

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

Anonymous Referee #1

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General comments:

The paper submitted by Wilske et al. aims at modelling the annual carbon fluxes of biological soil crusts in the northern Negev Desert. In doing so, they have tackled an important scientific issue that fits well in the scope of BG. Recently, BSC have been identified as a terrestrial CO₂ sink, that may fill at least part of the existing knowledge gap regarding the fixation of 20 % of the terrestrial CO₂ (<http://www.whrc.org/carbon/missingc.htm>; Wohlfahrt et al., 2008; Elbert et al., 2009). Up to now, however, profound annual balances of BSC are rare and methodically extremely difficult to be accomplished (Lange, 2000a; Lange, 2000b; Lange, 2003).

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Therefore, the idea of Wilske et al. to establish a modelling approach, which gets along without elaborative long-term gas-exchange and/or microclimate measurements looks very interesting at first sight. On a closer inspection, however, this study exhibits numerous substantial deficiencies and major mistakes, that make their approach and the resulting deliverables extremely questionable.

Specific comments:

The authors base their modelling approach on data partly published in a previous paper (Wilske et al., 2008). In order to fully understand their methodological approach, the data assessment described there consequently also has to be taken into account:

1. Methodological setup of gas exchange measurements

In their gas exchange measurements, the authors measure BSC-related CO₂-fluxes against CO₂-fluxes of disturbed soil in a differential mode (BG 5, page 1413, last paragraph). By doing so, they mix the response of two completely different systems within one measurement.

They describe that the soil of reference sites was prepared in that way, that the “top 10 mm in the reference collars were removed, refilled with soil, and then once flooded with distilled water” (BG 5, page 1413, second paragraph). By doing so, they strongly disturb the system, which definitely will react to this treatment – normally by increased respiration rates. This treatment was repeated every second month. I could not find any hint on measurements, that evaluate the effects of this treatment on the CO₂ gas exchange.

In the paper it is argued, that measurements of individual BSC and soil samples were compared to results obtained from the differential mode (BG 5, page 1414, left paragraph). It is remarked that “based on 10-min averages of consecutive measurements of dry and wet samples during nights and over-casted days, a difference of 0.1 $\mu\text{mol}/\text{mol}^{-1}$ was not significant”. It is, however, not explained, how the values taken on

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sunny days correspond to each other.

In my opinion, the combined measurement of the CO₂-fluxes of BSC and disturbed soil comprises two complex mechanisms, which could not reliably be separated. Therefore, I do not see how the fluxes of BSC can be modelled based on these data.

2. Implementation of gas exchange measurements

The authors write, that “the general enclosure time of a sample was 15 min” (BG 5, page 1414, left paragraph). In doing so, they crudely disobey a possible heating effect within the cuvette. In the results section (BG 5, page 1415, 4th paragraph) they mention air temperature measurements inside the enclosure (which cannot be found in the methods section) but it seems that the cuvette temperature was not really monitored during the experiments.

During the last decades, heating of the cuvette was recognized as one main problem in gas exchange measurements, since both photosynthesis and respiration are strongly temperature dependent. Gas exchange systems measuring in a differential mode have therefore been optimized to allow measurements within 1 to 2 minutes. In long-term measurement systems like the Klapp-Kuvette (Lange et al., 1997) the chamber has to be closed for 3.5 min which has been recognized as the upper time limit that is possible without obvious heating effects. If, however, the cuvette is closed for 15 min, temperatures within the cuvette must increase substantially above the surrounding values (at least on sunny days) and the physiological activity of the organisms does not at all reflect that of the surrounding BSC.

The following comments refer to the newly submitted paper.

3. Outline of methods

The methodology used in this paper to model the annual carbon fluxes is not presented in a clear and comprehensible way. On page 7301, last sentence, it is written that “carbon deposition was (1) extrapolated from estimated activity periods and mean net

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exchange rates, and (2) simulated using a precipitation-driven activity model (PdAM)". Are these two alternative methods with different results or is the second part based on the first? This doesn't really become clear to the reader.

4. Extrapolation of carbon deposition from estimated activity periods and mean net exchange rates

In chapter 3.1, the authors write that they determined mean daytime net CO₂ deposition and night respiratory CO₂ emission. The mathematical approach, however, is not explained.

These mean values are then multiplied by a total activity period, which is calculated from rain, humidity and soil moisture data. It is assumed, that certain precipitation amounts keep the soil moist for a fixed time-span. However, it is well-known that this time-span is strongly depending on site-specific factors like temperature, light intensity and soil-conditions. If the plan is to define mean time-spans of wetness, this has to be examined experimentally in detail rather than picking some more or less arbitrary time-spans.

Beyond that, photosynthesis and respiration rates of BSC components not only depend on the presence or absence of water. The amount of water also affects photosynthetic assimilation rates, since suprasaturation impedes the gas exchange of BSC. As shown in multiple studies –many of them are even cited in the introduction of this paper- the physiological activity of the crust organisms is also controlled by light intensity and temperature. None of these factors are considered in this chapter, making the calculations of exchange rates more or less meaningless.

5. PdAM simulation

The precipitation driven activity model of the authors consists of three parts: The activation switch, the trigger and the modulation.

The activation switch corresponds to the determination of the activity period, as defined

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Temperature [°C]	Solar insolation [W m ⁻²]					
	5	10	20	40	80	700
5	-0.98402283	-1.99567482	-4.01122919	-7.98145074	-15.4677236	-49.5428624
15	4.97307038	9.70975715	19.146846	37.7359418	72.7876491	232.331899
30	3.03917494	5.68612524	10.9597492	21.3476882	40.9352462	130.091596

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in their chapter 3.1 with its flaws as discussed under comment no. 4.

The trigger allows to accumulate switch signals to different levels of activation (page 7304, line 10). As I understand it, the level of activation is varied, depending of the amount and/or frequency of rain/humidity events. I do not understand at all, where these numbers were derived from. Is there any experimental design behind these numbers or are they just picked more or less randomly to best fit the existing data?

In the modulation part, an algorithm is used to depict the temperature and light dependency of net CO₂. This algorithm is treated in the following comment.

6. The algorithm

If one has a closer look at the algorithm described on page 7313, one cannot resolve the equation in that way, that the units on the right hand side fit that of ΦCO_2 on the left hand side. Consequently, the equation cannot be correct in the way it is written now.

Testing the algorithm for a few temperature and solar insolation values, the following results were obtained for ΦCO_2 [$\mu\text{mol m}^{-2} \text{s}^{-1}$], assuming fully activated crusts ($Z = 1$).

Looking at the effect of different temperature values, one sees, that at high temperatures lower net CO₂ fluxes are obtained, which is reasonable. At 5°C, however, net CO₂ fluxes reach negative values independently from solar insolation. This can be seriously doubted and is contradictory to existing literature values. Looking at the effect

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of solar insolation at one temperature, one observes an almost linear relationship. It is, however, well known that net CO₂ flux plotted against solar insolation always yields a saturation curve. At very high light intensities, net CO₂ flux may even decline again due to damage of the photosynthetic system.

At 700 W m⁻², a typical insolation on mid-Europaeen clear summer days, the results are just not explainable anymore.

7. Statistical methodology

The authors use only 10 out of 23 field campaigns explaining that "the other 13 field campaigns contained no new information and had only low fluxes with almost no variation to test the simulation" (page 7303, second paragraph). In the following study, the same 10 field campaigns are used both to establish the model and then to also test it. This method is completely inappropriate!

If a model is elaborated on the basis of field data and afterwards should be tested or validated, the existing field data have to be SPLIT RANDOMLY in two parts. One half of the field data is used to establish the model, the second half is only used for a validation of the model, not for its establishment. If the same field data are used for establishment and validation of a model, the statistical results are just meaningless.

8. Figure 1

In the methods section (page 7300, second paragraph) as well as in figure 1, the authors speak about 3-day field campaigns. However, in each campaign, field data are only drawn for less than two days (about 38 hours). It does not become clear, why 3-day time-spans are drawn and mentioned if field data only exist for less than two days.

If one compares figure 1 with fig. 3 in Wilske et al. 2008, it becomes clear that fig. 1a-e correspond to figure 3a-e in the paper published in 2008. In the new paper, the mean values of 10-min measurements are drawn, while in the previous one, the means

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with 90 % confidence levels were shown. A close comparison, e.g. of fig. 1a with fig. 3a gives the impression, that on November 22 around 2:30 pm there were some gas exchange measurement points in fig. 3a, which are not shown in fig. 1a. If one compares figs. 1d and 3d, one finds about 10 measurements on January 14 between 1 to 4 pm in fig.3d, whereas in fig. 1d only three circles are drawn. In figure 3e around midnight between January 20 and 21 there are many measuring points that could not be found in figure 1e. Are these just optical illusions or where are these measurements in figure 1?

9. Conclusions

Reading the conclusions (page 7312), one learns that the authors seem to not really believe in their results, either. They conclude in first place, that sensitive moisture-indicating equipment has to be developed to “replace or simplify the activation switch”. Their second conclusion is, that this new method will then allow “relatively precise assessments of BSC-related carbon deposition”. Does that mean that they also estimate their proposed method as inaccurate or even inappropriate?

Technical corrections:

Page 7297, line 19: Beymer instead of Beymar; also wrong in the literature list

Page 7313, line 10: T_M has to have the unit $^{\circ}\text{C}$; otherwise it cannot be subtracted from T_{SSB} .

Literature cited:

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