Biogeosciences Discuss., 10, C563–C565, 2013
www.biogeosciences-discuss.net/10/C563/2013/

Biogeosciences Discussions

Interactive comment on "Atmospheric turbulence triggers pronounced diel pattern in karst carbonate geochemistry" *by* M. Roland et al.

Anonymous Referee #1

Received and published: 27 March 2013

Review of the manuscript "Atmospheric turbulence triggers pronounced diel pattern in karst carbonate geocehmistry" by Roland et al., submitted to Biogeosciences

General comment:

This paper presents an interesting study of CO2 exchange between carbonaceous systems and the atmosphere induced by turbulent soil ventilation. A modified version of the WITCH carbonate weathering box-model is used to simulate CO2 exchange due to soil ventilation. The results agree well with field data from south-eastern well showing that the suggested process may, indeed, play an important role in Mediterranean karst areas.

The paper is well written and deserves publication in Biogeosciences. Below, I list

some comments, which may further improve the paper.

p. 5, 1st paragraph: "In dry conditions, ..." It is obvious that water vapour condensation or adsorption during the night may compensate daytime water loss, but it is not straightforward that the CO2 lost during the day is compensated as well. Thus, the authors should add a few sentences discussing the sources of CO2 resulting in "undersaturation of DIC" as stated in the paper. The discussion of the sources of CO2 is given at the end of the paper, but the reader may ask this question here already.

p. 5, last paragraph: "The term further ..." "further" is not used in the previous sentence, so this sentence does not make sense.

p. 6, 2nd paragraph: "... induce a strong carbonate disequilibrium ..." Really? What is the magnitude of this disequilibrium, e.g. the corresponding saturation index? What are the corresponding pCO2 values in the soil? If these values are available from the modelling, they should be given in the paper. This is important additional information, which is of great interest for the reader.

p. 7, Eq. (2), ff.: As far as I understand, all results presented in the paper strongly depend on the parameters used to prescribe the CO2 efflux (i.e., turbulent soil ventilation). Thus, a more detailed discussion of the individual parameters would be good. Which value has been used for modelling? How do these values compare with the values measured in the field? What is the influence of the different parameters on the final results? This is even more important as the authors themselves mention that the ventilation equation cannot be fully validated.

Line 131: "scenario's" should be "scenarios"

p. 10, 1st paragraph: "Note that ambiguity exists ..." This is important, in particular because carbonate precipitation is normally considered as a sink in carbon sequestration studies. Thus, I would clarify this earlier in the paper.

Line 167: "Model outcomes confirmed ..." Considering the uncertainties in Eq. (2) and the parameters themselves, it may be better to write "model outcomes suggest ..."

p. 13, 2nd paragraph: "Also, the observed CO2 concentrations in both the upper and deeper layers could only be simulated well with our model by including a very small but continuous production of CO2 in the deeper layers." This information, which seems to be essential for the whole modelling, must be provided earlier in the manuscript.

p. 14, last paragraph: As I understand, the suggested process and modelling does only work in semi-arid areas. This important information should be provided in the abstract.

Interactive comment on Biogeosciences Discuss., 10, 1207, 2013.