

## ***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 #2**

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This manuscript reports the results of a modeling experiment to assess the impact of shifts in sowing date of winter wheat on surface energy balance and canopy temperature as mediated by changes in plant phenology in the North China Plain. The authors use a model that has been recently modified to better represent winter wheat systems in this geographic region and constrain model runs into early and late sowing scenarios run at ten sites distributed across the North China Plain and forced with measured meteorological data from 1980 – 2012. This is an interesting and significant topic to pursue with global implications. They find overall that earlier sowing leads to more biomass production in the early winter and a higher LAI during the dormant season that leads to warming via changes in albedo, whereas the higher LAI during the growing season

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as a result of earlier sowing leads to a cooling effect due to increased latent energy partitioning of radiation. The paper is generally easy to follow, although I suggest a thorough edit for grammar throughout. While a valuable study, I think the manuscript should undertake major revisions prior to publication. Below I outline some points of general feedback, followed by feedback on specific sections with general comments and more fine-scale line edit suggestions and questions.

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.

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.

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?

4. Could you be more specific about the management implications of this study? For

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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.

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

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

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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.

Results: Line 274: Would be nice to include the verification data here as a supplement. Lines 288-291: This sentence is difficult to follow, please rephrase. By "positive difference" do you mean were delayed or accelerated relative the actual dates? Line 302: This phrasing seems to overstate the difference in LAI, I suggest rephrasing Line 309: I suggest specifying "aboveground" here, or even changing from "organic matter" to "biomass". Lines 312-316: As mentioned above, more detail on this analysis should be included in the methods.

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.

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