Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-223-AC2, 2018

Geoscientific Model Development Discussions
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EGU

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.

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Received and published: 18 January 2018

Responses to reviewer RC2 (anonymous) on "Modelling soil CO2 production and transport with dynamic source and diffusion terms: Testing the steady-state assumption using DETECT v1.0" by Edmund Ryan et al.

Reviewer 's general comments In their manuscript, Ryan et al. study under which conditions soil CO2 production is in steady state with CO2 ïňĆuxes at the soil surface using a modelling approach, in which they focus on the effects of grain size and antecedent

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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 simpliïňĄed version of the model which assumes steady state conditions (no delay between subsoil production of CO2 and CO2 the ïňĆux at the soil surface), by applying the model to an experimental site in Wyoming (PHACE).

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 speciïňĄc 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 speciïňĄc 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 speciïňĄc comments). The fact that the authors provide the codes of their model together with a clear user manual increases the impact of their contribution.

Thank you for these comments. We will ensure we carefully and fully address the specific comments you refer to here.

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 speciïňĄc 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

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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 efïňĆux (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.

Thank you for this comment, and we completely understand your concern. We have evaluated an alternative production versus soil water content function (i.e. an alternative to equation 4a). This alternative function simulates the production of soil CO2 versus soil water content as a bell shaped curved. In other words, production increases as soil water content increases but only up until an optimum soil water content value. When soil water is higher than this value, the production decreases. The formula for this alternative function is given in appendix S4 of the revised supplemental material. A graphical representation of the original function (equation 4a) and this alternative function (appendix S4) is shown in figure S7 of the supplemental material. For the Wyoming field site, however, soil water content never reached values that would have resulted in reduced ecosystem or soil CO2 flux or respiration rates . Hence, the graphical representation of this alternative soil CO2 production function shows production increasing for values of soil water content up to the optimum soil water content value. We ran the DETECT model for March-September, 2008, using this alternative produc-

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tion versus soil water content function, and the time series of predicted soil respiration is given in figure S8. The predicted soil respiration fits the ecosystem respiration and microbial respiration measurements equally well, when comparing this figure with the corresponding figure in the manuscript (figure 2) which used an exponential function for the production versus soil water content relationship. Thus, either function (bell-shaped or exponential) is equally good at representing the relationship between soil CO2 production and soil water content at our well-drained, mid-latitude field site. To address your final point about how equations 4a and 4b affect the CO2 produced by roots and microbes, figure S9 in the supplemental material shows modelled S (production term in equation 1) against soil water content with the points colour coded according to three soil temperature bands.

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?

This is an interesting question, but DETECT is not designed to follow slowly changing soil carbon pools. That is, we can't use DETECT to infer changes in the SOC pools (denoted C_SOM in model description). SOC is an input to the model (here, field data inform SOC, but other models focused on soil carbon pools could also be linked to DETECT), so the model does not predict changes to SOC. DETECT is most useful for understanding and modelling fast-time scale processes (e.g. fluxes) given known, measured, or hypothesized pool sizes (e.g. SOC). A future model development would be to couple DETECT to a dynamic soil C pool model, but this is unrealistic for the manuscript revision. Our view is that C_SOM will not differ between the SS and NSS models because the total amount of CO2 lost from the profiles was the same over of a growing season. The NSS is only important in clayey soils exposed to wetting/drying cycles and only for CO_2 efflux on periods of weeks-months. The C_SOM pool would take years to change.

Other suggestions and remarks are formulated in the speciïňĄc comments below. SpeciïňĄc comments P 4 L17-18: in addition to delays due to CO_2 transport times, is also something known about the effect on additional CO_2 production (as this is one of the outcomes of the study)? We don't understand what the reviewer means by this question. In particular, we don't understand what 'effect on additional CO_2 production' as an 'outcome of the study' means. The aim of the study is to determine if it is reasonable to assume that soil CO_2 produced in the soil is respired at the same time point (steady-state), under different soil texture and precipitation regimes.

P5 L21: please clarify what you mean with 'displacement of CO_2' We mean physical displacement. We will make this clearer in the revised version of the manuscript.

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 Okay, we will try to reduce this text for the revised version.

P8 L6-16: this is mostly a repeat of the last paragraph of the introduction and can be removed Fair point. We will remove this text or remove the part of last paragraph of the introduction where there is a repeat in text.

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. Thank-you for this suggestion. We will consider this for the revised version.

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

P11 eq 3: It's not clear to me how you obtained the value for RRbase, can this be stated explicitly? This is a parameter that we estimated. It is given in table 1. We will add a sentence to explain how we estimated it.

P13 L16-17: how were these different values for the constants obtained? Please provide a reference if appropriate Section 2.4.5 (page 21) explains how these parameters were estimated.

P14 L13-14: please provide the value for the atmospheric CO2 concentration that was used here. This is already given a few lines below equation 9c.

P16 L17 – P17 L8: This paragraph belongs to the introduction, not to the materials and methods section. We see your point. This text sets the scene for what comes after and it's specifically about the field site conditions. We will consider moving it to the introduction if it will fit (the introduction is already too long). Otherwise, we will shorten the text.

P18 L8: please be more speciïňĄc about the data that was created We will improve the clarity of this sentence in the revised version.

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. These weight parameters are already included in table 1.

P21 L5 – 10: This can be removed in my opinion, this is also explained in the caption of the table Thanks for the suggestion. We're definitely looking for ways to reduce the length of the manuscript, and this portion of text is certainly a candidate.

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

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? We have measurement of C_SOL and C_SOM but only a very limited number. Equation (7) is simulating C_SOL for all 100 depths and all 732 time points. We'll amend the text to make this clearer.

P22 L9: It is not clear how both parameters (Vbase and Km) were obtained through ïňĄtting the microbial respiration submodel to data. Please clarify. Also, why are Cmic and CUE left out? We'll modify the text to make this clearer. For your second question, the model fitting took place a period of time prior to the DETECT model being developed, and the formula (eqn 5) in that instance was use to estimate the soil respiration of CO2 from microbial sources. At the time, we did not have measurements of C_MIC or CUE so these were left out of that version of the submodel.

P22 L16: please clarify how these values were adjusted. Okay, we'll make this clearer for the revised version.

P24 L6: I agree with the comment from reviewer 1 here: please clarify how texture affect the model outcomes. The soil texture is an input into the HYDRUS model which was used to simulate soil water content and soil temperature for all depths and times. By varying the soil texture in HYDRUS, this resulted in different sets of soil water content and soil temperature values. We will make this clearer in the revised manuscript.

P24 L15: please provide the amount of precipitation in 2009 here. Okay, will do.

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 This is just a stylist preference I think. We prefer to start the results section with bold statements about what the results revealed. The aim is to grab the attention of the reader. We take your point though, so we will consider dropping this text.

P26 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 result, see comment above. I agree. You can think of the model with antecedent terms and the model without antecedent terms as two versions of the model. There are other versions that could be considered. A growing body of evidence (see introduction) suggests that antecedent conditions are important when simulating respiration (at different soil depths in this case). This is why we consider it here.

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 signiïňĄcant and greatly contributes to the increase in total Rsoil. Why is this not mentioned in the text here? We will address this in the revised version.

P27 L11: I don't see how Fig. 3 shows that there is a greater root respiration. Thanks for spotting this. An earlier version of figure 3 included the green and blue lines (like in fig.2). We will amend the text to remove any confusion.

P27 L16 – 20: This formulation is confusing: in the ïňĄrst 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. We'll reword this text to improve clarity.

P30 L6 – 8: from the data you show in the ïňĄgures 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. We'll reword this text to make this clearer.

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. Thank-you for this comment. We'll amend the text addressing the points you make.

Technical comments Thank-you for spotting all of these. We will fix all of these issues that you mentioned. P2 L34: ... down to 1 m P3 L51: ... precipitation inputs. The DETECT model... P5 L8: ... coarse-grained P5 L9: fast CO_2 diffusion rates P5

L11: ... we expect coarse-grained soils P5 L13: ... air-ïňĄlled pore space P6 L14: ... depth-invariant CO_2 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 ïňĄgure S4, you can refer to that ïňĄgure 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 CO_2 ïňĆuxes.' 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-ïňĄgures 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 'Group1', 'Group2', 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 ïňĄrst paragraph: 'decays' should be 'declines'? Figure S2 - Same remarks as for Fig. 3

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2017-223/gmd-2017-223-AC2-supplement.pdf

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