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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.

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Responses to reviewer RC1 (F. Moyano) 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 The manuscript describes a modeling study with the main objective of determining the signiïňĄcance of non-steady states for determining and understanding soil respiration ïňĆuxes. The paper is well written, with a logical structure and clear sentences. Apart from some minor comments, We ïňĄnd 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 deïňĄned 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.

Thank you for these positive comments.

Generally, the study could focus more on the speciïňĄc 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 analysed 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 We comment below). On the other hand, a closer look at how concentrations change in soils, the amounts of air-ïňĄlled pore-space and how much/fast CO2 is displaced upon wetting would be a nice addition.

Thank you for these very helpful suggestions. We would like to keep the simulation experiments and the different scenarios, but we will amend the manuscript to better link them to the research questions. While there are many ways to create different simulation conditions (or scenarios), the scenarios that we selected were motivated by real data, from a real field site. The different scenarios lead to different soil conditions, thus allowing us to evaluate potential conditions or situations under which NSS conditions might be relevant.

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Discussion paper

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 ïňĆux for CO2). This also makes clearer that a NSS is always a temporal condition, so any difference (at daily or seasonal scales) should be explained by changes in storage.

Thank you for this comment. A storage state variable for soil CO2 is already included in the DETECT model. After reading your comment in its entirety, we now realise that by a storage state variable, you mean total soil CO2 over the soil profile. You state the total (Rsoil + storage) should be equal for both models, but we think you meant to say that the total (Rsoil + change in storage) should be the same. We have checked this. Please see appendix S3 for details.

Because changes in CO2 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 ïňĄtted from data.

Thank you for this comment. We essentially did as the reviewer suggested. The DETECT model was run during the growing season of 2007 when measurements of soil CO2 concentrations were available for three different depths as well as above ground CO2 concentration. The initial values for this 2007 run (i.e. the soil CO2 concentrations for all depths) were estimated by fitting a simple function (described in appendix S2 of the supplemental material) to the CO2 data from near the start of the 2007 growing season. The initial conditions used for 2008 (i.e. soil CO2 concentration for 1st April, 2008) were taken from the soil CO2 simulated from DETECT from the final day of the growing season for 2007 (30th September, 2007). In a follow-up paper (Samuels-Crow et al., in revision), we found that it only takes about 1-2 weeks to achieve an equilibrium state, so the model output after this initial time period should not be affected by the initial conditions.

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Reviewer ‘s specific comments Further questions and suggestions are given below as speciïňĄc comments. SpeciïňĄc 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. Agreed. We will remove the ‘moreover’.

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 [CO2] on respiration or as a ïňĆux of dissolved inorganic C to groundwater (neither modeled) would result in different accumulated long-term Rsoil. We will include your above comments in the discussion. Thank-you for your insight.

4/4 A comparison with fossil fuels is misleading if not better clariïňĄed. Rsoil is part of the fast C cycle. Not necessarily a net addition of C. This comparison is purely to help the reader appreciate the size of the global scale Rsoil aggregated over a year. We will amend the text though to ensure that it is clearer.

5/22 The hypothesis that the Rsoil spike after re-wetting is caused by pores ïňĄlling with water and displacing CO2, is presented here, but not quite tested in the study. This is something for a future study to address.

10/15 How is Ψe(z) calculated? Is θsat(z) not the same as ÏŢT? The air-entry potential is calculated from measurements. We will add a reference to support this. The formulae we use are taken from the literature. We will add extra references, where required, to support these.

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 lead to different seasonal ïňĆuxes. The discussion of those differences should include this. This point was raised by the other reviewer. Please see our response to that. In summary, θ at our field site never reached high enough values for respiration to decline. For completeness, however, we redid the control run using a respiration vs θ function that was bell shaped instead of exponential. We found that the time series of predicted soil respiration resulted in a very similar fit to the measurements. We will include extra discussion on this and all the points you make above.

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 θ3. As with the f(θ, θant) function, it changes Rsoil in response to changes in precipitation. This needs mentioning in the discussion. This formula was taken from the Davidson et al. (2012) paper (mentioned above this formula in the manuscript) which used field data to test its suitability. We have thus adopted this formula here, but we appreciate that there are other options. We will try to find space in the discussion to include your point, but the paper is already too long so this may not be possible given the other discussion points we need to include.

14/4 'time-varying' Thanks for spotting this. We'll change it.

15/11The expression is not an equality so it does not say how exactly Ndt is calculated. Fair point. We will clarify this in the revised version.

15/eq.10 Would be nice to see this derived in the appendix. There's nothing to derive. The expressions in equation 10 is just the discretised (or finite differenced) version of equation 1. We put equation 1 in this form in order to be able to numerically solve it. The Habernam book (that we reference) gives a great explanation of this.

16/9 Should actually cite the original derivation (by Cerling 1984) Okay, we will do.

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. I'm not sure I follow. This is the steady state solution to equation 1, so it has to involve a z term.

19/15 A reference for this procedure would be useful. Yes, of course.

22/5 Parameter p probably has a strong impact on Rsoil. Uncertainties in this parameter would be informative. Yes, you're right. Uncertainties are very important. For our study, we kept the parameters fixed, but when doing inverse modelling or uncertainty analysis we of course would want to assign a probability distribution to all parameters including this one.

22/10 Why without Cmic and CUE? 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 used 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.

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 speciïňĄed how). Given the discussion related to texture, this should be made clearer. Thanks for this. We'll update the methods to make this clearer.

26/1-2 The ïňĄrst sentence here is not clear. What effects? We'll use a different word to make it clearer what we mean.

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. Thanks for this. We agree that the paper is rather long, so we'll remove this section as you suggest.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2017-223/gmd-2017-223-AC1-supplement.pdf

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