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Interactive comment on "Soil carbon release responses to long-term versus short-term climatic warming in an arid ecosystem" *by* Hongying Yu et al.

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C: the original comments; R: the responses to the comments.

General Comments:

C: The paper describes a four year warming and wetting experiment in a desert steppe in Northern China. The introduction gives a good overview of the latest and more established scientific insights and the authors did a thorough measurement campaign. I particularly appreciate how much work went into the various additional belowground measurements. Given the limited number of such experiments for this ecosystem type,

this work is certainly of interest to readers of Biogeosciences. Overall, the paper is well structured and written clearly, but I feel there are some elements in the text that require clarification or some more in-depth information. If the authors manage to improve these elements I would recommend the paper for publication.

R: Thank for the positive comments. The manuscript has been revised as kindly suggested.

Major comments:

C: My first major comment is directly about the abstract. There is a seemingly counter-intuitive message there that confused me when reading it: Long-term warming reduced Rs by 32.5 percent (line 18). Yet, long term climatic warming decreased SOC (line 24)? While this is certainly possible, it is not directly what one would expect. Was this reduction in SOC caused by an initial spike in Rs at the beginning of the experiment? The lower SOC content could then also contribute to decreasing respiration due to reduced availability of substrate to microbes to decompose. Yet, the authors mainly talk about the moisture effect and how low soil moisture decreased Rs. The mentioned decline in SOC from the abstract is not presented in the results and discussion. Actually, the authors state that "in the present study, SOC concentrations were not significantly affected by climatic warming" and then later write "although SOC might be expected to decrease with long-term climatic warming" (conclusion iv). I do not understand how such a strong statement can be made in the abstract when the results and discussion show otherwise and even contradict one another. Given the high number of people that generally do not read beyond the abstract, my suggestion is to 1) rewrite this part of the abstract more clearly and 2) to present the evidence to support this claim more clearly in the results and discussion.

R: This suggestion is very valuable for us. To be clearer and more logical throughout the text, we have rewritten the relevant expressions: e.g., we deleted the "soil organic carbon content tended to decrease with long-term climatic warming" because of the results: “SOC concentrations were not significantly affected by climatic warming (Supplementary Table S1). Yes, based on the present results, we mainly focused on the new significant findings’ aspects: the long-term warming effects on soil preparation, watering effects, and the its relationships of with soil moisture and soil temperature. Thank you for the kind suggestions.

C: My second major comment is about the authors’ choice for the various model fittings in the statistical analysis and in particular for the Gompertz function. The authors provide limited explanation for choosing the Gompertz equation in section 4.2, line 334-337 and mention the parabolic curve function as another viable option. Indeed, in section 3.2 there is another model with a better fit: the quadratic functional model. The authors do not argue further why they still continued parameter fitting with the Gompertz curve, despite the quadratic model having a seemingly better fit (figure 2 and section 3.2). I would like to know 1) why the Gompertz function was selected and 2) how picking that curve to fit the parameters for the non-linear regression model (eq 4) affected the results compared to taking the parameters from a quadratic model fitting (sensitivity analysis)?

R: We conducted the Gompertz relationship to clarify the relationship because the data most likely support a Gompertz (i.e., saturating, sigmoidal) relationship rather than a linear relationship. The parabolic curve mentioned (in section 4.2) is inappropriate, was deleted. 1) the Gompertz relationship can well fit with the relationships between Rs and soil water content (R2 = 0.87; RMSE = 4.88; also refer to e.g., Gompertz, 1825; Yin et al., 2003), which also can obtain some key thresholds (e.g., the asymptote value, the optimal SWC) that can not obtain from both linear and quadratic functional models. 2) A non-linear regression model (eq 4) is used to fit the relationship of Rs with both soil temperature and soil moisture. The optimal SWC of 0.229 (v/v) was estimated by the Gompertz functional curve. This optimal SWC (means that a SWC value when Rs reach a maximum) is a necessary parameter of equation 4, which is just obtained the Gompertz functional model. Thanks for the valuable comments.

Minor comments:

C: The Gompertz function (line 22): This function (and its shape) might not be a given knowledge for all readers. My suggestion is to rephrase in the abstract to “whereas the relationship between Rs and soil moisture was better fitted to a sigmoid function” and explain the Gompertz curve further in Section 2.6 (see major comment 2).

R: Thank you, this has been done in line 25 the abstract and line 212 in Section 2.6.

C: Line 48: The desert steppe is c. 8.8 million square hm. Do the authors mean total global desert steppe area or the area in China?

R: It means the area in China, and has been revised to “The desert steppe of China”.

C: Line 74/75: I would suggest adding the more recent reference to Yan et al. 2018 here as well.

R: Thanks, we have added it already in line 81.

C: Reference: Yan, Z., B. Bond-Lamberty, K. E. Todd-Brown, V. L. Bailey, S. Li, C. Liu, and C. Liu (2018), A moisture function of soil heterotrophic respiration that incorporates microscale processes, Nature Communications, 9(1), 2562, doi: 10.1038/s41467-018-04971-6.

R: It has been cited and added in the reference list.

Many thanks for the constructive comments and suggestions.

Please see the Manuscript-Revised-with supplement as Supplement (pdf).

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-236/bg-2019-236-AC4-supplement.pdf

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