modeled using detailed climate records.

Anonymous Referee #2: The description of the modeling on pgs 7304 is not especially clear.

Authors reply: We will try to improve the description and may come up with a better flow chart.

Anonymous Referee #2: Pg 7311: the conclusion regarding the possible contribution of BSCs in the Wohlfahrt study is important, but the speculative text of lines 27 through the end of the paragraph should probably be cut.

Authors reply: We regarded it important to point out that factors affecting BSC-related carbon fluxes at the two sites may differ; hence, there will be an element of uncertainty in estimating the proportional contribution of BSC fluxes at the Mojave site based on results from our site.

Anonymous Referee #2: Technical corrections: "insolation", not "insulation", is the correct word as applied to solar radiation – the latter refers to thermal diffusivity or electrical properties

Authors reply: We thank the referee for the correction of the misspelling, which apparently came into the paper through an automatic spell check.

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