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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.

Anonymous Referee #2

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In their manuscript, Ryan et al. study under which conditions soil CO2 production is in steady state with CO2 fluxes at the soil surface using a modelling approach, in which they focus on the effects of grain size and antecedent temperature and soil moisture conditions. Therefore, the authors present a new model of non-steady-state soil CO2 production (DETECT v1.0) and compare the model results with a simplified version of the model which assumes steady state conditions (no delay between sub-soil production of CO2 and CO2 the flux at the soil surface), by applying the model to an experimental site in Wyoming (PHACE).

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Discussion paper

The authors address some important questions: which environments factors control subsoil CO2 production and how can these processes be correctly simulated using a modelling approach. Overall, the manuscript is well-written and has a good structure. The abstract is informative and provides a good overview of the questions the authors address and a brief overview of the set-up of the study. The introduction gives an overview of the studied subject and existing knowledge, although it could be shortened in my opinion (see specific comments). The methodology provides a complete overview of the structure of the DETECT model and the equations it uses. At some points, however, some information is still missing (see specific comments). In the results section the authors present how they applied the model to assess the effect of different environmental factors supported by clear graphs. In the discussion section, in my opinion, the authors should focus more on the processes lying at the basis of their observations, such as the effect of soil moisture on microbial and root CO2 respiration (see specific comments). The fact that the authors provide the codes of their model together with a clear user manual increases the impact of their contribution.

Although I believe that this manuscript provides a valuable contribution to existing knowledge on how to model CO2 production in soils, I have some concerns and suggestions, as formulated below and in the specific comments.

A main concern is that most of the different amounts of modelled Rsoil between the scenarios arise from the effect that soil moisture has on the production of CO2 from both sources (roots and microbes), e.g. as shown in Figure 2 between days 220 and 240. The effect of soil moisture on CO2 production by both roots and microbes is regulated by equation 4a, which assumes an exponential relationship between θ and the amount of CO2 respiration. The conclusion that precipitation regime characteristics and/or including antecedent soil moisture and temperature conditions have an impact on the magnitude of the soil CO2 efflux (as formulated in the conclusion) is thus greatly affected by the use of eq. 4a. Using a different equation in which e.g. CO2 respiration rates decrease at very high soil moisture contents, might thus lead to

a different conclusion. E.g., using a soil moisture – respiration response function in which CO2 production is inhibited at very high soil moisture levels might lead to less CO2 respiration using NSS conditions. Therefore, I would encourage a more elaborate discussion (in addition to P33 L11-13) on the effect of this equation on your results or, better, an assessment of how including a different soil moisture - respiration response function affects the model results. Moreover, it should be more clearly explained how eq. 4a and 4b affect the produced CO2 by roots and microbes, so this is more easily understandable for the reader.

The authors state that a correct simulation of CO2 respiration in soils can improve modelling soil C processes. Therefore it would be interesting to assess the effect of the NSS vs SS approach on the total SOC pool: does the increase in CO2 respiration using the NSS conditions lead to substantially decreasing SOC pool, or is this effect limited? Or in other words, is a correct simulation (NSS vs SS) of CO2 respiration necessary in order to correctly model changes in the total SOC pool? Other suggestions and remarks are formulated in the specific comments below.

Specific comments

P 4 L17-18: in addition to delays due to CO2 transport times, is also something known about the effect on additional CO2 production (as this is one of the outcomes of the study)?

P5 L21: please clarify what you mean with 'displacement of CO2'

P6-7 L18-13: In my opinion, this detailed explanation of your set-up can be formulated much shorter here, as this is explained in detail in the methods section

P8 L6-16: this is mostly a repeat of the last paragraph of the introduction and can be removed

P8 L17 – P9 L2: If you want to shorten the manuscript I would remove this part, as this is also clear from the introduction and the rest of the methods section.

P 11 L 20: please provide a reference for this equation

P11 eq 3: It's not clear to me how you obtained the value for RRbase, can this be stated explicitly?

P13 L16-17: how were these different values for the constants obtained? Please provide a reference if appropriate

P14 L13-14: please provide the value for the atmospheric CO2 concentration that was used here.

P16 L17 – P17 L8: This paragraph belongs to the introduction, not to the materials and methods section.

P18 L8: please be more specific about the data that was created

P20 L9 – 22: It would be good if you could summarize the values of these parameters in a supplementary table, this would increase the readability and reduce the amount of text.

P21 L5 – 10: This can be removed in my opinion, this is also explained in the caption of the table

P21 L12 – 20: this is also explained in Appendix S1, this can be removed either in the text or in the appendix.

P22 L5 – 7: Here you state that you obtained a value for the parameter p as the ratio of Csol to Csom. However, in eq7 you state that you calculate Csol from the p parameter. This is rather confusing: is eq. 7 actually used in the model?

P22 L9: It is not clear how both parameters (Vbase and Km) were obtained through fitting the microbial respiration submodel to data. Please clarify. Also, why are Cmic and CUE left out?

P22 L16: please clarify how these values were adjusted.

P24 L6: I agree with the comment from reviewer 1 here: please clarify how texture affect the model outcomes.

P24 L15: please provide the amount of precipitation in 2009 here.

P25 L4 – 10: In my opinion, it's strange to already summarize the results before you have presented them, I would remove this paragraph as this is also clear from the rest of the results section

P 26 L7 – 8: the fact that Rsoil is larger when including the antecedent effect is likely to be a result of relationship between soil moisture and respiration (eq 4a), another formulation of this relationship could lead to a different results, see comment above.

P26 L9 – 11: You attribute the greater Rsoil to an increase in root respiration, while from Fig. 2 the increase in microbial respiration is even more significant and greatly contributes to the increase in total Rsoil. Why is this not mentioned in the text here?

P27 L11: I don't see how Fig. 3 shows that there is a greater root respiration.

P 27 L16 – 20: This formulation is confusing: in the first sentence you state that different precipitation scenarios led to little difference between Rsoil predicted using SS and NSS, while in the second sentence you state that precipitation regime affects the magnitude of Rsoil predicted by SS and NSS. Please re-formulate this.

P30 L6 – 8: from the data you show in the figures is seems like the difference in modelled Rsoil between SS and NSS at the timescale of a growing season is rather limited (e.g. the bars on the right side of Fig. 3), please clarify this. Also, in Fig. 3e I don't see substantial differences between SS and NSS after day 218.

P31 L1-4: I think this conclusion should be formulated less strong: the 'erroneous conclusions' depend on what you are modelling. Your results appear to show that using SS or NSS conditions does not have a large effect on e.g. the total amount of Rsoil over a whole growing season. However, if someone want to obtain detailed daily estimates of Rsoil on a (sub-)daily timescale, this is indeed important. I suggest the

authors re-formulate these sentences.

Technical comments

P2 L34: . . . down to 1 m

P3 L51: . . . precipitation inputs. The DETECT model. . .

P5 L8: . . . coarse-grained

P5 L9: fast CO2 diffusion rates

P5 L11: . . . we expect coarse-grained soils

P5 L13: . . . air-filled pore space

P6 L14: . . . depth-invariant CO2 production rates

P7 L 16: behavior and to (no comma)

P11 L12: remove the comma before 'and'

P18 L5: . . . to 1 m depth

P20 L10: change to '(J previous time periods)'

P21 L20: if the SOC data you talk about is the same as shown in figure S4, you can refer to that figure here.

P23 L18:. . . 2013). These data were. . .

P30 L17: You could change this to: . . . it may take about 15 minutes for a. . .

Figures and tables

Figure 1

Caption: everything after '. . . ,and temporally varying bulk CO2 fluxes.' is redundant here. You could alternatively refer to the material and methods section where this is

also explained.

Figure 2

- Legend: add that root and microbial contributions are simulated using the DETECT model

- For easier comparison of the Rsoil between the two scenarios, you could indicate the Rsoil values shown in (a) on the bars in (b)

- Caption: 'see Table 2' should be Table 3 (also in Fig. 3, 4, 6, S1 and S2)

Figure 3

- Names of the scenarios in the sub-figures could be replaced with more intuitive names, followed by the scenario name between brackets, to increase readability.

- Include a legend for the grey and red lines

Figure 5

- Subplots (a) and (b): as you want to make the comparison between measurements and model results, you could choose only to show the timespan for which measurements are available (and show the entire timespan in the supplement)

- Legend: add 'depth': e.g. 3 cm depth

Table 1

- Instead of grouping the variables by 'Group 1', 'Group 2', etc, it would be more intuitive to provide the names to which the groups refer in the table (e.g. Group 1 = microbial submodel parameters, etc.)

- I would encourage the authors to include the references from where the parameter values were obtained in the table (where appropriate), now this is only described in the text

Table 3

- Bottom row, middle column: ‘about’ should be ‘above’?

Supplementary information

Appendix S1

- Is there any evidence that root biomass varies between 0.5 and 1.5 times the amount measured in the middle of the growing season? Please include this.

- Last sentence of first paragraph: ‘decays’ should be ‘declines’?

Figure S2

- Same remarks as for Fig. 3

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