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

B. Wilske et al.

We thank the referee for the comments and appreciate that the referee acknowledges the tackling of an important scientific issue, i.e., the effort to model BSC related fluxes to assess their potential contribution to the ecosystem and/or larger scale carbon fluxes. In contrast to the referee, we think our results contribute substantially to the recent discussion on the contribution of BSC in semiarid/arid ecosystems (Wohlfahrt et al., 2008; Stone, 2008; Elbert et al., 2009). The field work was performed to inform about the driving forces for BSC activation in a semiarid system. The data were incorporated into a relatively simple model that can summarize the bulk of the physiological activity under natural conditions and the potential CO_2 sink strength. Below we respond to the specific points.

Anonymous Referee #1: 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). 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 defficiencies and major mistakes, that make their approach and the resulting deliverables extremely questionable.

Authors reply: We like to emphasize that the model aims at substituting for long-term flux measurements, however, the model explicitly requires data input from microclimate / climate records. Specifically, the modeling emphasizes that microclimate records, such as are available at LTER sites, can be used to assess BSC related carbon fluxes. Furthermore, we think we can rebut or correct some of the deficiencies that the referee found in our paper.

We regard the papers by Wohlfahrt et al. (2008) and Elbert et al. (2009) as valuable contributions to the recent discussions as well as demonstrations of our need for more data. Based on our observations (see previous paper), PdAM incorporates a clear dependence on rain (and fog), and hence allows an estimate of the bulk CO_2 flux to indicate the significance of BSC as CO_2 sinks. Interestingly, the estimate fits perfectly into the overview published by Elbert et al. (2009).

Anonymous Referee #1: 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 destilled 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.

Authors reply: At the time we conducted the study, we could not find bare soil area the size of an enclosure collar that could have provided ground for a comparison between changed and unchanged soil surface. Exchange of the upper 10 mm (and 5 mm during subsequent procedures) was necessary to remove any possibility of BSC development within the samples. The upper 10 mm were removed when the reference collars where set in. Only the upper 5 mm (i.e., ca. 8 cm^3 of soil) were exchanged at the end of a field campaign every two month. The subsequent measurement campaign was at least two weeks later. Although it is obvious to ask the critical question about the influence of the treatment to obtain reference samples, we think we have made our statements concerning this point in our previous paper (Wilske et al., 2008):

(1) The larger number of BSC-related CO_2 fluxes were obtained in direct differential measurements between a BSC- and a soil enclosure. However, as described in the BG 5 paper, BSC and soil samples were measured intermittently as separate samples, i.e., versus an empty enclosure and/or versus the IRGA reference channel flushed with ambient air purged through the chemical cartridges. We found that the measured soil CO_2 flux was comparable to values reported by other studies of dryland soils in Mediterranean climate (BG 5, page 1420, section 4.1, last paragraph; referred references: Maestre and Cortina, 2003; Tang et al., 2003).

(2) If there would have been a significant effect from this treatment, this would have been observed in the soil collar measurements, decaying with time over subsequent measurement campaigns. No such responses were observed, indicating any effects of the treatment were minor and transitory and confined to the period after the treatment (at the end of a measurement campaign) and had disappeared by the next measurement campaign.

(3) We compared the in-situ BSC-related CO_2 flux with values from a previous laboratory study (Zaady et al., 2000) that used samples from the same site, and found that individual values of net flux were very similar (BG 5, page 1420 section 4.1, second paragraph). Particularly the latter comparison suggests that, although fluxes from treated and untreated soil may be different, the differences were not large enough to significantly change the net signal of BSC

related CO_2 fluxes. We considered (prior to action) two reasons for a relative low response to the disturbance introduced by the treatment. (1) The treatment of the upper 10 mm does not open up new or previously buried sources of organic matter for aerobic respiration, which is the usual source of increased soil CO_2 efflux following disturbance. (2) Carbon content and structure of the soil column remains the same below the treatment. Flooding and subsequent drying of the exchanged surface layer recovers a resistance to gas transport - not equal but - not too different from the original soil layer such that it would affect our measurements significantly.

Anonymous Referee #1: Specific comments (continued): 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 µmol/mol^{-1} was not significant". It is, however, not explained, how the values taken on sunny days correspond to each other.

Authors reply: The test required similar conditions during the consecutive measurements of BSC versus empty enclosure, soil versus empty enclosure, and BSC versus soil. Conditions during sunny periods when BSC were active were fluctuating and did not allow a comparison of both types of measurements within a short enough time frame to enable reliable comparisons. However, as the technique was a test of the accuracy of the measurement method, and is independent of the magnitude of the fluxes, we believe that the fact that the measurements proved reliable under low light conditions means they were also reliable under high light.

Anonymous Referee #1: Specific comments (continued): 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.

Authors reply: We like to emphasize that the comparison was not between disturbed and undisturbed soil. The bulk of the soil was undisturbed and only the upper layer with BSC was removed, hence enabling us to separate the soil component from the combined BSC+soil flux. We have replied to this context in our statements above, and we are confident that the data obtained with our technique provide a proper basis to model the bulk of BSC-related CO_2 fluxes.

Anonymous Referee #1: Specific comments to the previous publication: **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.

Authors reply: The temperature inside and outside the enclosure was monitored at about the same level of 2 cm above the soil surface. The temperature sensors inside the enclosure were detailed in BG 5, page 1413 section 2.3.2. The enclosure system. In view of the importance of temperature, we suggest to start the description of microclimate measurements in the present paper (page 7301, line 1) with "Air temperature and humidity inside the enclosures were monitored using a thermocouple (Type E, Campbell, Loughborough, UK) and a thermo-hygro-meter (HM122, Vaisalla, Helsinki, FI). {continuing as is:} Photosynthetically active radiation …"

Anonymous Referee #1: Specific comments to the previous publication: **2. Implementation of gas exchange measurements** (continued)**:** 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.

Authors reply: We did not discount a possible heating effect. In fact, the studies by Lange and colleagues reporting the temperature sensitivity of the CO_2 exchange of BSC provided the guidelines for the manual operation of our field measurements. 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). This means that we increased the number of enclosure rotations during rain events, day time, and higher insolation, whenever BSC were moist. The shortest enclosure rotation was 7 min. Short period measurements were combined based on 1-min data logging. The measurements were very manpower intensive during day periods when BSC were active. However, we did not pay the same attention under dry conditions when BSC were inactive, and the enclosure temperature increased at times up to 9°C above the ambient temperature; however, this will not have a bearing on the results, as there was no activity in these periods. The temperature difference between inside and outside the enclosure was between 1−2°C degrees during nights and only in a few cases larger than 5°C degrees during daytime periods when BSC were active. Thus, there is no disagreement between the referee and the authors in that shorter enclosure periods are required to avoid overheating of samples. However, the 3.5 min enclosure period of the Klapp-cuvette may not be the absolute optimum for all cuvette systems. For example, there can be differences in the heat capacity of the enclosure material, and the flush to volume ratio.

According to Lange et al. (1997), the Klapp-cuvette consists of a Plexiglas cylinder with a volume of 190 cm^3 and runs with a flush of 500 ml min^{-1}. Our enclosure system was built using a Plexiglas cylinder with Teflon foil on top, and had a higher flush to volume ratio ((3.142 x 7.15 cm x 4.5 cm) ~ 101 cm^3, flush 1000 ml min^{-1})

Anonymous Referee #1: Specific comments to the present BGD article: **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 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.

Authors reply: The two approaches are independent of each other. We thank the referee for his comment that gives us the opportunity to rephrase in order to provide better readability.

Anonymous Referee #1: Specific comments to the present BGD article: **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.

Authors reply: This analysis was provided as a first-order estimate of BSC data that may be obtained from field estimates where some data is available, but detailed climate data might not, and as a test of the sensitivity of the more process-driven full model. It is also similar to the approach used by Elbert et al. (2009). However, we do consider removing completely this 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.

With respect to the remarks of the referee: (1) The study presents site-specific results, both in terms of BSC-related CO_2 fluxes, moisture conditions and timing of precipitation events. (2) We see that our description of the calculation is insufficient. Mean exchange rates were calculated as the mean of measured flux 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 to accumulate through the 15-min moisture records to the annual total.

(3) Lange et al. (2006; 1990) calculated the annual carbon gain of lichens in the Namib Desert based on 16 days in the field, the diurnal courses obtained by linear interpolation between individual periods of measurements, and (literature-derived) lower and higher numbers of foggy days. We regarded the assessment as relatively reliable because the predominant water supply allows a correlation with distinct time windows of activity that depends almost exclusively on fog (BGD 6, pages 7298, lines 17−20). The estimate remains the best available for the BSC type and area, even though probably not all southern hemisphere spring and autumn days showed

the exact light and temperature regime as the days investigated by the study. We considered the moisture availability at ILTER-SSK to be more complex, and hence, considered that the detailed moisture records in combination with average exchange rates for active day and night conditions must be more accurate than an average carbon exchange per day.

Anonymous Referee #1: Specific comments to the present BGD article: **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 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?

Authors reply: We are not sure whether the referee refers to the switch levels or the periods of activation. It is well established that the activity (rates and duration) of poikilohydric organisms and response to temperature and light are moisture-dependent. We have characterized these responses for intact BSC *in situ* for the first time, based on observed responses over the full range of moisture conditions and hydrological drivers typically inducing BSC activity in this environment (see BGD 6, page 7320 Table 2). In Table 1 of BG 5 (page 1417), we showed the length of measured activity periods in relation with water availability from different sources. In Table 1 of BGD 6 (page 7319), we summarized the range of periods with BSC activity that were observed during field campaigns and complemented by climate records regarding PPT, RH, and soil moisture. It is obvious that each mm of PPT may result in a wide range of different activity periods depending on temperature, radiation, and the antecedent soil moisture. Hence, the setting of resulting activity periods for certain ranges of PPT was only the first step to a finer

calibration. The range of resulting activity periods was then dissected by controlling feedback. We used records of soil moisture and relative air humidity as the controlling feedback mechanisms to extend or shorten resulting activity periods. RH had two advantages over net radiation, which is usually applied to drive evapotranspiration (and may be used to simulate desiccation): (1) especially in arid climates, RH is usually negatively correlated with radiation and temperature. (2) Using RH, we avoided interference with radiation and temperature inputs to the algorithm. Subsequent to having the feedback mechanisms in place, we run the model with various PPT-BSC activity relations to compare measured and model results.

Anonymous Referee #1: Specific comments to the present BGD article: **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 _CO2 on the left hand side. Consequently, the equation cannot be correct in the way it is written now.

Authors reply: We are grateful for the referee's attention and apologize to the readers of BGD. The equation presented in BGD 6 is incorrect at two positions. Firstly, the activation switch Z is repeated as a factor to drive temperature related dark respiration in the BSC-soil continuum. Secondly, the denominator in term 3 should start with 1+exp(bracket content) instead of 1-exp(bracket content). The equation used to model BSC-related CO_2 fluxes should read:

$$\Phi_{CO_2} = Z \times \left(\begin{array}{l} \left(Z \times \Phi r_{CO_2} \times \exp\left(\frac{cTp}{R} \times \left(\frac{T_{SSB} - T_S}{T_{SSB} \times T_S} \right) \right) \right) + \\ \left(\Phi l_{CO_2} \times c_L \times \left(\frac{\alpha \times L}{\sqrt{1 + \alpha^2 \times L^2}} \right) \times \frac{\exp\left(\frac{cT_1}{R} \times \left(\frac{T_{SSB} - T_S}{T_{SSB} \times T_S} \right) \right)}{1 + \exp\left(\frac{cT_2}{R} \times \left(\frac{T_{SSB} - T_M}{T_{SSB} \times T_S} \right) \right)} \right) \end{array} \right) \qquad \text{E1}$$

Apart from these mistakes, we do not understand, why and where the referee does not see the equation showing fluxes at both left and right hand of the equation. With two exceptions, the equation is unchanged from Schuh et al. (1997): We introduced Z, which is a dimensionless

factor. We use the term for light-regulated exchange without the square of the part in brackets, and thereby return to a version previously published by Guenther et al (1993). With respect to the referee comment that T_M has to have the unit °C in order to enable subtraction from T_{SSB}, we speculate that the referee similarly refers to cTp over R in the dark respiration (2nd term). Schuh et al. (1997) write "cTP is an empirical constant that, to a first approximation, is similar to the enthalpy of vaporization for the VOC under consideration". Thus, we can assign the unit J mol^{-1} K^{-1} to cTp and thereby eliminate the unit of R, if this is the problem the referee refers to.

Anonymous Referee #1: Specific comments to the present BGD article: **6. The algorithm** (continued)**:** Testing the algorithm for a few temperature and solar insulation values, the following results were obtained for _CO2 [_mol m 2 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 insulation. This can be seriously doubted and is contradictory to existing literature values. Looking at the effect of solar insulation at one temperature, one observes an almost linear relationship. It is, however, well known that net CO2 flux plotted against solar insulation 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 insulation on mid-Europaean clear summer days, the results are just not explainable anymore.

Authors reply: We have already discussed the typographical errors in the equation. Similar to other flux studies (e.g., Wohlfart et al., 2008), in this study negative fluxes represent net CO_2 deposition due to CO_2 uptake by BSC photosynthesis, and positive fluxes represent net emission due to BSC respiration (BGD 6, page 7300, lines 19-20). Using the corrected equation, we like to show in our reply results for the ranges of temperature and insolation the

referee used in his tables (Figure 1). The figure shows what the model can and cannot simulate. The model is able to operate in a certain window of conditions that have been observed during the study. The model simulates well (1) the moisture and temperature dependent dark respiration, (2) low light compensations points that increase with temperature, (3) significant negative fluxes (net uptake rates) at lower temperatures (5-10°C) and (3) the moisture, light and temperature dependent increase of net deposition (photosynthetic uptake) up to a temperature of 25°C. We did not measure significant activity above this temperature that would allow training the model, because BSC were usually dry or dried out quickly. (4) PdAM simulates desiccation through decreasing moisture levels. The model cannot simulate (1) fluxes at temperatures at and below 0°C; (2) the slight overshoot into net respiration at the end of desiccation and the resaturation bursts following rewetting under high temperatures.

Anonymous Referee #1: Specific comments to the present BGD article: **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.

Authors reply: We understand the concern of the referee. Indeed, we had aimed at using only data of one year for model development. However, testing the outcome with data of the second year urged a few adjustments, as a result of which we cannot claim to have validated the model using independent data. The statistical results are not completely meaningless. Although they

do not constitute a validation of the model, they present how far the model attains results that are in agreement with measurements. The aim of this study was to provide estimates of annual BSC-related carbon flux through applying measurement derived flux-climate relationships to similar climatic periods without measurement data. Improving the accuracy of these empirical relationships improves confidence in the estimates from non-measured periods, as is used in many flux studies (e.g. chamber up-scaling and eddy-flux gap-filling), particularly where there remains insufficient information to develop process models. This is the first study to provide BSC-related ecosystem flux values in the context of these studies from seasonal in situ measurements with this type of BSC, and it remains a crucial area of research to understand the long-term and geographical variation in BSC-related flux behaviour.

Anonymous Referee #1: Specific comments to the present BGD article: **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.

Authors reply: It should be acknowledged by the referee that field measurements need substantial time periods of preparations and never deliver data for the whole time invested. In our case, each field trip included three days. Day 1 included mainly set up and extensive checks of the measurement system. Dismantling of the setup started usually the afternoon of day 3.

Anonymous Referee #1: Specific comments to the present BGD article: **8. Figure 1** (continued): 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 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?

Authors reply: Not all data points in Figure 1 in BGD 6 are exactly the same as those shown in Figure 3a in BG 5. Data had to be checked and partly aligned to the 15-min time window of the TEMS climate records. Hence, some data at the junction of two 15-min intervals had to be either reprocessed or removed. For example, some measurements included shorter periods of enclosure rotation to confirm rapid changes in fluxes, e.g., due to the desiccation of BSC. As data were logged in one-minute averages, we could reprocess and integrate most data where necessary. However, some shorter measurement periods (e.g., on January 14 between 1 to 4 pm) would not represent a mean for the 15-min climate data windows that are presented in Figure 1. Apart from the reprocessing, a few data points were eliminated due to missing climate data required for model development. The nighttime measurements around midnight between January 20 and 21, to which the referee refers, lacked soil surface temperature due to cut thermocouples.

Anonymous Referee #1: Specific comments to the present BGD article: **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 inacurate or even inappropriate?

Authors reply: In response to the referee comment, we suggest to change the leading and subsequent sentences of our conclusion and write:" We regard the accuracy of the measured and modeled fluxes presented in this study sufficient to assess the bulk of BSC-related carbon deposition within the investigated ecosystem. Taken together with results from the study of Elbert et al. (2009), estimates of net BSC carbon uptake rates in arid and semi-arid regions now range between ~10 and 160 kg C m^{-2} a^{-1}. The observed inter-annual variability in the BSC-related carbon fluxes suggests that it requires more long term research to understand the contribution of BSC to the carbon dynamics within dryland ecosystems. A potential contribution of up to 7% to the NPP of dryland ecosystems may be regarded as one of the smaller missing carbon sinks within the terrestrial biomes. However, NPP represents only a part of the ecosystem carbon budget. To derive a more comprehensive metric, such as net biome exchange, would require information on the fate of BSC carbon, which is not available at this time.

The PdAM model was quite successful in simulating the changes in the amplitude of daily CO_2 fluxes. However, the activation switch represents a rough mechanism that can be improved with sensitive moisture-sensing equipment monitoring moisture at the soil surface and within the top one centimeter of soil and/or BSC. With improved soil surface moisture data, and validation through data from other sites, PdAM may provide a reliable basis to develop consistent assessments of BSC-related carbon deposition in many dryland ecosystems."

Anonymous Referee #1: Specific comments to the present BGD article: **Technical corrections:** Page 7297, line 19: Beymer instead of Beymar; also wrong in the literature list Page 7313, line 10: TM has to have the unit _C; otherwise it cannot be subtracted from TSSB.

Authors reply: We thank the referee for the two technical corrections, which will be changed in the revised manuscript.

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Figure 1
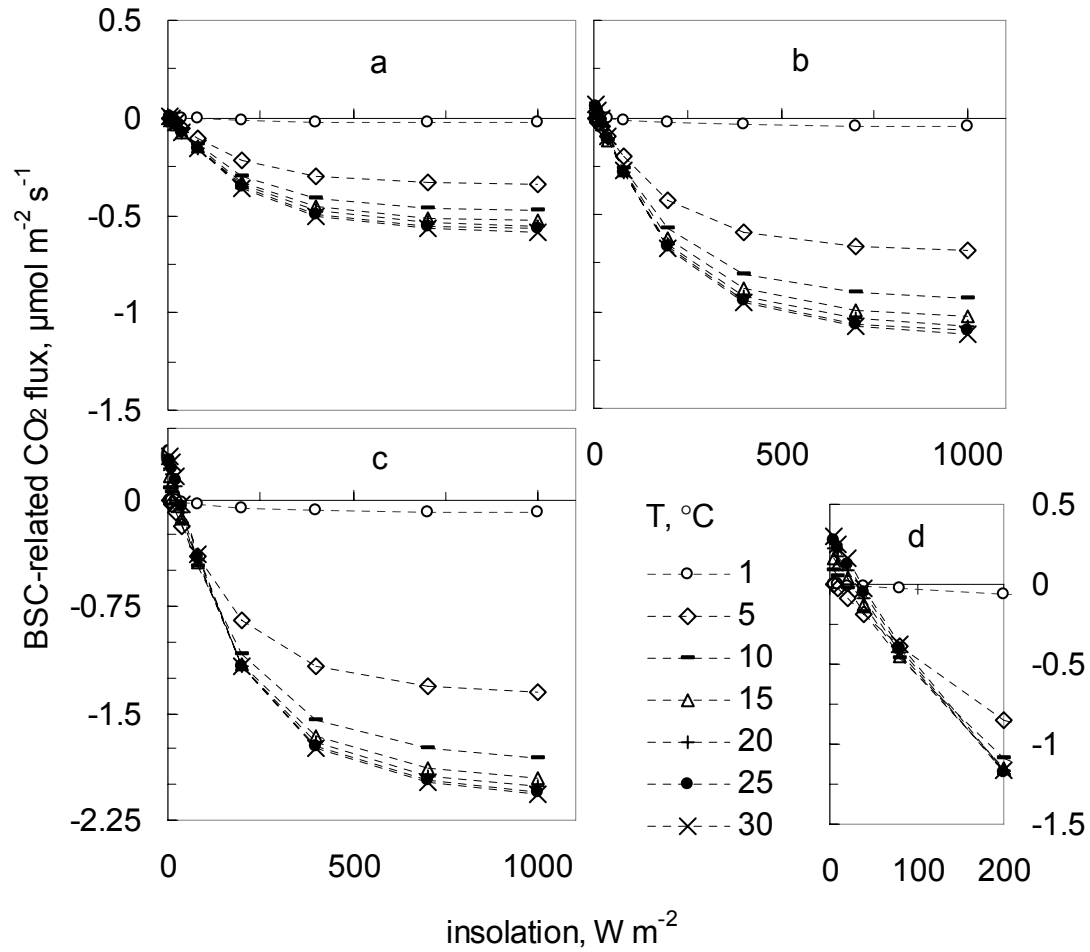
Modeled response of BSC-related CO_2 fluxes to insolation, temperature, and increasing water availability (figure a to c; Z: a=0.25, b=0.5, c=1). Figure 1d shows the model simulating that higher temperatures require higher light conditions to pass the compensation point between net emission (respiration) and net deposition (net uptake by BSC).