Response to associate editor decision

Dear Dr. Akihiko Ito,

We sincerely thank you for your thoughtful review of our manuscript and responses to the referees. It is with great pleasure to have the opportunity to submit a revised manuscript to Biogeosciences. Your commitment of time in providing valuable feedback on our work is greatly appreciated. Enclosed below is our response addressing your comment.

Comment: *Concerning Comment 5 from Referee #2, i.e., spin-up of the model. You wrote that the model adopted observational data as the initial condition and did not perform a spin-up. I agree that this applies to soil moisture but have a concern about carbon stock and LAI, because most models require a long spin-up to obtain stable vegetation and soil carbon state. Please clarify this point.*

Response: While it holds true that spin-up procedures are beneficial for establishing stable vegetation and soil carbon states, it is important to note that the STEMMUS-SCOPE model does not involve the simulation of carbon stocks, including LAI. Just as with soil moisture, we utilized observed LAI values as the model input, which served well as a stable vegetation state. Our model primarily focuses on simulating carbon assimilation within the existing framework. Consequently, we do not consider the spin-up (to attain a stable LAI and soil carbon state), primarily due to the absence of carbon allocation in our model. We are aware of this issue and is addressing it in our upcoming study by coupling the STEMMUS-SCOPE model with the WOFOST crop growth model. This coupling will allow us to simulate LAI and plant growth processes effectively. The evaluation of the coupled STEMMUS-SCOPE-WOFOST model, performed with various plant functional types, showed successful simulations of vegetative dynamics, including LAI and plant height. We intend to submit the study of the STEMMUS-SCOPE-WOFOST model to a journal soon.

Thanks a lot for your attention and consideration! We look forward to the insightful feedback and discussions on our revised manuscript.

Yours sincerely,

Enting Tang, on behalf of Zhongbo Su, Yijian Zeng, Lingtong Du

Point-by-point response to the reviews including a list of all relevant changes

Only the comments with relevant changes in the revised manuscript are listed below. Elaboration on other comments from Referee#1 and Referee#2 have been addressed in the open discussion platform https://bg.copernicus.org/preprints/bg-2023-70/#discussion.

Comments from Referee#1

1. In the introduction section, the scientific question is not clear. Generally, the authors thought root water uptake is a critical process in the modeling, and the dynamic root length density for estimating root water uptake is necessary. However, no contents about the root water uptake were presented in this study. What is the impact of dynamic root length density? What is the performance of root water uptake simulation? This is the major limitation of this study.

7. Why were root-related parameters not identified as influential parameters? This is the main focus of your study.

Response: Thank you for the insightful suggestions, and the following points were addressed in the revised manuscript.

(1) Lines 59-65 were added to elaborate the research gap further, thus drawing out the need to explore the soil water and root water uptake in different layers by applying the STEMMUS-SCOPE model.
(2) Lines 269-274 and Lines 434-435 were added to analyze root water uptake simulation.
(3) The dynamic root length density defines the relative amount of root water uptake in each soil layer. Therefore, we directly compared the difference in RWU, which is simulated based on maximum rooting depth, fitted extinction coefficient, initial root density and root biomass. The impacts of maximum rooting depth, fitted extinction coefficient and initial root density were evaluated in the sensitivity analysis (Lines 202-207). We found that the root-related parameters are more influential in the simulations of soil water content and ground heat flux and have a relatively strong interactive effects in all the simulated fluxes (Fig. S5 in Supplement).

8. How did the authors optimize the parameters (best-fit trail in Line 196)?

Response: Incorporating three root parameters, we updated the sensitivity analysis results where 220 sets of parameters for shrubland were generated. The determination of the optimal trail is described in Lines 181-186 in the revision. The updated parameters were updated in Lines 205-206. The optimal trail was adapted for grassland scenario with adjustments to root parameters as detailed in Table S1.

10. The sensors were installed under the grassland, but the simulated soil water content is the average of shrub, grass, and bare soil. So, direct compassion of them may have a large bias.

Response: A more detailed discussion on this bias was updated in Lines 351-356.

12. Why cannot the model capture the wet deep soil layer? Is it related to root water uptake? More analysis and simulation should be performed.

Response: It was an unclear statement in the first manuscript. From Lines 377 to 388 in the revised version, we rephrased the analysis of the uncertainties in simulating LE and GPP. In conclusion, the underestimations might be caused by the uncertainties in the approximated LAI, V_{cmax} and initial setting of the SWC profile. The root parameters are not the main influential parameters to LE and GPP simulations, while they are found to be more influential to the simulations of soil moisture and ground heat flux. Because the root growth/distribution directly interacts with the soil water content/distribution in STEMMUS-SCOPE calculation.

Comments from Referee#2

The title was changed into *"Understanding the Effects of Revegetated Shrubs on fluxes of Energy, Water and Gross Primary productivity in a Desert Steppe Ecosystem Using STEMMUS-SCOPE Model"*.

1. Equation (1), what does C_shrub mean?

Response: We have added an explanation of this variable in Line 161.

6. Figure 4c, there are a few points aligning as a vertical line. Does it indicate there are some problems with the data or the model?

Response: The cause of the vertical line has been explained in the author's response in the open discussion platform. Accordingly, we summarized this uncertainty in Lines 356-360.

8. Figure 6, could you also show the variability?

Response: The variability was added in the updated plot (Figure 6, Line 275).

9. Section 3.3.3 on GPP, I don't think the interpretation of midday depression is accurate. It may involve factors more than radiation. Perhaps you can show the diurnal pattern of leaf temperature, and temperature dependence of carboxylation as well.

Response: The diurnal pattern of leaf temperature was added in Figure S10 (c) in the revised Supplement. And the interpretation of midday depression was improved as written in Line 418-423.

10. Discussion 4.2.1, paragraph 2, why don't you use the Bowen ratio as an indicator, which is likely more informative than the ratio of LE/Rn here.

Response: The Bowen ratio was calculated and updated in Line 398 and indeed is a better indicator to tell the dominance between sensible heat flux and latent heat flux.