

Interactive comment on “Divergent climate feedbacks in the growing period and the dormancy period to sowing date shift of winter wheat in the North China Plain” by Fengshan Liu et al.

Anonymous Referee #1

Received and published: 26 November 2020

General comments.

The manuscript by Liu and collaborators presents an experimental simulation study aiming at characterizing surface biophysical processes as influenced by shifts in sowing dates of winter wheat in 10 selected locations in northeastern China. The topic is obviously suitable for the Journal and is of interest to an international audience targeting more resilient cropping systems in a changing climate scenario. Therefore, examining how sowing date affects wheat phenology is of clear importance and should improve our understanding of the mechanisms that govern winter wheat yields in intensively managed agro-ecosystems. The use of model simulation as a method of analysis is

C1

another advantage of this work since it provides a comprehensive characterization of crop dynamics at the regional level. Research on this topic is limited, both for Asia and North America.

This work should be considered for publication in Biogeosciences only after a substantial revision is provided by the authors. Despite the novelty of this data set, the paper is not well-written, and sometimes hard to follow. I recommend the authors revisiting their work, particularly taking into account that several grammar, punctuation, and syntax errors are diminishing the quality of this paper. Some extra information is required in the "Data and Methods" section. Furthermore, a clear objective and a closing statement summarizing the management applications of this work are necessary to strengthen the impact of this study. Please find below my specific comments which I believe could improve this manuscript.

Specific comments.

Title. The authors should consider modifying the title of this work, as its current version is confusing. I suggest the following: "Divergent climate feedbacks on winter wheat dormancy as affected by sowing date in the North China Plain". I am hesitant about why the word "shift" is necessary for "sowing date". This applies to the entire manuscript. By studying the effect of sowing date, isn't it implicitly understood that a shift is imposed? Could the authors justify? Thanks.

Abstract. Lines 18-19: Start with "Land cover and management changes...". Please modify the following sentence to: "Crop phenology exerts measurable impacts on soil surface properties, biophysical processes, and climate feedbacks, particularly at local/regional scales". Lines 21-23. It is not clear what is meant by this sentence. If my interpretation is correct, please modify it to "Nevertheless, the response of surface biophysical processes to climate feedbacks as affected by sowing date in winter wheat croplands has been overlooked, especially during winter dormancy". Line 24. The transition to the core of this study is not clear. The authors should introduce first

C2

the objective of the study and then how it was accomplished, rather than providing a sequence of how the data was modeled and further analyzed. Line 27. Mentioning winter wheat is redundant, especially if it has been mentioned before. In my opinion, it also reads better "locations" rather than "stations". Lines 28-30. "better simulated" relative to what? Please clarify. Line 36. "Whilst". Line 37-41. I believe that these sentences should be combined or condensed somehow. There are a lot of redundancies in the use of climate "feedbacks", "effects" and "responses". Line 41. What are the management implications of this work?

Introduction. This section is generally well-organized, yet I recommend the authors re-visiting lines 46-115 as I encountered substantial grammar, punctuation, and syntax errors. Please find below some conceptual comments and term usage suggestions. Line 49. I would replace "agricultural management" with "crop management" and then introduce the concepts of sowing date, and perhaps "cultivars" rather than "bio-geoengineering". Line 64. Consider deleting "The main contributors..." This passage is redundant. Line 71. Organic matter?! Shouldn't be carbohydrates? Or grain starch? Line 73. What is meant by soil depletion? Soil degradation? Lines 77-95. This section is hard to follow. The authors should consider starting this passage with the ideas outlined in lines 68-76. Line 85. "Corn Belt". Line 96. I frequently encountered a discrepancy in how certain terms are referred to in his manuscript. It is recommended that the authors unify and maintain consistent criteria throughout the document. For example, phenology change, phenology shifts change, crop phenology dynamic. They all mean the same? Line 101. Which surface characteristics? Soil surface characteristics? Line 110. What is meant by "are relatively indirect". The authors should clearly state the objectives of their study.

Data and methods. For the study sites, it is recommended that the authors provide an estimate of the total surface area covered by the NCP, and which are "the natural conditions and production levels" that are typical for the NCP. How heterogeneous are the sites? It seems that the area covered by this study is vast, so I am wondering

C3

about the differences other than the air temperature and precipitation? For example, what are the soil types of this region? e.g., north vs. south locations? The quality of the figures and tables (also applies for the R&D) is appropriate. I only recommend referring to the Journal's guideline to verify that the presentation of data in the Tables (particularly the use of spaces) is the correct one. Line 117. "Study locations". Lines 157-158. Could the authors explain why they utilized different periods to validate the model in the two sites? In their previous work at the same locations (Chen et al., 2020), the authors examined a 3- vs. 1-yr period, whereas in the current study a 7- vs. 2yr period is utilized. Lines 181-191. Some of these statements, if not all, seem to belong to the Results section. Line 191. "were representative of the NCP". Lines 200-263. This section only needs some minor corrections, but it is generally well-written, clear, and easy to read. It is recommended that the authors justify the use of SiBCrop relative to other alternatives outlined in Lokupitiya et al. (2009). This is appropriate given that other models are discussed and referenced at the end of the Discussion section.

Results. In general well-written. Yet, some statements do not belong to this section and should be either deleted or moved to the discussion. The quality of the figures presented herein is appropriate and easy to interpret. Lines 267-273. I believe this statement belongs to the discussion. Alternatively, it could be deleted as this information was provided in the data and methods section. Line 279-282. Are these statements necessary in this section? Also, please avoid the use of "So" as a connector. This applies to the whole manuscript. Line 294-296. Again, I believe these types of statements do not belong to the results section. They should be moved to the discussion. Line 309. What is meant by organic matter? Lines 336-339. I am wondering if the study locations, instead of being listed alphabetically in the Tables, could be arranged by north vs. south locations. A simple subheading within the left column will suffice.

Discussion. This section needs some extra work to improve the quality of the writing. Given the substantial number of edits required, my comments are mainly focused on major points rather than correcting English grammar errors. Lines 383-389. It is not

C4

clear if the authors are discussing their results or contextualizing their findings with other research also conducted in China. Line 399. “a proper”. Lines 405-407. To which extent these practices are applied to such a wide surface area? What is the typical farming operation size in this region? Lines 408-409. Please avoid the use of colloquial language “and this affects probably more than we think”. Line 410. Figure 5 should be supplemental. Lines 263-264. I’m curious if the authors considered how fallow (rather than corn) would affect the outcome of EP vs. LP.

Conclusion. Lines 495-505. Easy to follow and well-written. Lines 506-519. Needs some extra work. Please merge these two paragraphs into one body. The highlights of this passage should be (i) the limitations of this study, which I agree is the lack of consideration of how the locations were spatially distributed, and (ii) the management implications of this work.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-388>, 2020.