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Discussions

Interactive comment on "Modelling the diurnal and seasonal dynamics of soil CO_2 exchange in a semiarid ecosystem with high plant-interspace heterogeneity" *by* Jinnan Gong et al.

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General comment: "This study concerns the development and application of a highly detailed physically based patch-scale land-atmosphere energy, water and carbon balance model for a semiarid ecosystem with high plant-interspace heterogeneity. The model represents an expansion of the model developed by Gong et al. (2016. Ag. Forest Met) that compared patch scale water and energy exchange into soil-plant C exchanges. The model represents most of the C stocks and ïňĆuxes that you expect to be relevant for dryland ecosystems, but which are not normally represented in ecosystem C models like photodegradation, biocrust photosynthesis and respira-

tion, gas and liquid phase co2 transport, etc. The model is used to simulate bare and plant shaded surfaces as well as biocrust covered surfaces and compared to measurements. The model was shown to be very capable of accurately simulating measured soil temperatures, soil moisture and soil respiration(Rs). The main ïňĄndings were that totalCO2 production in the soil and Rs could deviate substantially from one another due to root uptake, crust respiration and photosynthesis, and variations in CO2 dissolution, emphasizing the processes beyond heterotrophic and autotrophic respiration and highly heterogeneous nature of CO2 cycling in patchy ecosystems. These results shed some light on the importance of these other processes that are not commonly represented in ecosystem models as well as our ability to represent them in ecosystem models. The paper is well written. The authors do a great job in discussing the background literature in the Introduction as well as tying their ïňĄndings to previous studies in the discussion. The paper is very long, but this should probably be expected given the highly detailed modeling work that is being presented. Overall, there is nothing fundamentally ïňĆawed with the paper and I expect that this work will be of interest to ecosystem modelers, particularly those interested in dryland ecosystems. My main complaint about the paper involves equiïňĄnality of the model results and the lack of data to be able to validate their ïňĄndings on the relative roles of the different component ïňĆuxes. A model with far fewer parameters and processes is likely to be equally as capable of simulating soil moisture, temperature, and Rs for these cases or tests so how can one have much conïňĄdence that extra capabilities of the model (to represent the individual ïňĆuxes and transports like photorespiration, crust photosynthesis/respiration, CO2 uptake by roots) are valid? Table 3 is great, but it could be entirely ïňĄctitious. While I'm excited to see models being built with these processes considered, I'm wonder how we can build conïňĄdence that they are any better than simple, more empirical models already out there. "

Response to general comment:

We sincerely appreciate the hard work of reviewer and sharp comments. The

manuscript has been revised in light of the comments, with several main changes in the structure, figures and tables. All the modified texts have been marked in red color in the revised manuscript. See the changes in text, figures and tables in supplementary file.

For the main complaint of the reviewer, we fully aware that over-complexity and over-parameterization could be important sources of uncertainties for process-based models. However, good fitting may not be the ultimate goal of modelling. Mechanistic models are found on mimicking the system structure and processes, breaking the big black box into smaller and simpler ones (which is also easier to experiment on) and connecting them by known cause-effects, so that it could integrate existing knowledge and possibly make some extrapolation to a different space-time. I must emphasize that a well-trained regressive model with much less parameters may have high goodness-of-fitting, but does not necessarily explain how an ecosystem works, or clarify the scope of its applicability – so we might argue if it is the suitable way to apply Occam's razor. For example, one may need enormous empirical models to calculate CO2 emissions, in order to cover different combination of environmental factors, soil properties, canopy features and biocrust types. However, through incorporating different modules (processes) and parameter values, mechanistic modelling actually serves a simpler and more rational way to aid this complication. Moreover, mechanistic models (like this one) are eventually found on small "black boxes", at which level detailing the mechanisms further become difficult and using simple empirical functions become near-optimal. In this sense, Occam's razor still applies, and stays with the concept of the mechanistic model.

We also keep in mind about the uncertainties of modelling. To separate the individual fluxes, we used multiple chambers (C1, without crust influences; C2, with only dark respiration; C3 with photosynthesis and photodegradation) to perform a step-wise validation. However, as pointed out by reviewer, only one chamber for each step may not be enough and fluxes like photorespiration and CO2 uptake by root are still lack of sup-

port from data. We are planning more measurements addressing these issues. These uncertainties, along with several other possible aspects, have been demonstrated in revised section 4.3 "Uncertainties and challenges".

"A couple of relatively minor issues: 1. There could be better setup in the introduction. What are the objectives and rationale of this study? Major questions or hypotheses? "

Response to minor issue 1: This setup of introduction may not be optimal for generating questions and hypotheses. However, we decided to bring up the work from the view of modelling, as the main problem is that there hasn't been any method, so far, that for researchers could integrate those most discussed C processes for dryland ecosystem. Without such a "playground" in the first place, generating questions and hypothesis regarding the componential fluxes and subscale heterogeneities, or their environmental sensitivities, will be difficult. In this case, model development become a primary objective, and this has been emphasized particularly in Line 96-108 of introduction. For better demonstrate the modelling results and tests, we re-organized the result section and tried to investigate two specific questions: i) the roles of componential C processes in regulating soil CO2 effluxes in the studied ecosystem, and ii) the plant-interspace differences in the componential C processes. These contents has been added to introduction as well, see Line 115-117.

"2. Figure 8 used as an estimate of photorespiration. How do you separate the effects of greenhouse effect under the clear chamber versus the shade effect of the opaque one? In other words, the opaque chamber shields the surface and reduces the heating when the chamber is closed. The clear chamber, by allowing solar radiation in and blocking thermal radiation out, is going to be heated much more potentially during the measurement cycle, potentially increasing heterotrophic respiration. Is Rs higher because of higher temps or because of photorespiration? "

Response to minor issue 2: The C fluxes are measured by automatic chambers, which only seals the collar during measurement (2.5 minutes), then move away from the

collar. Therefore, the collar was not blocked by chamber in most of time, and the temperature disturbance by measurement are marginal. As suggested by Figure 8, flux signals during the daytime nearly doubled during those periods; it is difficult to be explained as heating effect, as even 2 degree heating lead to < 10% changes in efflux (Table 4, sensitivity analysis). Also, the period was dry and with almost no rain event (see Fig. 6 in revised manuscript). Therefore, photorespiration by crust organisms is also unlikely.

“Text speciïňĄc comments: L13. This sentence is an unusual way to open up an Abstract. I am wondering if it could be replaced with a sentence that provides context and rationale for the study ”

Response: The abstract opening has been revised (Line 13-16).

L54. cannot

Response: Revised to “may not” (Line 56)

L55. periods

Response: Revised to “periods” (Line 58)

L58 intensive? Also, why would water and CO2 transport be more intensive in the drylands?

Response: We are agree with reviewer that this claim could be assertive. The sentence has been revised to “In dryland soils of high salinity/alkalinity, CO2 transport and water cycle are tightly coupled, as large inorganic C fluxes can be driven solely by dissolution and infiltration of CO2 and carbonates” (Line 59).

L62. Here’s another paper with the relevance of abiotic C with ïňĆuxes on the diurnal time scale. Hamerlynck, Erik P., et al. "Nocturnal soil CO2 uptake and its relationship to subsurface soil and ecosystem carbon ïňĆuxes in a Chihuahuan Desert shrubland." Journal of Geophysical Research: Biogeosciences 118.4 (2013): 1593-1603.. There

are several papers out that seem to show that inorganic C uptake is unlikely to be a very signiïňĄcant ïňĆux...see e.g. review in Schlesinger, William H. "An evaluation of abiotic carbon sinks in deserts." Global change biology 23.1 (2017): 25-27.

Response: It is true that inorganic C uptake may not be very significant flux in many cases. Our simulation also showed that such a flux was only about 15% of total emission from collar, and those C may still emitted somewhere during the transport. The main idea to include the transport processes are to better explain the variations of efflux, which may not necessarily caused by changes in soil C pool, but just caused by noises from the transportation process.

L69. matter Response: Revised as suggested.

L70. “could maintain inactive”? Response: Revised to “could be inactive”.

L77. Might consider H. Throop’s work here, e.g., Throop, Heather L., and Steven R. Archer. "Resolving the dryland decomposition conundrum: some new perspectives on potential drivers." Progress in botany. Springer Berlin Heidelberg, 2009. 171-194. If you can’t ïňĄnd this chapter, she has several articles about photodegradation.

Response: Good suggestion. Citation of Throop et al. 2009 has been added.

L79. periods Response: Revised as suggested (Line 80)

L95 DeïňĄne “global change” Response: Revised to “global climate change” (Line 96)

L104. “works” Response: Revised as suggested (Line 105)

L114-115. This sentence seems out of place. If this represents an advance of Gong et al. you should cover what this model development is. Response: This paragraph has been revised.

L118. How about “Model Overview” Response: Revised as suggested (Line 118)

L119. modeling work was based on measurements? Response: Reworded as “...

model was build based on ... " (Line 119)

L128. Don't understand this sentence Response: The sentence has been reworded (Line 128).

L456 "probably"? Response: Revised as suggested (Line 460).

L491. Later on, Test 4 is mentioned, but it should probably be included in this paragraph Response: Test 1-3 are for model validation, Test 4-5 are for sensitivity analysis, so it could be better to separate them into different sections.

L503. component Response: This paragraph has been revised.

L521. "was more pronounced" Response: Revised as suggested (Line 536).

L523-525. I don't know of many soil C3 water probes that are good at measuring frozen water content. Are you sure the measurements are valid during these times? Response: It is true that water content measurement during freezing period may not be reliable. We have changed the statement here (Line 537).

L534 4b? Response: revised to 5b (Line 549).

L554. All the variables need to be clearly redeïňĄned in the Table caption so that this paragraph is much easier to understand. Response: Table 3 has been revised and the definition of variables has been added.

L565. Compared to Response: The paragraph has been revised.

L566 ,irrespective of the size... Response: Revised as suggested (Line 583).

L573. "compared" Response: Revised as suggested (Line 599).

L598. "our model capably reproduced the time series for the water and energy ïňĆuxes at ..." Response: Revised as suggested (Line 542).

L605 Suggest using another heading before this paragraph, something like "modeling uncertainties" Response: This has been suggested by both reviewer and we have

reorganized this part to section 4.3

L705. provides L708 caution L709 Our simulations showed that a Response: The result section has been reorganized. Those sentences has been rewritten or removed.

L773. uptake Response: revised as suggested (Line 783).

L785 Are the model and data available for others to use ? Response: So far, it is among several collaborators but yes. We are still trying to include the aboveground vegetation and develop the system further.

Table 3. All terms need to be deïňĄned in this table caption including Fs, Fft PCt Response: Revised. Definitions have been added to table footnote.

Figure. 1. The photo is really too small to see much of anything. Suggest deleting this so there is more space for the conceptual ïňĄgure Response: Revised. The conceptual framework has been displayed separately as Fig. 2.

Fig. 4. Ppt is not labeled or given a scale Response: Revised (see Fig. 5).

Fig. 6. Greek letter is not deïňĄned in the caption. Response: Revised (see Fig. 7).

Fig. 7. This ïňĄgure is very hard to see. Could you use more colors for the different symbols so that it is easier to see? Response: We revised the figure by differentiating the coloring (Figure 8). The resolution of initial figure was also limited by the file-size restrictions of discussion paper. We will upload bigger images with better qualities in the final submission.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-95/bg-2017-95-AC2-supplement.zip

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-95, 2017.