

Response to Reviewer 2

General comments

The authors effectively addressed my main concerns regarding the improvements achieved with the new model version. In this regard, I find Figure 2 quite compelling. I believe that, at the current state, the manuscript is satisfactory from a scientific content standpoint and has the minimum requirements for publication on GMD.

Nonetheless, there are still many parts that require clarifications, and the study needs many improvements in terms of style, coherence, and notation. In particular, Section 2.1 needs to be greatly improved in clarity, and the authors are required to carefully review all equations.

[Response] We sincerely thank the reviewer for the careful second-round review and for the many constructive and insightful comments. They have helped us substantially improve both the clarity and the robustness of the manuscript. We have carefully revised the manuscript to address all major concerns. The main changes are summarized below, with detailed, point-by-point responses provided afterwards (the line numbers refer to the tracked text).

- (1) We clarified the interpretation of the drying climate scenario and revised Sect. 3.4 to ensure consistency between the experimental setup and the analysis (see reply to comment(s) #1).
- (2) We improved the interpretation of residue management effects on hydrological processes, and more clearly stated the limitations of the model in Sect. 4.5 and Conclusion (see reply to comment(s) #2).
- (3) We corrected the description of the spatial setup in Sect. 2.1.2 and 2.1.3 by removing the misleading 0.5° resolution and clarifying that all simulations are conducted at site level with interpolation of gridded meteorology data to site locations (see reply to comment(s) #3).
- (4) We clarified the parameterization of surface roughness under residue cover, emphasizing that the formulation remains unchanged while parameter values are adjusted (see reply to comment(s) #4).

Please find below a non-exhaustive list of required corrections. The authors are also required to extensively review the overall manuscript by carefully choosing wording and improved logical flow.

[Response] Thanks for the helpful suggestions. Following the reviewer's suggestions, we are convinced that the revised manuscript has improved further.

Furthermore, I still have some major concerns:

1. A methodological concern remains regarding the “drying climate scenario”, despite the authors’ reply to my previous review. The authors mention that one meteorological year is employed to force the model, but I still fail to see how the 10-year simulations in the dry scenario differ across years. From section 3.4, it seems that results from the dry simulation are the same from one year to the next. If there is barely any difference between the years, then the pairwise comparison between “dry” and “non-dry” years is wrong: in this case, the authors should thoroughly review that section and their overall approach regarding the “drying climate scenario”.

[Response] In the drying climate scenario, the model is forced by meteorological data from a single dry year, which is repeated over the 10-year simulation period. Therefore, we agree that there is no interannual variability in climate forcing, and model results are not expected to differ substantially from one year to the next.

The purpose of our constructed scenario is to represent a persistent dry condition and to examine the cumulative effects of crop residues over a 10-year period, rather than to analyse interannual variability. The averaging over the simulation period is used to provide a robust estimate of this sustained response. We note that results from the final simulation year are consistent with the multi-year average, indicating that the system response is stable over time.

Following the reviewer's suggestion, we have revised Sect. 3.4 to clarify this point and remove wording related to interannual comparisons or trend analysis.

Line 498-547: "The 10-year simulation **under** current climate shows a minor short-lived increase in SWC at the soil depth of 0.25 m (relative root depth of winter wheat in ORCHIDEE-CROP model) due to the presence of crop residues. Averaged over the simulation length and across six sites, total SWC is 0.24 ± 0.60 kg m⁻² ($2.81 \pm 6.15\%$) higher than in the simulation without crop residues (Fig. 5a). **No clear** carry-over effect of SWC savings **from** one growing season **to the next is observed** at any site. This is because increased drainage losses compensated for reduced bare soil evaporation (Fig. S9). Soil temperature (T_{soil}) at 0.25 m soil depth penetrable by winter wheat roots decreased by an average of -0.37 ± 0.77 °C ($7.56 \pm 5.96\%$) over the 60 days of residue covering periods across sites, **with no consistent temporal evolution** during the 60-day residue covering periods over 10 years (Fig. 5b). The largest reduction in T_{soil} occurs **shortly** after harvest, coinciding with **the** largest increase in α_{surf} due to residue presence compared to simulation without residue effects.

In the drying climate scenario, the meteorological forcing corresponds to a single dry year that is repeatedly applied over the 10-year simulation. Therefore, this experiment represents persistent dry conditions, and temporal changes reflect the sustained system response rather than interannual variability.

Under these conditions, the presence of crop residue shows a similar increase in SWC at 0.25 m soil depth under current climate, with an average increase of 0.23 ± 0.53 kg m⁻² ($3.01 \pm 5.97\%$) compared to simulations without residues (Fig. 5a). Results from the final simulation year are consistent with the multi-year average, indicating a stable response over time.

Although no systematic temporal evolution is observed in near-surface SWC (at the soil depth of 0.25 m), deeper soil layers (50% soil depth) exhibit a gradual accumulation of soil moisture over the simulation period (not shown). This phenomenon likely reflects the sustained effect of reduced E_{soil} and enhanced soil water retention at depth. In contrast, superficial SWC responds more rapidly to short-term fluctuations in soil water balance, limiting the emergence of longer-term signals.

E_{soil} suppression by crop residues enhances soil water retention, underscoring their critical role in mitigating drought stress under persistent dry conditions. As diminished water input (e.g., rainfall) (Fig. 5d) restricts both runoff and infiltration, water loss is governed primarily by E_{soil} . Consequently, the decrease in E_{soil} by crop residues allows more water to remain in the soil, leaving more residual moisture available for the next growing season, potentially enhancing crop development by alleviating water stress.

Average T_{soil} at 0.25 m soil depth decreases with a statistically significant trend by an average of -0.42 ± 0.80 °C ($-8.42 \pm 3.58\%$) over years (Fig. 5b), consistent with the sustained effects of residue-induced surface cooling."

2. It seems to me that the authors claim that residual managing has uncertain implications in terms of climate change mitigation/adaptation (which is reasonable), but it could be useful as an agricultural practice. However, unless I've missed something, the authors evaluate differences between ORC-D and

ORC-AE, and conclude that in terms of SWC there are no significant differences. I assume that ORC-D here would represent a configuration in which there is no residual managing (bare soil), while ORC-AE maintains the residuals. Based on just statistical significance, it seems that residual managing might be a marginally relevant agricultural practice. Regardless of how the authors intend to address this point, the conclusions should better reflect and convey the hypothetical nature and uncertainties of their evaluation.

[Response] We agree that the effect of residue management on SWC may appear small based on our results of statistically non-significant differences in SWC between ORC-D and ORC-AE. However, this result does not imply that residue management is of marginal importance in reality.

In our model, ORC-D represents a configuration without residue cover after harvest, whereas ORC-AE includes retained residues (Table 1). The present study primarily focuses on the biophysical effects of residues through albedo changes. In contrast, residue-related hydrological processes are represented in a simplified manner, mainly through modifications of soil conductance and soil evaporation resistance (Sect. 2.3). In addition, the current model configuration does not explicitly account for the effects of tillage practices on soil hydraulic properties, which are often closely linked to residue management. Future model developments could further improve the representation of residue management impacts by incorporating processes related to soil organic matter accumulation and its influence on soil physical properties, such as aeration and water holding capacity. As a result, the model is more suitable for assessing radiative impacts than for capturing the full range of soil-water conservation mechanisms associated with residue management.

Therefore, the limited and statistically insignificant differences in simulated SWC should not be interpreted as evidence that residue management is ineffective, but rather as a reflection of the limited role of albedo and surface roughness in that matter. Following the reviewer's suggestion, we have revised the manuscript to more clearly emphasize these limitations and to avoid misleading interpretation of the results.

Line 720-726: "Accordingly, the limited and statistically insignificant differences in simulated SWC between ORC-D and ORC-AE should be interpreted with caution. In the current model configuration, crop residues primarily affect the land surface through surface albedo, roughness effects, and a simplified treatment of soil evaporation resistance. Processes that are closely linked to residue management, such as the effects of tillage on soil hydraulic properties and the longer-term influence of soil organic matter on soil physical characteristics, are not yet explicitly represented. Therefore, the present simulations are particularly well suited for assessing the biophysical energy-balance impacts of residue cover, while the representation of soil water responses remains an area for further model development."

In addition, we revised the conclusion to better emphasize the most robust impact of albedo changes, whereas hydrological and management implications remain more uncertain and require future model development:

Line 767-769: "However, conclusions regarding soil water benefits or optimal management practices remain dependent on future model developments that further improve the representation of soil hydrological responses to residue management, including interactions with soil organic matter dynamics and tillage practices."

3. Where does the 0.5° spatial resolution come from (e.g. L139)? The ORCHIDEE-CROP model is stated to have a 0.5° spatial resolution (L152-153), but it's unclear how this is relevant, since the model is forced by local measurement sites data. It seems to me that what is being doing is just running the model at sites

location: if that's the case, shouldn't resampling gridded data at 0.5° be replaced just by interpolation at the measurement site location?

[Response] We apologise for the confusion caused by the reference to a 0.5° spatial resolution. In this study, the model is run at site scale, not as a gridded simulation. Therefore, the 0.5° resolution is not relevant here and has been removed from the manuscript.

We have revised Sect. 2.1.2 and 2.1.3 accordingly to address this issue:

Line 146-148: “At two ClieNFarm sites with no flux observations, the meteorological variables from the hourly 0.25° ERA-interim product were **extracted** and interpolated to **exact site coordinates to provide meteorological forcing data**”.

Line 161-163: “Since the model is run at site scale, the bare soil albedo was extracted at each site position, and linear interpolation was applied to convert the data to a daily time step, aligned with the model temporal resolution.”

4. L301: So H_{rough} can be expressed as $H_{rough} = f(crop, max) \times 0.5 \times (1 - H_{dis})$, where 0.5 basically replaces the wheat canopy height in the original formula. So, the formula would be the same as the one above and the only thing that changed is the canopy height considered? The same stands for Z₀.

[Response] Yes, the overall structure of the formulation remains the same. The main change is that the canopy height used in the calculation is reduced from the winter wheat height to a representative residue height (0.5 m), and the crop fraction is replaced by the residue fraction during the residue covering period.

Therefore, the formulation itself is unchanged, but the physical interpretation and parameter values differ to account for the presence of crop residues after harvest. The same reasoning applies to Z₀, where the parameterization follows the same structure but uses residue-related parameters.

We have revised Sect. 2.3.4 to clarify this point.

Line 323-324: “These two formulations remain identical to Eqs. (7) and (8), but with adjusted parameter values for canopy height and fractional cover to represent crop residues.”

Minor comments:

L117-118: Please add an introductory sentence, explaining that two different sources were used (ICOS and ClieNFarm)

[Response] We have added an introductory sentence at the beginning of Sect. 2.1.1 to clearly state that two different data sources (ICOS and ClieNFarm) were used.

Line 121-124: “To derive surface albedo (α_{surf} , unitless) from site observation, we combined datasets from two sources: (1) eddy-covariance measurements from the Integrated Carbon Observation System (ICOS) Data Portal (Dumont et al., 2023; Schmidt et al., 2023; Buysse et al., 2023; Brut et al., 2023; Brümmer et al., 2023; Tallec et al., 2023; Bernhofer et al., 2023), and (2) satellite-derived products at farms belonging to the ClieNFarm project. ”

L13 of supplementary: title would be “Quality filtering and preprocessing of surface albedo data at ICOS sites”? To differentiate them from the ClieNFarm sites

[Response] We replaced the previous title in SI with “Quality filtering and preprocessing of surface albedo data at ICOS sites” to differentiate them from the ClieNFarm sites”

Line 13: “Text 1.1 **Quality filtering and preprocessing of surface albedo data at ICOS sites**”

L128-129: should this sentence be “Across the selected 10 sites, we chose only the years in which winter wheat was cultivated, and obtained a total of 33 winter wheat years”?

[Response] We revised this sentence in Sect. 2.1.1:

Line 136-137: “Across the selected 10 sites, we chose only the years in which winter wheat was cultivated, and obtained a total of 33 winter wheat years”

L142: I suggest removing “including precipitation, air temperature, wind speed, humidity, and radiation” – stating “All meteorological variables” would already be enough

[Response] We removed the repeated words according to this suggestion.

Line 151-152: “All meteorological variables from this year were then used to force the simulation.”

L151: If “ $S2 \alpha_{surf}$ ” is referring to the Sentinel-2, I suggest using “ $S2-\alpha_{surf}$ ” for clarity

[Response] We replaced $S2 \alpha_{surf}$ with $S2-\alpha_{surf}$ in Sect.2.1.3.

L158: “LAI was used primarily to constrain”, isn’t the term ‘primarily’ redundant? Based on what the authors said it was used just to constrain the onset, correct?

[Response] We have revised the description in Sect. 2.1.3.

Line 168-169: “MODIS-derived LAI was used to constrain the onset of the maturity period, rather than to reproduce absolute LAI values.”

L166: why winter wheat and maize instead of just winter wheat?

[Response] The reference to both winter wheat and maize relates to the calibration of the original ORCHIDEE-CROP model version used as a starting point in this study. In the present work, we only simulate winter wheat. We have clarified this point in the revised manuscript to avoid confusion.

Line 176-179: “This study employs a **previously developed model** version calibrated **against LAI**, flowering and harvesting dates, and crop yield for **both** winter wheat and maize (<https://github.com/yangsssu/ORCHIDEE-CROP.git>). **In the present study, only winter wheat is considered.**”

L178: “ $SW_{IN\downarrow}$ ” is used in the formula, but shouldn’t it just be either “ $SW_{\downarrow}$ ” or “ SW_{IN} ”?

[Response] We deleted “ $\downarrow$ ” in equation 3.

L196-200: is this formula correct? At first glance the units do not add up. What are the units for the beta terms?

[Response] We apologize for the lack of clarity in the original manuscript. The time step was not explicitly included in the equation. The bulk formulation on the right-hand side yields evaporation in $\text{kg m}^{-2} \text{s}^{-1}$, which is integrated over the model time step (dt) to give kg m^{-2} per time step.

In the model, this quantity is then expressed in mm day^{-1} using the equivalence $1 \text{ mm} = 1 \text{ kg m}^{-2}$.

In addition, the terms β_1 , β_4 and β_5 are dimensionless scaling factors representing soil moisture and resistance effects, and therefore do not affect the dimensional consistency. We have revised the equation and clarified the unit definitions in the manuscript.

Line 210-216: “ $E_{soil} = (1 - \beta_1) \times (1 - \beta_5) \times \beta_4 \times r_{au} \times v \times C_d \times (q_{surf} - q_{air}) \times dt$ (5)

Where β_1 , β_4 and β_5 are dimensionless scaling factors representing soil moisture and surface resistance effects for snow sublimation (β_1), soil evaporation (β_4) and floodplain evaporation (β_5), respectively. r_{au} (kg m^{-3}), v (m s^{-1}), C_d (-), q_{surf} (kg kg^{-1}), and q_{air} (kg kg^{-1}) denote the air density, wind speed, drag coefficient, saturated surface air moisture, and specific humidity, respectively. **The bulk formulation yields evaporation in $\text{kg m}^{-2} \text{s}^{-1}$, which is integrated over the model time step (dt) to obtain kg m^{-2} . This quantity is then expressed in mm day^{-1} using the equivalence $1 \text{ mm} = 1 \text{ kg m}^{-2}$.**

L210: Please improve formatting of the equation (use bigger parentheses). The same stands for the other equations

[Response] We have revised the formatting of the Eq (6) in line 225, Eq (7) in line 234, Eq (14) in line 290, Eq (15) in line 297 and Eq (17) in line 320 by using appropriately sized parentheses to improve readability.

L215: I believe that a more standard notation for canopy height could be h_c (or H_c if the authors want to be more consistent with the rest of the notation)

[Response] We have replaced ‘Height’ with H_c in Sect. 2.2.4.

L222: The meaning of “weighted height of vegetation with the maximum fcrop for winter wheat in each grid” is unclear

[Response] We have revised the description in Sect. 2.2.4 for clarification.

Line 237: “...and the vegetation **height weighted** by the maximum f_{crop} ($f_{crop,max}$) **of** winter wheat **within** each grid”

L225: H_{ave} appears on both left- and right-hand side of the equation (which I assume it's an error)

[Response] We agree that the original formulation was incorrect. The equation was intended to represent an area-weighted average of vegetation heights within each grid cell. In general, this equation is:

$$H_{ave} = \sum_i f_i H_i$$

where f_i is the fractional coverage and H_i is the corresponding crop height.

However, in this study we only consider winter wheat within each grid. Therefore, the formulation simplifies to:

$$H_{ave} = f_{crop,max} H_c$$

We have corrected the equation in line 241 and clarified this point in the manuscript.

L274: meaning of k_3 and k_4 should go after eq. (14)

[Response] We explained k_3 and k_4 after Eq. (14).

Line 291-292: “Where α_{max} is the simulated α_{crop} on the last day of the albedo increase phase. $(1 - f_{res}(t))$ represent the daily variation of bare soil fraction. **k_3 and k_4 represent the fitting parameters that need to be calibrated at the site scale (see below).**”

L328: “Data from the remaining five sites with five winter wheat years” – why five sites? Isn’t it just the remaining five years?

[Response] We deleted “five sites with” in Sect. 2.4.2 to be specific.

Line 350: “Data from the remaining five winter wheat years (n=179 daily α_{surf} observation) were utilized for testing.”

L332: “Simulations were performed for the 10 sites”

[Response] We have revised this sentence to be specific.

Line 354: “Simulations were performed for 10 sites **with 14 farms** (Table S2)”

L345: Fix Table 1 caption

[Response] We have revised the caption of Table 1 to improve clarity and make it more informative.

Line 367: “Table 1 **Overview of simulation experiments and model configurations.**”

L371: “of the old and new models and observation”. Do the authors mean “the comparative performances of the old and new models against observations”?

[Response] Yes, we intended to refer to the comparative performance of the old and new models against observations. We have revised the wording accordingly for clarity.

Line 396-398: “With the calibration of the simulation of harvesting date for all sites, we further compared the **simulated** surface temperature (T_{surf}), LE , H from the old and new models **against** observation during the foliar yellowing and residue covering periods.”

L372: “to assess the statistical significance of the results” is unclear. Please state clearly the null hypothesis that is being tested.

[Response] We have revised the text to explicitly state the null hypothesis being tested. In the paired t-test, the null hypothesis is that the mean paired difference between the simulations from the two model configurations is equal to zero. Statistical significance is then assessed based on this hypothesis.

Line 398-399: “**For each variable, a paired t-test was also used to test the null hypothesis that the mean paired difference between the simulations from the old and new models is equal to zero.**”

L435: “ORC-AE indicates” instead of “ORC-AE suggests”

[Response] We have revised the wording accordingly in the legend of Figure 4 by replacing “ORC-AE suggests” with “ORC-AE indicates”.

L505-506: Why would both positive and negative perturbations of k_1 lead to enhancements of surface cooling?

[Response] In our model, k_1 controls the linear increase of residue albedo during the first 15 days, and also determines the albedo value at the transition to the decay phase (Sect. 2.3.2). This value influences the subsequent exponential decay of albedo. At the same time, the residue fraction gradually decreases over the 90-day period. As a result, changes in k_1 affect the full temporal evolution of the product of residue

albedo and residue fraction, which determines the cumulative reflected radiation. This leads to a nonlinear response in surface cooling, where both increases and decreases in k_1 can enhance cooling, although the effect is stronger for positive perturbations.

We have clarified this point in the revised manuscript.

Line 552-555: “This reflects a nonlinear response, as k_1 controls not only the rate of albedo increases but also the initial value of the subsequent decay phase (Sect. 2.3.2). Combined with the gradual decrease in residue fraction over time, changes in k_1 modify the cumulative contribution of residue albedo to surface radiation, leading to enhanced cooling in both cases.”

L527: I wouldn't say that the control is “even stronger” – to me it looks like the responses are usually below the 2°C cooling

[Response] We have removed “even stronger” from the revised manuscript.

L531: missing space between “increased” and “Z0”

[Response] We have corrected this error.

L548: comma instead of period before “while”

[Response] We have revised this sentence in Sect. 4.2.

Line 596-598: “Our simulations revealed that the surface-atmosphere interactions (ORC-E) dominate a decreased LE in both periods (Fig. 4e and Fig. S6b), with this effect jointly mediated by changes in β_4 and Z_0 (Fig. S11d), **while** α_{surf} -driven radiative impact is less important.”

L568: “both and their”?

[Response] We have deleted “their” to correct this error.

L621: “while removing the residue cover, temporarily boosting E_{soil} and LE after the end of residue covering” seems an anacoluthon. Please check wording.

[Response] We have revised the wording in Sect.4.3 to improve clarity and readability.

Line 669-671: “Early tillage, as observed at the FR-Gri site in 2018 (three weeks after harvest), **retains** more SWC in the topsoil **and removes** the residue cover, **thereby** temporarily **enhancing** E_{soil} and LE after the end of residue covering (Fig. S12).”

L627-628: Check wording.

[Response] We have revised the wording in Sect.4.5 to improve clarity and readability.

Line 676-677: “Despite improvements **to** the ORCHIDEE-CROP model **in simulating** crop albedo changes **associated with foliar** yellowing and **crop** residues, **as well as** their biophysical climate impacts, uncertainties and limitations **remain**.”