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## ***Interactive comment on* “Biological soil crusts on initial soils: organic carbon dynamics and chemistry under temperate climatic conditions” by A. Dümig et al.**

**Anonymous Referee #3**

Received and published: 20 February 2013

I read with interest this contribution, which presents results from a study aiming to i) determine the amount of OC fixation by biological soil crusts (BSCs) and the BSC-derived OC input into the underlying substrates; ii) characterize the chemical composition of OC in BSCs and substrates by applying solid-state  $^{13}\text{C}$  NMR spectroscopy and analyses of the carbohydrate signature, and iii) assess the OC dynamic and timescales of BSC establishment. This work deals with a topic very poorly studied by the BSC community, and as such is a welcome contribution that helps to understand the role of these important organisms on the C cycle of those ecosystems undergoing initial development.

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The manuscript was an enjoyable read, and was well written for the most of it, albeit I found some typos and minor grammatical errors. Thus, I would advice the authors to have their manuscript thoroughly revised by a native English speaker. A reduction in the length of the ms would also be desirable to increase its impact, as some sections of the ms, particularly the results and discussion, are very long and are somewhat difficult to follow.

I did not found major experimental or analytical errors, but since I am not an expert on the use of  $^{14}\text{C}$  and NMR spectroscopy, I may miss some important details that need revision.

Despite the inherent value of the manuscript and the potential relevance of the results presented, the manuscript has a fatal flaw, which is the lack of replication of the transects surveyed in the natural and artificial dunes. The “replicates” from each location within the transect are indeed pseudoreplicates. As a result the manuscript is very descriptive, the authors cannot present formal statistical analyses and the results cannot be properly compared and generalized. I have no problems with descriptive studies, as I think that information such as that presented here is very valuable and will likely foster future research on the topic, but certainly a manuscript lacking proper replication is not suitable for a leading journal like BG. Do the authors have additional data collected from other dunes/independent transects within each dune? If so they must be included, so the data can be properly analyzed. If not, and unless the authors can provide a sound justification for the lack of replication, I am afraid that the data collected cannot certainly allow the authors to achieve the objectives of their study.

Other minor issues are described below:

1) The abstract is very long and includes too many results. Please select those that are more relevant and/or interesting and highlight them there. 2) I think that there needs to be more of a “punch” in the final sentence of the abstract. I would advice the authors to add a sentence to highlight the new insights and advancements over previous knowl-

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edge this work provides. 3) Line 61: Add some references to support this statement. 4) Line 76: BSC-dominated microsites are also an important contributor to total soil CO<sub>2</sub> efflux (e.g. Castillo-Monroy et al. 2011, Thomas 2012). 5) Lines 123-127: Please indicate the date of sampling. 6) Lines 258-259: Do not repeat in the results data that are already present in tables and/or figures. 7) Line 419: Please update these values with the more recent Elbert et al. (2012). 8) The discussion should be shortened and improved, as it is quite “plane” and descriptive, and there are not many clues on why the results reported are relevant and interesting for a leading journal such as BG. 9) Lines 407-421: The authors discuss that “the net contribution of BSCs to the SOM pool is difficult to assess as net photosynthesis takes place for only short periods after sufficient surface wetting due to rain or dewfall events, whereas carbon losses through respiration can be large during other periods”. Does this mean that the approach used here can provide more insights or precise estimates than those reported by previous studies that conducted in situ measurements of net CO<sub>2</sub> exchange? This issue is not properly discussed, and the authors could clearly discuss what are the pros and cons of the approach they used compared to what others have done before. 10) Lines 562-568: This pattern is also commonly found in grasslands from Spain (e.g. Maestre et al. 2002, Castillo-Monroy et al. 2011). 11) The conclusions section is too long. Please encapsulate the main take-home messages in a few sentences. 12) Check the reference list, I found some errors in the years of some references and in the way the authors spell the abbreviated name of the journals. 13) The x axis of Figs 2-4 is not very informative, and no guidelines are provided in the caption on the meaning of 1-5. Please check this and rewrite the caption and/or the label of the axis, so readers can fully understand the figure.

## References

Castillo-Monroy, A. P., F. T. Maestre, A. Rey, S. Soliveres & P. García-Palacios. 2011. Biological soil crusts are the main contributor to soil CO<sub>2</sub> efflux and modulate its spatio-temporal variability in a semi-arid ecosystem. *Ecosystems* 14: 835–847.

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Maestre, F. T., M. T. Huesca, E. Zaady, S. Bautista & J. Cortina. 2002. Infiltration, penetration resistance and microphytic crust composition in contrasted microsites within a Mediterranean semi-arid steppe. *Soil Biology & Biochemistry* 34: 895-898

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10, C114–C117, 2013

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