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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.

Anonymous Referee #2

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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.

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. 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 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.

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? 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. 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.

Despite my concerns about the conclusions, the discussion is well-written and informative, as is the introduction.

Specific comments:

Pg 7296 line 25: (Stone 2008) – this is not peer reviewed, it's a news article

Pg 7299, line 16: your grammar is incorrect

7301, line 1: photosynthetically active radiation?

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

The description of the modeling on pgs 7304 is not especially clear.

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

Technical corrections:

"insolation", not "insulation", is the correct word as applied to solar radiation – the latter

refers to thermal diffusivity or electrical properties

Interactive comment on Biogeosciences Discuss., 6, 7295, 2009.