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 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 surface biophysical process of land management in modulating climate."

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 km^2, 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 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 in Chen et al., 2020. 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.

195 The spatial distribution map can be referred to Fig.1.

196 Question 6: Discussion. This section needs some extra work to improve the quality
197 of the writing.Given the substantial number of edits required, my comments are mainly
198 focused on major points rather than correcting English grammar errors. Lines 383-389.
199 It is not clear if the authors are discussing their results or contextualizing their findings
200 with other research also conducted in China. Line 399. “a proper”. Lines 405-407. To
201 which extent these practices are applied to such a wide surface area? What is the typical
202 farming operation size in this region? Lines 408-409. Please avoid the use of colloquial
203 language “and this affects probably more than we think”. Line 410. Figure 5 should be
204 supplemental. Lines 263-264. I’m curious if the authors considered how fallow (rather
205 than corn) would affect the outcome of EP vs. LP.

206 Answer 6: We generally accept the comments.

207 (1) This section needs some extra work to improve the quality of the writing.Given
208 the substantial number of edits required, my comments are mainly focused on major
209 points rather than correcting English grammar errors. The English grammar errors is
210 corrected.

211 (2) Lines 383-389. It is not clear if the authors are discussing their results or
212 contextualizing their findings with other research also conducted in China. The
213 paragraph modified “The spatiotemporal changes of winter wheat phenology had been
214 extensively examined in the NCP. In the period of 1981-2009, the sowing date was on
215 average delayed by 1.5 days/decade, but 8 out of the 36 agro-meteorological experiment
216 stations were advanced (Xiao et al. 2013). The diverse trends in sowing date were also
217 existed at the national scale, where 6 stations significantly advanced by up to 9.1
218 days/decade, and 11 stations significantly delayed by up to 10 days/decade (Tao et al.
219 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. “There are also management practices to counteract the effects of advanced sowing date, such as deep tillage and delayed irrigation, which reduce the development of leaves and stems. Until now, fewer studies had focused on the phenomenon of early sowing date and its underlying causes and countermeasures.”

(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. Figure 5 moved to Supplement Fig.2. and added location labels.

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

244 management, which was also the magnitude of our simulation, cooling the hottest
245 summer days by 2 °C or more (Davin et al. 2014). The inter-sowing period is equivalent
246 to no-tillage period, when early sowed wheat absorbed more net radiation with lower
247 albedo by destroying stubble and causing higher temperature (Fig.3b, Fig4a).” . The
248 reflectivity of different surface coverings in near-infrared and visible bands in the
249 SiBcrop model was provided in Table 6.

250 Question 7: Conclusion. Lines 495-505. Easy to follow and well-written. Lines
251 506-519. Needs some extra work. Please merge these two paragraphs into one body.
252 The highlights of this passage should be (i) the limitations of this study, which I agree
253 is the lack of consideration of how the locations were spatially distributed, and (ii) the
254 management implications of this work.

255 Answer 7: We generally accept the comments.

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

257 (2) Lines 506-519. Needs some extra work. Please merge these two paragraphs
258 into one body. The highlights of this passage should be (i) the limitations of this study,
259 which I agree is the lack of consideration of how the locations were spatially distributed,
260 and (ii) the management implications of this work. Two paragraphs merge into one
261 body and made minor changes. “The study had some shortcomings. The single model
262 simulation was highly dependent on the structure and parameterization scheme of the
263 model. The climate feedback was reflected by the canopy temperature. In the SiBcrop
264 model, the spatial distribution of stations was not fully considered in the determination
265 of sowing date, which resulted in too early or too late sowing at some stations.
266 Nevertheless, the study highlighted the divergent climate feedbacks on winter wheat
267 dormancy as affected by sowing date. The simulation error of sowing date in land
268 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 crop management changes as a potential way should be considered in mitigating climate warming. In the cold dry north, delayed sowing and reduced irritation would alleviate the temperature increase in winter, whereas in south with better hydrothermal conditions, enhanced vegetation coverage would be beneficial."

Reply to **Anonymous Referee #2**

We thank the reviewer 2 for the overall positive comments on our research. The specific comments on grammar, figures, tables, Introduction, Methods, Results and Discussion greatly improve the article quality. The reply is as follows.

Question 1 ： Figure 3 indicates there is a lot of variability in the response to sowing date across sites. The effect of climate differences across the gradient of sites examined is likely very important. The approach to analyzing the effect of Ta and P on the modeled Tc are not described in the methods as far as I can tell. I think the effects across climate should be important based on how this study was framed, so that analysis deserves more attention.

Answer 1 ： The method was added in section "**2.2.1 Meteorology**": "Climatological mean T_a and accumulated P during the wheat growth period were calculated in the 10 stations and were linearly regressed with the simulated differences between scenarios."

Question 2: The overall approach of the simulation experiment is a bit confusing to me. Since the study sites are so widely distributed in space (and climate), why apply a constraint to the sowing date that doesn't account directly for the variability in climate? As you describe, this leads to the northern sites and southern sites "shifting" sowing dates in opposing directions compared to the known phenology (becoming earlier at some sites and later at others). You suggest early in the paper that the trend in sowing dates overall is likely to be a delay due to the extension of warmer conditions later in the year. I'd like to see this choice more clearly justified and contextualized.

Answer 2: the reason was justified in secion "**2.3.2 Model simulation**": "The SiBcrop model was modified to be more cold tolerance (section 2.3.1), which causing

the sowing date was less controlled by temperature. The climate variability among stations has less constraint on sowing date. Our previous study showed that the delayed sowing date of winter wheat was mainly caused by the delayed harvest of maize in the NCP (Xiao et al. 2013). The sowing date in the two scenarios is within the climatological average of the region."

Question 3: I am left wondering about the impact of snow cover on the response of energy balance during the winter dormant period at these sites. The effect of snow at other sites in other studies is discussed, but the characteristic snow cover across this geographic region is never explicitly stated here. Is snow cover an important feature and is it included in the model? If so, why doesn't it affect radiative balance in the dormant season as elsewhere?

Answer 3: We thanks the comments. The snow is a very important factor influencing the surface albedo in winter. But in our simulation, the two scenarios had no difference in snow coverage. So we added some sentence for explanation: "Previous studies showed that the increase of vegetation cover caused warming feedback by destroying the high albedo of snow in the case of snow cover (Richardson et al. 2013; Bagley et al. 2015; Lombardozzi et al. 2018). In our simulation, except for the large difference in crop coverage in phase 1, the snow and crop had consistent coverages in other phases (Supplement Table 1), which means albedo difference between two scenarios was not caused by snow.

Question 4: Could you be more specific about the management implications of this study? For example, can you speculate about how the modeled changes in LAI impact yield, which was discussed as an important factor in changing management practices early on in the paper.

Answer 4: We detailed the management implication of the Conclusions. The

previous version were too broad. The last paragraph was modified into "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 crop management changes as a potential way should be considered in mitigating climate warming. In the cold dry north, delayed sowing and reduced irritation would alleviate the temperature increase in winter, whereas in south with better hydrothermal conditions, enhanced vegetation coverage would be beneficial."

Question 5: Introduction: Since it is such an important piece of understanding to your study, I think a short overview of the annual lifecycle of winter wheat should be included in the introduction, perhaps even with a diagram indicating the critical period between sowing date and dormancy period that is the focus of your study. As you later describe in your results and discussion, there are significant differences one would expect as a result of different sowing times during the winter and growing season which would be helpful to explicitly state early on. Lines 59-62: Unclear which study these numbers come from. Please clarify references. Lines 66-67 By what management approaches were these various stages changed? Line 68 This statement needs support or a qualifier, eg if referencing changes due to climate, "These phenology changes are likely to benefit yield." or if referencing changes due to management, "These management strategies that shift phenology are intended to increase yield." Line 83: This way of stating the changes to latent and sensible heat is a bit confusing. Can these changes just each be explicitly listed for clarity? Do you mean ET here? Line 92: perhaps change to ": : :, a shift in radiative forcing with the

potential to warm the atmosphere by 1-1.4 C through declining evapotranspiration"? Line 103: Should this be "widely" instead of "wildly"? Line 110: Not sure how the effects last longer. Not supported in immediately following sentences

Answer 5: We generally accept the comments.

(6) Introduction: Since it is such an important piece of understanding to your study, I think a short overview of the annual lifecycle of winter wheat should be included in the introduction, perhaps even with a diagram indicating the critical period between sowing date and dormancy period that is the focus of your study. As you later describe in your results and discussion, there are significant differences one would expect as a result of different sowing times during the winter and growing season which would be helpful to explicitly state early on. The key phenology was marked in Fig.2 and interpreted in the text.

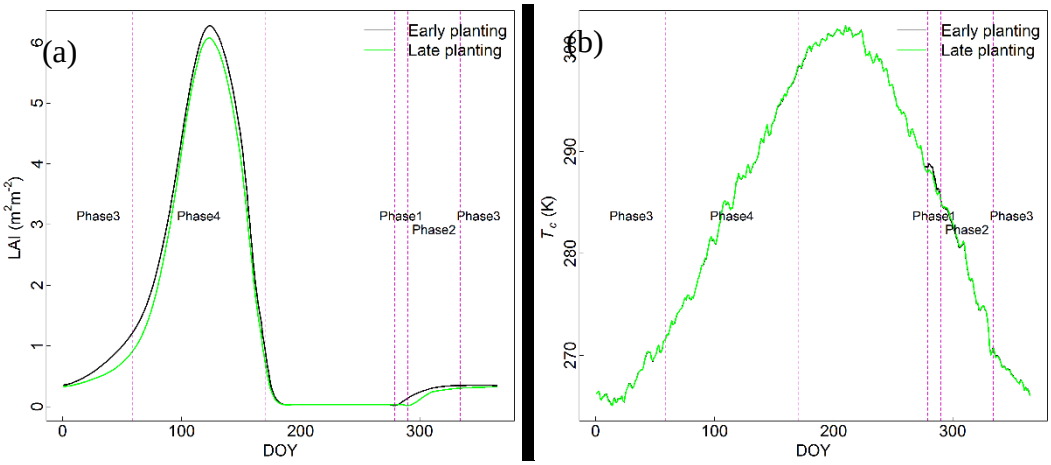

Fig.2 Dynamics of (a) LAI and (b) T_c under two sowing scenarios in winter wheat growing season

Phase 1: inter-sowing period, when wheat had been sown in the EP but hadn't in the LP; Phase 2: early growing period, from sowing date of LP to dormancy date; Phase 3: dormancy period, from dormancy date to re-greening date; Phased 4: late growing period, from re-greening date to maturity date.

In the section "**3.2 Seasonal dynamics of LAI and T_c in scenarios**", we added "According to the T_c difference between scenarios, the following phenologies of winter wheat were relatively important: sowing date, dormancy date, re-greening date and maturity date. Based on the simulation results, the phenological dates used here as follows: EP sowing date, DOY279; LP sowing date, DOY290; dormancy date, DOY334; re-greening date, DOY59; maturity date, DOY170 (Fig.2a). The T_c difference between scenarios was separated into 4 phases: Phase 1, inter-sowing period, when wheat had been sown in the EP but hadn't in the LP; Phase 2: early growing period, from sowing date of LP to dormancy date; Phase 3: dormancy period, from dormancy date to re-greening date; Phased 4: late growing period, from re-greening date to maturity date (Fig.2b)."

(7) Lines 59-62: Unclear which study these numbers come from. Please clarify references.

Added reference. "In the North China Plain (NCP), the dates of sowing, dormancy, re-greening, anthesis, and maturity in wheat system were changed by 1.5, 1.5, -1.1, -2.7, and -1.4 days/decade (a positive value indicates delay and a negative value indicates advance), respectively (Xiao et al. 2013)."

(8) Lines 66-67 By what management approaches were these various stages changed?

We added including sowing data adjustment and varietal change. "Crop management, including sowing date adjustment and varietal change, reduced the lengths of vegetative stage, but increased the length of reproductive stage (Liu et al. 2010; Liu et al. 2018)."

(9) Line 68 This statement needs support or a qualifier, eg if referencing changes due to climate, "These phenology changes are likely to benefit yield." or if referencing

changes due to management, “These management strategies that shift phenology are intended to increase yield.”

We accepted the comment. The statement changed to “The management induced phenology dynamics are intended to increase yield”.

(10) Line 83: This way of stating the changes to latent and sensible heat is a bit confusing. Can these changes just each be explicitly listed for clarity? Do you mean ET here?

Modified. New sentence: “Earlier planting date and longer grain-filling period increased the *LH* by 3 W m^{-2}, decreased *SH* by 2.5 W m^{-2}, in June and enhanced the net radiation (R_n) by 1.2 W m^{-2} in October by reducing the interval time from maturity to harvest in American Corn belt (Sacks and Kucharik 2011).”

(11) Line 92: perhaps change to “: : :, a shift in radiative forcing with the potential to warm the atmosphere by 1-1.4 C through declining evapotranspiration”?

Comment accepted, the sentence changed to “Harvest shifted the key influence factors of the radiative balance and evaporative fraction from leaf area and soil-atmosphere temperature difference to soil moisture in U.S. winter wheat (Bagley et al. 2017), and a shift in radiative forcing with the potential to warm the atmosphere by 1~1.4 ℃ through declining *LH* in the NCP (Cho et al. 2014).”

(12) Line 103: Should this be “widely” instead of “wildly”? Yes, Widely. Thanks!

(13) Line 110: Not sure how the effects last longer. Not supported in immediately following sentences

The sentence modified to “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 6: Methods: This is only a personal preference, but I find it difficult to interpret the climate data in a table and perhaps the range of variation in sites could be more clearly conveyed in a figure? Table 1. The label for "P" seems to be cut off. Table 2. Was canopy temperature measured or modeled at Yucheng, I am a bit confused by the caption description Lines 177-180 I suggest adding in the range of time periods as DOY, perhaps parenthetically to the months, to be consistent for reader to compare to sowing date. Also, I think there should be a reference to Table 3 here. Table 4. I suggest somehow highlighting (bold or shading) the significant trends in this table. Lines 214-220: Could you please provide a bit more detail as to why the original model is so different? Was it developed for warmer climates, hence the lower cold tolerance in the modifications? A very brief summary of how Chen et al 2020 came to these modifications would be useful. Line 251: Please define alpha here as well. I assume albedo.

Answer 6: We generally accept the comments.

(7) This is only a personal preference, but I find it difficult to interpret the climate data in a table and perhaps the range of variation in sites could be more clearly conveyed in a figure?. We've arranged our stations from high to low latitude to make it easier for readers to spot patterns.

(8) The label for "P" seems to be cut off. Corrected.

(9) Table 2. Was canopy temperature measured or modeled at Yucheng, I am a bit confused by the caption description. The data in the table are all measurements used to calibrate the model.

(10) Lines 177-180 I suggest adding in the range of time periods as DOY, perhaps parenthetically to the months, to be consistent for reader to compare to sowing date. Also, I think there should be a reference to Table 3 here. We accepted the comments. The DOY and reference added. "The phenology information was obtained from China agro-meteorological experiment stations and available in the period of 1981-2009, except for 2003 at Zhumadian and 1986 and 1988 at Miyun station (Table 3)."; "Winter wheat dormancy stage generally begins in DOY 330-360 (December) and ends in DOY 40-70 (late February and early March), and reaches maturity in DOY 150-160(mid-June). The standard deviation shows that the inter-annual fluctuations of dormant and re-greening period is larger, and harvest period is relatively stable.

(11) Table 4. I suggest somehow highlighting (bold or shading) the significant trends in this table. Bolded.

(12) Lines 214-220: Could you please provide a bit more detail as to why the original model is so different? Was it developed for warmer climates, hence the lower cold tolerance in the modifications? A very brief summary of how Chen et al 2020 came to these modifications would be useful.. We briefly explained the reason in section "**2.3.2 Model simulation**": "The SiBcrop model was modified to be more cold tolerance (section 2.3.1), which causing the sowing date was less controlled by temperature. The climate variability among stations has less constraint on sowing date. Our previous study showed that the delayed sowing date of winter wheat was mainly caused by the delayed harvest of maize in the NCP (Xiao et al. 2013). The sowing date in the two scenarios is within the climatological average of the region.

(13) Line 251: Please define alpha here as well. I assume albedo. Defined in the section "**1. Introduction**"

Question 7: Discussion: It seems like the albedo results should be included in the

results rather than the discussion section. In general, it is a little confusing throughout this section to determine when the authors are discussing the results of this study versus other studies. Again, I am left wondering what exactly the snow regime is at these sites (and does it vary across the gradient), since it is so important in understanding dormant season energy partitioning in other studies. I also think it would be nice to have a brief discussion on how this choice of model could influence results compared to other models. Figure 5. Where do these photos come from? Line 444 - 445: This sentence is confusing, please rephrase Lines 476-477: Needs a reference and also more specificity on what kind of ecosystems this refers to.

Answer 7: We generally accept the comments.

(3) Again, I am left wondering what exactly the snow regime is at these sites (and does it vary across the gradient), since it is so important in understanding dormant season energy partitioning in other studies. We provide the snow and crop coverages in 4 phases at each station in Supplement Table 1. The data show little difference in coverage. "Previous studies showed that the increase of vegetation cover caused warming feedback by destroying the high albedo of snow in the case of snow cover (Richardson et al. 2013; Bagley et al. 2015; Lombardozzi et al. 2018). In our simulation, except for the large difference in crop coverage in phase 1, the snow and crop had consistent coverages in other phases (Supplement Table 1), which means albedo difference between two scenarios was not caused by snow."

(4) I also think it would be nice to have a brief discussion on how this choice of model could influence results compared to other models. We realized that "The single model simulation was highly dependent on the structure and parameterization scheme of the model.". And we compared the published results with our simulation in section "**4.2 Warming effect of EP-LP in the dormancy period**". "Although there were

literatures reporting that the albedo process in winter is relatively important (Richardson et al. 2013; Lombardozzi et al. 2018), fewer studies directly addressed the influence of different surface characteristics and climate effect through biophysical process in the dormancy period. In the Oklahoma's winter wheat belt, the rapid crop growth during November exhibited a distinct cool anomaly against adjacent regions of dormant grassland. Over the period of December through April, the cool bias was visibly diminished although the greenness difference between grassland and wheat was more distinct (McPherson et al. 2004). The biophysical impacts between maize and perennial grass were simulated using Agro-IBIS model in US corn belt (Bagley et al. 2015). The results showed that much higher LAI of perennial scenario was existed in winter December–February (3 vs 0 m^2 m^{-2}) and in summer June–August (10 vs 4 m^2 m^{-2}). Perennial grass had smaller surface albedo (coupling snow effect) than maize in winter, but showed quite small difference in summer. During winter and summer, the perennial scenario had slightly higher *LH* than the maize scenario, but the difference in R_n between two scenarios was more than 10W m^{-2} in winter (Bagley et al. 2015). The above studies indicated that the cooling effect of higher LAI was inhibited in winter. The results of this current study indicate that higher LAI in winter has a warming effect. The main reason was due to the relative contributions of surface albedo mechanism and surface flux distribution process.

(5) Figure 5. Where do these photos come from?. Figure 5 moved to Supplement Fig.2. and added location labels.

(6) Line 444 - 445: This sentence is confusing, please rephrase. Rephrased. “In the SiBcrop model, the reflectivity of different surface coverings varies greatly in the visible band (Table 6). The germination of winter wheat immediately changed the bare soil into soil with crop, which is favorable to the sharp reduction after crop covered.”

(7) 476-477: Needs a reference and also more specificity on what kind of ecosystems this refers to. New sentence is: “Previous studies showed cooling effect in the photosynthetic active period through surface biophysical mechanism in the cropland (e.g. (Sacks and Kucharik 2011; Zhang et al. 2013; Bohm et al. 2020)).