Reviewer #1

Comment R1.1:

It is impressive that the authors made a significant contribution and provided a comprehensive validation from various perspectives. I can see a lot of implications based on this work. Its contribution to the model development is worth being published in GMD. However, although the manuscript has improved a lot after a major revision, I think a substantial revision is still required before acceptance.

Response R1.1:

Many thanks for these positive comments and for your in-depth review of the manuscript. We did our best to further revise the manuscript.

Comment R1.2:

Although the title mentions a new crop scheme, I didn't see a explicit introduction to it in the main text. Moreover, there is no independent validation of the new crop scheme (i.e., correlation coefficients and RMSD between ISBA_pheno and ISBA_ref). Moreover, it is trivial to mention SURFEX_v8.1. I suggest to enrich the contents for the new crop scheme only (details see some comments below) and remove SURFEX_v8.1 from the title.

Response R1.2:

We agree that the way we presented ISBA_pheno results could give the wrong impression that the new crop phenology scheme results are not presented. The title could actually be improved because the meaning of « crop scheme » is not clear. The manuscript already contains specific results for the « ISBA_pheno » experiment. Differences between ISBA_ref, ISBA_pheno, and ISBA_pheno_irr are shown in Figures 5, 6, 8, 9, 11, S3.5, and in Table 4. Score maps in Figures 6-10 and 12 are shown for ISBA_pheno_irr only because the score maps are nearly identical as for ISBA_pheno. This is indicated at the end of section 3.3: 'The ISBA_pheno correlation and RMSD differences with respect to ISBA_ref are nearly identical to those showed for ISBA_pheno_irr in Figs. 6-7 (not shown).' Moreover, quantitative differences in the performance of ISBA_pheno and ISBA_pheno_irr are presented in Sections 3.2 and 3.4.
In order to further clarify this, we:
- (1) changed the title from
'Implementation of a new crop and irrigation scheme in the ISBA land surface model using SURFEX_v8.1'
to
'Implementation of a new crop phenology and irrigation scheme in the ISBA land surface model using SURFEX_v8.1'.
In order to be consistent with the title, 'crop and irrigation scheme' was replaced by 'crop phenology and irrigation scheme' everywhere in the text. Including « SURFEX_v8.1 » in the title was requested by the executive Editor. This is needed to follow GMD guidelines. The paper was submitted to the SURFEX special issue.
- (2) created a new subsection in Section 2.2, dedicated to 'new crop phenology processes'.

Comment R1.3:

Sect 2.1: It is not necessary to introduce the details of the SURFEX platform. And please try to introduce something directly. For example, it is not necessary to mention ECOCLIMAP-II when this study used ECOCLIMAP-SG. Moreover, this sentence should appear in the section of experiment setting up.

Response R1.3:

We agree. We deleted part of the text of Section 2.1 and moved part of it to Sections 2.2.1 and 2.3. Since this paper is for the GMD SURFEX special issue, we need to keep a brief description of the SURFEX framework.

Comment R1.4:

There are a lot of overlaps between section 2.2 and 2.2.1. And it is still unclear for me the key differences between the old and the new scheme. Please first simply introduce the key processes implemented by the former crop and irrigation schemes without any details (i.e., parameters, formula of SWI, etc) and emphasis the key limitations of them which will be solved by the new scheme.

Response R1.4:

We created a new subsection in Section 2.2, dedicated to 'new crop phenology processes' (see Response 1.2) and we slightly reduced the old section 2.2.1. The site-level processes represented by the new irrigation scheme are not fundamentally different from the old one. The novelty is that the new scheme is able to spatialize irrigation, to manage harvest dates, to handle several irrigations types over several vegetation types, and is interoperable with the most recent versions of the soil water diffusion model. A lot of technical work was done to achieve this result.
In order to clarify this
'In this study, a pre-existing simple irrigation scheme (Calvet et al., 2008) within the ISBA LSM is upgraded to build a new version able to work at a global scale and to represent several types of irrigation practices' (Section 2.2)
was replaced by
'An old irrigation scheme working at a local scale (Calvet et al., 2008) is available in the ISBA LSM. Major limitations of the old scheme are the lack of (1) spatialization at a global scale, (2) representation of harvest, (3) diversity of irrigation types and irrigated vegetation types, (4) interoperability with the multi-layer soil hydrology scheme. Key processes implemented in this scheme are briefly described below.'.
Moreover, 'The yearly sum of irrigated water can be compared to the USGS data described in Section 2.4.3. The irrigation water flux is evenly distributed over a period of time of 8 hours (by default) and is applied on top of the vegetation canopy like precipitation. The irrigation water can be intercepted by vegetation canopy.'
was moved to new Section 2.2.2.

Comment R1.5:

Sect 2.2.2: Although the aggregation rule is new, its key aim is to reduce the computing burden. Thus I suggest to simply introduce the aggregation process here and move the details to the supplementary materials. Moreover, the last paragraph talks about the water supply (unlimited) for irrigation. It may belong to Sect. 2.3.

Response R1.5:

We agree. This part of the « new aggregation rules » section was moved to Supplement 1, together with Fig. 3. The last paragraph was moved to Section 2.3.

Comment R1.6:

If authors underline the difference of metrics values rather than their spatial patterns, some figures can be simplified (optional). a. Figure 6, 8, and 9: This three can be assembled together by combining their top panels together and a box plot (or a violin plot) aside instead of two associated images. b. Figure 7, 10, and 12: They can be assembled together by making three subplots with time series of boxes or violins.

Response R1.6:

Thanks for this suggestion to improve the readability of the paper. In response to comment R1.9, we removed Figures 9 and 10 related to GLEAM. We reorganized former Figs. 6 and 8. LAI and GPP time series are now in the same Figure (new Fig. 5). LAI and GPP score differences are now in the same Figure (new Fig. 6).

Comment R1.7:

Supplement S5 is not needed.

Response R1.7:

We believe that the technical Supplement 5 is needed by SURFEX users using the new scheme or willing to reproduce our experiments. Supplement 5 is cited in Section 2.2.

Comment R1.8:

Two issues regarding validation must be dug out. Firstly, irrigation amount is highly influenced by precipitation, which however is poorly reproduced in all reanalysis data. I'm not surprise that the fairly performance of ERA5 precipitation (Fig. S4), which may lead to vast bias between simulated irrigation and census data. Thus, I suggest to show how simulated irrigation amount is improved by the new scheme in comparison to that based on the old scheme rather than to demonstrate the fit between simulation and census data.

Response R1.8:

The precipitation quality issue is briefly discussed in Section 4.1. In response to Reviewer 2 (see response R2.3) we completed Supplement S4 to better capture the shortcomings of ERA5 precipitation.

Comment R1.9:

Secondly, there are significant mismatches among ET products. Although assimilated with a lot of observations, the ET from GLEAM is a model output, not observations. The model have a very coarse representation of plant functional types, and irrigation processes is not taken into account (at least for v3.2b). So I don't think it is suitable to call it 'observations' (e.g., in Figure 9). Furthermore, it is not suitable to validate the simulated ET by GLEAM. Nothing can be demonstrated if the simulation is compared to an unreliable product. I suggest to remove this part. Other parts are sufficient to demonstrate the advances of the new scheme.

Response R1.9:

We agree. We removed Section 3.5 and Figures 9 and 10 related to GLEAM. Section 4.2 was revised accordingly. This will further improve the readability of the paper.

Comment R1.10:

Line 49-50: There is still a large uncertain in terms of ET-precipitation feedback. It could either positive (rain prefers wet soil) or negative feedback (rain prefers dry soil), which relies on numerous factors (e.g., surface heterogeneity, atmospheric boundary conditions, wind speed/ direction, spatial scales, etc). So I suggest to underline the contribution of irrigation to ETprecipitation feedback but avoid mentioning where the precipitation may occur.

Response R1.10:

We agree.

'Water vapour originating from large scale irrigation water supply can be recycled to rainfall and affect non-irrigated areas'

was replaced by

'Water vapour originating from large scale irrigation water supply can be recycled to rainfall'.

Comment R1.11:

Line 67-70: It is not true. See some new works including specific crop types, cultivation schedules, as well as multiple irrigation techniques.

Leng, G.Y., Leung, L.R., Huang, M.Y., 2017. Significant impacts of irrigation water sources and methods on modeling irrigation effects in the <scp>ACME</scp> <scp>L</scp> and Model. J. Adv. Model. Earth Syst. 9, 1665–1683. https://doi.org/10.1002/2016MS000885

Yin, Z., Wang, X.H., Ottlé, C., Zhou, F., Guimberteau, M., Polcher, J., Peng, S.S., Piao, S.L., Li, L., Bo, Y., Chen, X.L., Zhou, X.D., Kim, H., Ciais, P., 2020. Improvement of the Irrigation Scheme in the ORCHIDEE Land Surface Model and Impacts of Irrigation on Regional Water Budgets Over China. J. Adv. Model. Earth Syst. 12, 1–20. https://doi.org/10.1029/2019MS001770.

Response R1.11:

We replaced

'Efforts are made to achieve this goal in the Community Land Model (CLM) and Noah-MP LSMs (Felfelani et al. 2020, Zhang et al. 2020, respectively).'

by

'For example, efforts are made to achieve this goal in the Community Land Model (CLM), in Noah-MP, in Accelerated Climate Modeling for Energy (ACME), and in ORganizing Carbon and Hydrology in Dynamic EcosystEms (ORCHIDEE) LSMs (Felfelani et al. 2020, Zhang et al. 2020, Leng et al. 2017, and Yin et al. 2020, respectively).'.

Comment R1.12:

Line 400-402: This is a literature at 2001. I guess both seed selection and fertilization contributes to the LAI increase. Figure 5b shows that the observed LAI in 2002 coincides with Boedhram et al. (2001). Is there an increasing trend of LAI?

Response R1.12:

The 1999-2018 LAI observations used to produce this Figure tend to increase through time. The mean annual LAI presents a trend of 1.57 10^{-2} $m^2m^{-2}yr^{-1}$. The ISBA_pheno_irr simulation presents a less pronounced trend of 1.23 10^{-2} $m^2m^{-2}yr^{-1}$. The simulation accounts for the atmospheric CO_2 effect on plant growth and water use efficiency using the method descibed in Calvet et al. (2008) but this effect mainly concerns C3 plants. Agricultural practices such as seed selection, pest control and fertilization are not represented. This could explain the less pronounced trend in the simulation. Boedhram observations were made in 1994 and 1995 but the observed LAI trend is not large enough to question the use of these data.

Comment R1.13:

Minor comments:
1. Line 36-37: It is not necessary to mention a specific region. The previous sentence already well describe the key drivers of increasing water demand.
2. Line 40: 'controlling' -> 'mitigating'.
3. Line 100: 'the' -> 'a'.
4. Line 104: Modify it to 'by driven by atmospheric forcing ..'
5. Line 149: Modify it to 'tends to optimize water withdrawal according to water extracting abilities
of crops at different stages.'
6. Line 151: Modify it to 'sum of irrigated water will be validated by the ...'
7. Line 161-162: This sentence is abrupt. Consider to remove it.
8. Line 163-164: Please avoid hand-waving if there is little information about flood and drip irrigation in the manuscript.
9. Line 165: Seems Figure 1 doesn't appear in the previous content. Consider to change the order
between Figure 1 and Figure 2.
10: Line 169: 'that' -> 'whether'.

11: Line 173-174: Remove this sentence.
12: Line 188: 'with no' -> 'without'.
13: Line 1001: blue dots?
14: Line 1029: For the correlation coefficient, it will be good enough if the p-value < 0.05.

Response R1.13:

Many thanks. All your suggested changes were made. For p-value, see response R2.5.

Reviewer #2

Comment R2.1:

The authors have done a commendable job in addressing most of the questions in the revised submission and the overall quality of the presentation has improved substantially. I have a few additional comments here and hope this helps.

Response R2.1:

Many thanks for these positive comments and for your in-depth review of the manuscript. We did our best to further revise the manuscript.

Comment R2.2:

Comment to response R2.5: a bit more detail is needed to argue the robustness of using a model resolution of 0.25 deg when other input datasets are available at a much finer resolution such as the irrigation map. I understand that the resolution of the ERA5 met-forcing might be a factor at play, but I think it would help by adding discussions on how the choice of this spatial resolution may affect resolving the smaller irrigation variabilities and whether this may affect the results under different conditions? It might be good to clarify this.

Response R2.2:

We agree that the spatial resolution context needs to be clarified, although intercomparing several versions of the model at various spatial resolutions is out of the scope of this work.

In the Introduction,
'The objective of this work is to develop and evaluate a more detailed representation of irrigation practices into the ISBA LSM within the SURFEX (SURFace EXternalisée) modelling platform (Masson et al., 2013).'
was replaced by
'The objective of this work is to develop and evaluate a more detailed representation of crop phenology and irrigation practices into the ISBA LSM within the SURFEX (SURFace EXternalisée) modelling platform (Masson et al., 2013). The new scheme is designed to work on a global scale. We focus on a densely irrigated area in Nebraska where validation data are available.'

At the end of Section 4.1,
'A large-scale experiment involving ground and airborne measurements was recently performed in northeastern Spain to assess the impact of irrigation on atmospheric model simulations (Boone et al. 2021).'
was replaced by
'A large-scale experiment involving ground and airborne measurements was recently performed in northeastern Spain to assess the impact of irrigation on atmospheric model simulations (Boone et al. 2021). In this context, high resolution atmospheric data from the Application of Research at the Operational Mesoscale (AROME) numerical weather forecast model are available to drive the ISBA model. AROME is run operationally at 1.3 km over a large part of western Europe. Future developments will focus on the inter-comparison of ISBA simulations at various

spatial resolutions and under different conditions, as the choice of spatial resolution may affect the simulation of the smaller irrigation variabilities.'.

In the Conclusion,
'A new uncalibrated irrigation scheme is implemented within the ISBA land surface model in order to improve the representation of vegetation over agricultural areas.'
was replaced by
'A new uncalibrated crop phenology and irrigation scheme able to work on a global scale is implemented within the ISBA land surface model in order to improve the representation of vegetation over agricultural areas.'

Comment R2.3:

Comment to response R2.13: Regarding the discussion on the wet year overestimation of irrigation, could you explore and elaborate on what might be the main factor contributing to this overestimation? Could that be resulted by precipitation uncertainty or the relatively coarse spatial scale? Or could that be irrigation sensitivity to selected irrigation parameters? I would appreciate a short discussion of this aspect so that readers can have a better understanding of the ISBA irrigation performance.

Response R2.3:

Many thanks for this remark. We completed Supplement S4 with new Figures investigating ERA5 precipitation uncertainties over the Grand Island station from 1985 to 2018. ERA5 tends to markedly overestimate precipitation in April, by 0.57 mm d^{-1}, i.e. 27 % on average (Fig. S4.3). This is rather systematic (8 years out of 10, see Fig. S4.4). In July, ERA5 precipitation can be much smaller than the observations, for example in 1991 and 2007 (Fig. S4.5). In situ observations indicate that year 2010 is wetter than 2005 and 2000 during the growing season: 575, 508 and 277 mm from May to September, respectively. Year 2000, 2005, and 2010 are compared in Fig. S4.6. In 2010, the ERA5 precipitation bias in July and August triggers a cumulated precipitation gap of 150 mm. The model responds to this water deficit by triggering irrigation, especially in August (Fig. S4.6c).

Addition to Section 4.1:

'The comparison with in situ precipitation observations at the Grand Island station is Supplement S4 shows that ERA5 tends to markedly overestimate precipitation in April, by 0.57 mm d-1, i.e. 27 % on average (Fig. S4.3). This is rather systematic (8 years out of 10, see Fig. S4.4). In July, ERA5 precipitation can be much smaller than the observations, for example in 1991 and 2007 (Fig. S4.5).'

Addition to Section 3.1:

'In situ precipitation observations over Grand Island indicate that year 2010 is wetter than 2005 and 2000 during the growing season: 575, 508 and 277 mm from May to September, respectively. Year 2000, 2005, and 2010 are compared in Fig. S4.6. In 2010, the ERA5 precipitation bias in July and August triggers a cumulated precipitation gap of 150 mm. The model responds to this water deficit by triggering irrigation, especially in August (Fig. S4.6c).'

Comment R2.4:

Comment to response R2.15. Thanks for conducting additional analysis on ET. Figure 5&6 indicates that LAI seasonality differs largely between ISBA_ref and ISBA_pheno_irr, however, when looking at the seasonal cycle for ET, the peak timing seems to be not much affected. I wonder how the ISBA model estimate ET? Is LAI or vegetation related parameter at play? I think it would be good to elaborate the ET discussion in terms of its response to LAI from both the modeling and observational aspect.

Response R2.4:

Observational aspect: in response to Reviewer 1, we removed the ET comparison with GLEAM from the manuscript, since GLEAM has shortcomings over irrigated areas and cannot be considered as an observation. However, we kept Supplement 3 and a version of the 4.2 discussion section focussing on modeling aspects.

Modeling aspects: the following sentences were added at the beginning of Section 4.2:
'All evaporation terms (plant transpiration, soil evaporation, interception) are simulated by ISBA. Under given environmental conditions, the simulated plant transpiration is not proportional to LAI. A simple canopy radiative transfer model is used to simulate the available photosynthetically active radiation (PAR) within the vegetation canopy. The response of GPP and transpiration to PAR and to LAI is controlled by this radiative transfer model. Photosynthesis and transpiration are calculated for three layers and summed to calculate canopy level values. For large LAI values, the mean leaf-level GPP and transpiration simulations are reduced in relation to smaller vegetation transmittance to solar radiation. The impact of changes in LAI on mean leaf-level GPP and transpiration is large at intermediate LAI values ranging from 1 to 3 m^2m^{-2}. It is much reduced for LAI values larger than 3 m^2m^{-2}. An improved version of this radiative transfer model able to represent ten canopy layers, with a more realistic response to solar zenith angle will be available in the next version of SURFEX (Delire et al. 2020).'

Reference:

Delire, C., Séférian R., Decharme B., Alkama R., Calvet J.-C., Carrer D., Gibelin A.-L., Joetzjer E., Morel X., Rocher M., Tzanos D.: The global land carbon cycle simulated with ISBA-CTRIP: improvements over the last decade, Journal of Advances in Modeling Earth Systems, 12, e2019MS001886, https://doi.org/10.1029/2019MS001886, 2020.

Comment R2.5:

Comment to response R2.16: It sounds that the R and RMSD values are arbitrarily selected to be presented in white and marked with white plus. I would suggest testing the significance of the R and RMSD differences and use white to mask out the insignificant ones. And it is not necessary to add white plus in the figure. You may use fisher's z test for R difference and paired sample t test for RMSD difference.

Response R2.5:

Many thanks for noting this.
We completed Table 3 with the number of observations over the considered time period. In R Figures, we now use white to mask Fischer's test R p-values > 0.01. In R difference Figures, we now use white to mask Fischer's z test for R difference p-values > 0.05. In RMSD difference Figures, we now use white to mask paired sample Student's test for RMSD difference p-values >

0.05. Note that for LST, correlation and RMSD differences were not significant and we decided to remove former Fig. 12 together with the paragraph citing Fig. 12.
In order to improve readability, we reorganized former Figs. 6 and 8. LAI and GPP time series are now in the same Figure (new Fig. 5). LAI and GPP score differences are now in the same Figure (new Fig. 6).
The following sentence was added at the end of Section 2.3:
'The significance of r, r differences, and RSMD differences is tested using Fisher's test, Fisher's z test, and paired sample Student's test, respectively. Significance levels of 0.01, 0.05, and 0.05 are used, respectively.'