Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-223-RC1, 2017
© Author(s) 2017. This work is distributed under
the Creative Commons Attribution 4.0 License.

Geoscientific Model Development Discussions
Open Access

Interactive comment on "Modelling soil CO_2 production and transport with dynamic source and diffusion terms: Testing the steady-state assumption using DETECT v1.0" *by* Edmund Ryan et al.

F. Moyano (Referee)

fmoyano@uni-goettingen.de

Received and published: 20 October 2017

Manuscript Review

Modeling soil CO2 production and transport with dynamic source and diffusion terms: Testing the steady-state assumption using DETECT v1.0

General comments

The manuscript describes a modeling study with the main objective of determining the

Printer-friendly version

Discussion paper

CC BY

significance of non-steady states for determining and understanding soil respiration fluxes.

The paper is well written, with a logical structure and clear sentences. Apart from some minor comments, I find the abstract correctly describes the study. The introduction is also complete and informative. The same is valid for the methods, which require a detailed description given the amount of equations and assumptions used. Overall, the study succeeds as posing a defined set of questions and methods that are then used to obtain the results. By making the data and model code available the authors make a valuable contribution to the community.

The study is valid and provides some informative results as it is. However, the conclusions could be stronger with a slightly different focus. This considered, the below can be taken as suggestions for improvement unless a direct question or concern is stated.

Generally, the study could focus more on the specific question posed, i.e. when are NSS conditions relevant. It could discuss less the scenario comparisons not related to this, which make the article longer than required, since they are affected by a number of factors that are not analyzed properly. For example, some discussions on the response of Rsoil that are due to the source part of the model (SK) require a more detailed analysis of the functions used and could be left out. This includes precipitation effects not related to CO2 transport (as I comment below). On the other hand, a closer look at how concentrations change in soils, the amounts of air-filled pore-space and how much/fast CO2 is displaced upon wetting would be a nice addition.

Since the NSS and SS models do not differ in the production or source of CO2, the only difference should be where this CO2 remains after being produced. So it would be very informative to include the storage state variable, i.e. how much CO2 is in the soil. The total (Rsoil + storage) should be equal for both models (otherwise there is a mass-balance problem, as there is no other output flux for CO2). This also makes clearer that a NSS is always a temporal condition, so any difference (at daily or seasonal scales)

CC BY

should be explained by changes in storage.

Because changes in CO_2 storage can affect the net Rsoil, initial conditions that lead to a change in storage can affect the outcome. In that case it is better to get the model equilibrium to use as initial conditions instead of values fitted from data. Further questions and suggestions are given below as specific comments.

Specific comments (Numbers are for the page and line)

3/47 The term moreover here does not seem to connect the two sentences. The second does not add to the previous.

3/50-51 Integration time will surely also play a role, and NSS and SS differences will decrease for longer periods. Only a feedback of [CO_2] on respiration or as a flux of dissolved inorganic C to groundwater (neither modeled) would result in different accumulated long-term Rsoil.

4/4 A comparison with fossil fuels is misleading if not better clarified. Rsoil is part of the fast C cycle. Not necessarily a net addition of C.

5/22 The hypothesis that the Rsoil spike after re-wetting is caused by pores filling with water and displacing CO_2, is presented here, but not quite tested in the study.

10/15 How is Ψe(z) calculated? Is θsat(z) not the same as φT?

11/20 It is rather unusual to model the effects of volumetric moisture on respiration activity as an exponential function. This usually is an OK approximation only at the dry end of moisture content. Also strange is that when the θ and θant terms are 0 the function would equal 1. How does this make sense for a completely dry soil? There doesn't seem to be any information here or in the cited studies of why this function type was chosen (other than that it uses both current and antecedent inputs). Changes in the dynamics of soil moisture induced by modifying precipitation patterns will affect Rsoil largely as a result of the shape of this function. It's non-linear shape partly would explain why changing the frequency of precipitation with the same total amount would

Printer-friendly version

Discussion paper

CC BY

lead to different seasonal fluxes. The discussion of those differences should include this.

13/eq.7 Here is another function that directly affects respiration activity and is strongly non-linearly related to moisture, as it includes the multiplier $\theta 3$. As with the f(θ, θant) function, it changes Rsoil in response to changes in precipitation. This needs mentioning in the discussion.

14/4 'time-varying'

15/11 The expression is not an equality so it does not say how exactly Ndt is calculated.

15/eq.10 Would be nice to see this derived in the appendix.

16/9 Should actually cite the original derivation (by Cerling 1984)

16/eq.11 Since the only output is to the atmosphere, I'm guessing the depth terms are irrelevant and could be ignored in this model, unless the storage amount is of interest.

19/15 A reference for this procedure would be useful.

22/5 Parameter p probably has a strong impact on Rsoil. Uncertainties in this parameter would be informative.

22/10 Why without Cmic and CUE?

24 The paper makes texture a central point of the scenarios and discussion. However, the methods section did not make at all clear how texture affects the outcomes in the model. Presumably, texture is used in the HYDRUS model, thus affecting θ. Maybe also affecting eq.2 (but it was not specified how). Given the discussion related to texture, this should be made clearer.

26/1-2 The first sentence here is not clear. What effects?

32/3-23 This paragraph almost seems too out of topic. While the model could be used to explain some of the dynamics of post-wetting Rsoil, this does not seem to be the focus of the study. As commented above, these differences induced by changes in precipitation are strongly affected by the functions using θ, which are not really analyzed here. Since the paper is rather long, it would seem preferable to leave a more careful analysis of this topic for another paper.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-223, 2017.

Printer-friendly version

Discussion paper