**Biogeosciences
Discussions**

# *Interactive comment on* "Coupling land surface and crop growth models for predicting evapotranspiration and carbon exchange in wheat-maize rotation croplands" *by* H. Lei et al.

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Referee #1

General Comments:

This paper has coupled the Simple Biosphere model 2 (SiB2) with a crop phenology and physiology scheme based on SiBcrop (Lokupitiya et al., 2009, this journal). The new coupled model (a name is expected) was then rigorously evaluated at two agricultural sites (both having wheat-maize rotation croplands) in North China Plain against both satellite-based LAI and comprehensive in-situ observations, with a particular emphasis on the simulating latent heat and carbon fluxes. The topic of this manuscript is

quite important, since the dynamical vegetation descriptions are indispensable for land surface models to assess climate-change impacts. In my opinion, this study should be encouraged, since it can be very helpful to the climate change adaptation for crop production in China.

Response: We appreciate these positive comments very much. We have not given a name to our coupled model because it is only a part of an integrated project which has not been finished yet.

(Continued general comments) However, the differences between the new coupled model and the previous SiBcrop should be further discussed and clarified. The unique features of the new coupled model should be emphasized in the abstract and the conclusions.

Response: The details are in the response to Comment 3.

(Continued general comments) In general, this manuscript is well written and organized; and the conclusion is convincing and interesting to me. Based on my review, I would like to recommend this manuscript be accepted for publication in Biogeosciences after minor revision.

Response: We appreciate these positive comments very much.

Specific comments:

Comment 1: Page 5158, line 26 "more" should be "much"

Response: We accepted it and revised the word.

Comment 2: Page 5159, line 26 "Wang et al., 2007" is missing in the reference list.

Response: We added this to the reference list.

Comment 3: Page 5160, lines 15-18 Further discussions are needed to clarify the physical differences between the new coupled model with the SiBcrop. As we know,

SiB2 has incorporated a realistic canopy photosynthesis-conductance model, which is a significant improvement over SiB. As a result, the advantage of the new coupled model over the SiBcrop is expected. Please make some quantitative and/or qualitative comparisons between the two models.

Response: The SiBcrop was developed based on the SiB3 (not the SiB) and the crop phenology schemes for soybean, maize, and wheat in the US. The SiB3 is the latest version of SiB. The essential parts (e.g., photosynthetic carbon assimilation, stomatal conductance, flux exchange, resistances, etc.) of the two versions (SiB2 and SiB3) are almost the same. Based on the SiB2, some dominant or necessary processes were introduced in the SiB3 for specific regions such as the Amazon forest, the cold climate regions; for specific goals such as the simulation of carbon isotope discrimination, carbon budget (Baker et al., 2008, JGR; Schuh et al., 2010, BG; Suits, et al., 2005, GBC; Baker et al., 2003, GCB). Considering our research topics and the characteristics of our study region (the temperate climate region), we included a soil respiration model and an improved treatment of soil hydrology in our coupled model. Meanwhile, our comparison has shown that the SiB2 is accurate in simulating energy and carbon fluxes with observed LAI as input in our study (in preparation as another paper). Therefore, it is not necessary to compare the behaviors of the SiB3 and SiB2 quantitatively, in order to better focus on the topics. On the other hand, the current version of SiBcrop was developed to be used within the continental United Sates, and was evaluated with three AmeriFlux eddy covariance flux tower sites. The behavior of this model is not evaluated comprehensively for the cropland planted with winter wheat-summer maize in rotation in the North China Plain. This is the main difference between the SiBcrop and ours. We added this qualitative comparison to Section 1.

Comment 4: Page 5162, line 1. How is the vegetation cover change represented in the study? Please clarify it.

Response: The vegetation is constant for each vegetation biome in the SiB2. To better represent the dynamics of vegetation cover in row crops, it is estimated simply through

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the Beer's law which is expressed as: vegetation cover = 1-exp(1-kLT), where LT is the total leaf area index (consists of green leaf area, dead leaf area, and stem area in the SiB2), k is the extinction coefficient and is commonly equal to 0.5. We added these explanations to Section 2.1.

Comment 5: Page 5162, line 8 "emergency" should be "emergence"

Response: We accepted it and revised the word.

Comment 6: Page 5165, lines 1-2, "the groundwater table was deeper than 30 m because of well irrigation". Does the irrigation water come from groundwater in this region? If not, the groundwater table should be relatively shallow due to lots of irrigation recharges.

Response: The irrigation all comes from groundwater at the Luancheng site. In the region where the Luancheng site is located, almost all the rivers are dried up. The available water from the rivers is too limited to maintain the irrigation. Accordingly, there are many deep wells in this region. With the large irrigation withdrawals, the groundwater table is decreasing sharply. For clarity, we revised this sentence to "The groundwater table level was deeper than 30 m because of the large groundwater withdrawals for irrigation". See Section 3.

Comment 7: Page 5165, line 12 "analyze" should be "analyzer"

Response: We accepted it and revised the word.

Comment 8: Page 5166, lines 5-6 "the ability to use the model to simulate..." should be "the ability of the model to simulate..."

Response: We accepted it and revised this sentence.

Comment 9: Page 5166, lines 7 "concerning the impact" should be "concerning over the impact"

Response: We accepted it and revised this sentence.

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Comment 10: Page 5167, lines 7-10, "The 1km/monthly NDVI-based LAI has much lower peak value and was worse synchronous with the observed LAI (Fig. 2). This could be attributed to the interpolation scheme from monthly to daily values". What is the temporal resolution of the LAI observations? How did you compare the observed LAI with the simulated LAI (hourly or half-hourly) as well as satellite-based LAI values (8-day or monthly), which have quite different temporal scales? Did you interpolate all the values into daily values for the comparison?

Response: Normally, LAI was measured biweekly at the Weishan site, and was measured once a week at the Luancheng site. The simulation time step for LAI was daily. The satellite-based LAI values (8-day or monthly) were interpolated linearly to daily values. The daily simulated LAI and daily satellite-based LAI in the days when LAI was measured were selected to compare with the measured LAI. The simulated and satellite-based LAI were compared using their daily values. For clarity, we added the temporal resolution of the LAI observations in Section 3, and added the comparison method in Section 5.1.

Comment 11: Page 5167, line 26 Suggest changing "The seasonal variation..." into "For both sites, the seasonal variation..."

Response: We accepted it and revised the sentence.

Referee #2

General comments: The paper proposes a coupled land surface and crop growth model that can estimate an energy budget including ET and CO2 flux by considering the response of crop phenology and physiology to environmental change. The validity of the model is examined at two EC measurement sites on a wheat-maize rotation field. Simulation results for ET and yield at different irrigation amounts and CO2 concentrations are shown and discussed. The subject of the paper is important and the results could be useful for many readers; in particular, the simulation results could be well expressing the actual behavior of long-term evapotranspiration and CO2 exchanges on

cropland, which have not previously been expressed using only LSM.

Response: We appreciate these positive comments very much.

Comments and questions for the authors are as follows:

Comment 1: Page 5162 Lines 18-22: Carbon allocation pattern to each part with GDD could be important in this coupled model since it strongly controls LAI and grain biomass. It seems that the patterns in Fig. 1 were determined based on past experimental results (and seems to linearly connect some plots); however, the practical step is unclear. I think only an artificial determination of this pattern to fit LAI should be avoided. Please provide a more detailed explanation about how these allocation patterns with GDD were determined.

Response: The carbon allocation pattern to each part with GDD does strongly control LAI. Therefore, the relationship between allocation fractions and GDD for winter wheat and summer maize was directly from the literatures whose measurements were carried out in the same region as our study. It is not determined by fitting the simulated LAI to the observed one. Zhang et al. (2002) reported the relationships for summer maize under different irrigation conditions. However, these were drawn in the figures. Therefore, the relationship under no soil water stress condition was digitized, and was then used in our model. In Qiao et al. (2002), the relationship for winter wheat under the condition of no soil water stress was directly summarized in the table. We added these descriptions in Section 2.2.

Comment 2: Fig. 4: Calculated latent heat flux from the coupled model seems to be overall larger than the observation especially during the summer (maize) season; although I think it is within an acceptable range to apply the model for later simulation, it is necessary to discuss the reason. A related point: it would be valuable to provide a brief description about the energy budget closures at the two EC measurement sites in this paper. Soil heat flux at 0-3 cm depth should be estimated if it is possible from soil temperature measurements, etc.

Response: Yes, the simulated latent heat flux was larger than the observation during the summer maize season at the Weishan site. The reason is that the soil evaporation was over-predicted. Maize has lower vegetation cover than wheat, so the effect of over-prediction of soil evaporation is more significant in the maize season than in the wheat season. The equation for calculating soil evaporation in the SiB2 is an empirical equation which was calibrated in grassland. Our observed soil evaporation data showed that the SiB2 significantly over-predicted the soil evaporation. Since the soil evaporation simulation has slightly indirect influence on the carbon and water processes in the vegetation (for our sites where soil water is sufficient), this result was not presented in the manuscript because it is not relevant to the topic very much. Another reason for the over-prediction may be the under-measurement of latent heat flux. The slope of the linear regression between (Rn-G) and (LE+Hs) is 0.75 and 0.87 for the Weishan (half-hourly data) and Luancheng (hourly data) sites, respectively. The soil temperature measurements are available at the depths of 5 cm and deeper at the Weishan site. Therefore, calculation of the soil heat storage at 0-3 cm is hard. The situation is similar at the Luancheng site. Figure 4(d) and Figure 5(d) only illustrated that the magnitude of simulated soil heat flux was consistent of the observation. We added this discussion to Section 5.2.

Minor remarks:

Comment 1: Fig. 1: It would be helpful to show these fractions are "under unlimited water conditions" (alpha) in the figure or caption.

Response: We added this suggestion to the caption of figure 1.

Comment 2: Eqs (1)-(4): The change in the allocation fraction to leaf by light use availability on original function seems to be unused. Is it negligibly small?

Response: From the equations (9)-(11) in Arora & Boer (2005), the impact of light availability (expressed by LAI) on allocation fractions is significant. However, the reason why we did not introduce the influence of light availability on the observed allocation

fractions is that the observed carbon allocation fractions with the change in GDD have already included the impact of light availability on them. According to the equations (7)-(11) in Arora & Boer (2005), the allocation fraction for leaf will decrease with the increase of LAI. This relationship is consistent with the observed relationship between carbon allocation fractions and GDD used in our study (Fig. 1). For clarity, we added these explanations in Section 2.2.

Comment 3: Page 5165 line 8-12: LAI are measured both directly by random sampling and indirectly using LAI-2000. Please identify which measurements are used in the comparison with the model at each time-series in Figs. 2 and 3. This is related to the discussion on Page 5167 lines 10-13.

Response: We used two different symbols to identify the two different measurements in Fig 2.

Comment 4: Page 5165 line 26 "the coupled model was run half-hourly": Allocation of accumulated carbon was calculated half-hourly or daily? It would also be helpful to show time-intervals for the calculations of growth/maintenance respiration and the impact of water stress on carbon allocation.

Response: In the coupled model, allocation of accumulated carbon was calculated daily, as well as the calculations of growth/maintenance respiration and the impact of soil water stress on carbon allocation. We made it clear in Section 4.

Comment 5: Some of the light-grey lines in the figure are barely visible (e.g. Figs. 2, 3, 6. and 8).

Response: we revised these figures to make them more visible.

Comment 6: Page 5169 line 11-16: Differences of water stressed duration and their effects on LAI, ET and Yield could be an interesting result from the simulation. If I understood correctly, water stress in the coupled model affects not only through carbon allocation fractions but also stomatal conductance. It would be valuable to show the thresh-

old of SWC for stomatal closure (or a rough estimation of photosynthesis/transpiration decline at the SWC=0.22 level).

Response: The threshold of soil water content for the adjustment of carbon allocation fractions and incipient stomatal closure is assumed to be identical. In the SiB2, the soil water stress factor for both photosynthesis and stomatal conductance was assumed to be an exponent function of soil moisture potential. However, this inhibition factor was suggested to be calculated as a linear function of soil water content between the critical soil water content and the wilting point (Colello et al., 1998). We adopted this suggestion and used an observed threshold to be the critical soil water content. We added this explanation in Section 2.1

Other Revisions made by us:

(1) We updated the volume and article number of some references whose status was in press when we submitted this manuscript.

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