

Reply to **Anonymous Referee #1**

We thank the reviewer for their overall positive comments on our research. The specific comments on language, “Title”, “Abstract”, "Data and Methods" and “Conclusions” greatly improve the article quality. The reply is as follows.

Question 1 : 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.

Answer 1: The title changed to “Divergent climate feedbacks on winter wheat growing and dormancy periods as affected by sowing date in the North China Plain”

The “shift” is not necessary for "sowing date" and corrected throughout the text.

Question 2: 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 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?

Answer 2: Basically made modifications according to the comments. But please reviewer reconsider the opinion “locations” replace “stations”, because we used stations data and whether “locations” makes reader think our work was a local-scale simulation. The changed abstract:

“**Abstracts:** Crop phenology exerts measurable impacts on soil surface properties, biophysical processes, and climate feedbacks, particularly at local/regional scales.

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. The dynamics of leaf area index (LAI), surface energy balance and canopy temperature (T_c) were simulated by modified SiBcrop model under two sowing date scenarios (Early Sowing: EP; Late Sowing: LP) at 10 stations in the North China Plain. The results showed that the SiBcrop with a modified crop phenology scheme well simulated the seasonal dynamic of LAI, T_c , phenology, and surface heat fluxes. Earlier sowing date had higher LAI with earlier development than later sowing date. But the response of T_c to sowing date exhibited opposite patterns during the dormancy and active growth periods: EP led to higher T_c (0.05 K) than LP in the dormancy period and lower T_c (-0.2K) in the growth period. The highest difference (0.6 K) between EP and LP happened at the time when wheat was sown in EP but wasn't in LP.

The higher LAI captured more net radiation with lower surface albedo for warming effect, but partitioned more energy into latent heat flux with cooling. The climate feedback of sowing date, which was more obvious in winter in the northern areas, and in the growing period in the southern areas, was determined by the relative contributions of albedo-radiative process and partitioning-non-radiative process. The study highlight the divergent climate effects and dominant surface biophysical mechanisms of sowing date in winter dormancy period.”

Question 3: 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.

Answer 3: We generally accept the comments.

(1) 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". The sentence changed into "Cropland surface characteristic had been and will continue to be changed through crop management, such as cropping system (Jeong et al. 2014; Cui et al. 2018), sowing date and phenology shifts (Sacks et al. 2011; Richardson et al. 2013), and cultivars selection (Seneviratne et al. 2018), to keep high yield under climate change condition."

(2) Line 64. Consider deleting "The main contributors: : ." This passage is redundant. Deleted.

(3) Line 71. Organic matter?! Shouldn't be carbohydrates? Or grain starch? Sorry for our misuse of these English words, corrected.

(4) Line 73. What is meant by soil depletion? Soil degradation? "soil water depletion".

(5) Lines 77-95. This section is hard to follow. The authors should consider starting this passage with the ideas outlined in lines 68-76. The Introduction section was organized as follow: firstly, "The cropland changes have feedbacks with climate through surface biophysical processes"; then elaborated in 3 parts, "There are evidences that crop phenology has been shifts substantially", "The crop phenology affects the seasonal rhythm of surface greenness and energy and water exchanges", "dormancy period has been ignored in the winter wheat system".

(6) Line 85. "Corn Belt". Corrected.

(7) 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? Thanks to the author, the above phrases have the same meaning. Based on the question 1 "By studying the effect of sowing date, isn't it implicitly understood that a shift is imposed?", the

phrases was uniformly modified to crop phenology. We also check the full text.

(8) Line 101. Which surface characteristics? Soil surface characteristics? Means aboveground canopy. The sentence was changed to “In view of the close relationships between surface biophysical processes and aboveground canopy”

(9) Line 110. What is meant by “are relatively indirect”. The authors should clearly state the objectives of their study. The “are relatively indirect” was explained “Compared with other phenology dynamics, such as earlier re-greening stage (Xiao et al. 2013; Zhang et al. 2013), longer reproductive period (Sacks and Kucharik 2011) and inter-cropping period (Cho et al. 2014; Bagley et al. 2017), the climate feedback of sowing date emerges gradually with crop development. Particularly, winter wheat grows faster in early stage and slower as winter approaches, smaller change in sowing date could lead to larger and longer climate feedback in dormancy period.”

Question 4: 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 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.

Answer 4: We generally accept the comments.

(1) 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 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 “2.1. Study stations” section was modified to** “The NCP, with an area of 4×10^5 km², is the largest winter wheat production region in China, including Hebei, Henan, Shandong, Jiangsu, and Anhui provinces, and Beijing and Tianjin municipalities (Fig.1). Summer maize - winter wheat rotation is the main cropping system, except Anhui and Jiangsu where winter wheat-rice rotation system is dominated. The satellite data showed a high cropland density above 70% with flat and relatively homogeneous agricultural practices (Liu et al. 2005; Ho et al. 2012). The soil type is mainly classified as sandy loam according to the seven soil textures in the model (Sellers et al. 1996). Two stations with surface fluxes were used for model calibration (Fig.1, blue triangles). Ten randomly distributed stations with complete meteorology and phenology information were selected for simulation in this study (Fig.1, green dots). The details of fluxes, meteorology and phenology were further exhibited

below.”.

(2) Line 117. “Study locations”. We kept the old name “Study stations”, reasons was explained in “Answer 2”.

(3) 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. We used the same dataset as the previous work. Here is the table. Our table contained meteorological driver, which made the two tables wasn’t exactly corresponding in time.

Table 1 The basic information of Yucheng and Guantao stations, China

Parameter ¹⁾		Yucheng							Guantao	
		2004	2005	2006	2007	2008	2009	2010	2009	2010
Climate	Temperature (°C)	13.60	13.05	13.77	13.74	13.40	13.28	13.06	13.8	13.54
	Precipitation (mm)	846.20	627.20	380.20	535.70	477.90	733.70	186.50	435.86	577.86
Winter wheat	Variety	Keyu line 13	Line 13	Weimai 8						
	Maximum LAI (m ⁻² m ⁻²)	7.5	5.71	5						
	Sowing date (mon/d/yr)	10/24/2003	10/10/2004	10/29/2005						
	Emergency date (mon/d/yr)	11/02/2003	10/21/2004	11/9/2005						
	Harvest date (mon/d/yr)	06/10/2004	06/15/2005	6/11/2006						
Flux	LHF (W m ⁻²)	45.12	46.41	55.39	52.67	59.01	66.20	57.55	39.31	43.62
	SHF (W m ⁻²)	7.58	7.29	-1.66	-7.24	1.56	0.62	9.23	15.02	16.55

¹⁾ LAI, leaf area index; LHF, latent heat flux; SHF, sensible heat flux.

(4) Lines 181-191. Some of these statements, if not all, seem to belong to the Results section. Our results focused on the simulation results and the presentation of the observed data is placed in the method.

(5) Line 191. "were representative of the NCP". Corrected.

(6) 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. The sentence about Lokupitiya et al. (2009) modified to “The SiBcrop version added the crop-specific submodels of maize, soybean, winter and spring wheats, which was simple and detailed enough in predicting LAI (Lokupitiya et al. 2009). The submodel replaces remotely-sensed NDVI

information by simulated LAI.”.

Question 5: 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.

Answer 5: We generally accept the comments.

(1) 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. **This statement moved to “2.3 Methods” section.**

(2) 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. **Deleted and check the full text.**

(3) Line 294-296. Again, I believe these types of statements do not belong to the results section. They should be moved to the discussion. **Deleted.**

(4) Line 309. What is meant by organic matter? **The word changed to “biomass” according to the description in Lokupitiya et al. (2009) .**

(5) 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. The tables were arranged by latitude. The spatial distribution map can be referred to Fig.1.

Question 6: 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 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.

Answer 6: We generally accept the comments.

(1) 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. The English grammar errors will be corrected by English polish company.

(2) Lines 383-389. It is not clear if the authors are discussing their results or contextualizing their findings with other research also conducted in China. The paragraph modified “The spatiotemporal changes of winter wheat phenology had been extensively examined in the NCP. In the period of 1981-2009, the sowing date were on average delayed by 1.5 day/decade, but 8 out of the 36 agro-meteorological experiment stations were advanced (Xiao et al. 2013). The diverse trends in sowing date were also existed at the national scale, where 6 stations significantly advanced by up to 9.1

day/decade, and 11 stations significantly delayed by up to 10 days/decade (Tao et al. 2012).”

(3) Line 399. “a proper”. **Corrected.**

(4) 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? We don’t have the data. **The practices “deep tillage”, “delayed irrigation”, are potential methods to reduce the development rate of winter wheat, which were used to explain why some stations have advanced sowing data under global warming condition. We cannot provide the data, and providing data would distract from the focus of this article, i.e. sowing date.**

(5) Lines 408-409. Please avoid the use of colloquial language “and this affects probably more than we think”. **Deleted.**

(6) Line 410. Figure 5 should be supplemental. **We keep the Fig 5 in the text. Reader need extra work to download supplement containing only one figure.**

(7) Lines 263-264. I’m curious if the authors considered how fallow (rather than corn) would affect the outcome of EP vs. LP. **This is an important comment, especially the difference in the inter-sowing period between the two scenarios. We added a paragraph:** “The strong climate feedback in inter-sowing period, when wheat had been sown in the EP but hadn’t in the LP, was related to the effect of tillage on maize stubble. The NCP is dominated by summer maize - winter wheat rotation system in which the ground is covered with maize stubble before wheat is sown. The damage of sowing to stubble is conducive to the reduction of albedo since stubble have larger surface reflectivity than soil (O'Brien et al. 2019). The 0.1 increase of surface albedo caused by no-till management, which was also the magnitude of our simulation, cooling the hottest summer days by 2 °C or more (Davin et al. 2014). The inter-sowing period is equivalent to

no-tillage period, when early sowed wheat absorbed more net radiation with lower albedo by destroying stubble and causing higher temperature (Fig.3b, Fig4a).”

Question 7: 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.

Answer 7: We generally accept the comments.

(1) Lines 495-505. Easy to follow and well-written. **Thanks!**

(2) 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. **Two paragraphs merge into one body and made minor changes.** “The study had some shortcomings. The single model simulation was highly dependent on the structure and parameterization scheme of the model. The climate feedback was reflected by the canopy temperature. In the SiBcrop model, the spatial distribution of stations was not fully considered in the determination of sowing date, which resulted in too early or too late sowing at some stations. Nevertheless, the study highlighted the divergent climate feedbacks on winter wheat dormancy as affected by sowing date. The simulation error of sowing date in land surface models is commonly higher than 10 days (Song et al. 2013; Chen et al. 2020), which may produce detectable climate effect especially in northern winter and then misestimate the variation of minimum temperature. The findings showed that even when land use/cover type remains unchanged, variations in surface properties caused by sowing date

might still have detectable climatic effects by affecting the surface biophysical process. The conclusion implies that we need to consider not only conversions of land use/cover types but also changes in crop management to understand climate change.”