Review of "The impact of coupled 3D radiative transfer on surface radiation and cumulus clouds over land" by Tijhuis et al.

24 June 2024

General comments

This paper presents large eddy simulations of continental cumulus clouds with a coupled shortwave 3D radiation scheme. Results are compared to simulations with a standard 1D radiation scheme to assess the impact of shortwave 3D radiation effects on the clouds and surface radiation. Simulations with 3D radiation are found to have larger and deeper clouds, which reflect more shortwave radiation and therefore reduce the surface shortwave radiation. This acts in the opposite direction to non-coupled 3D radiation effects that tend to enhance the surface shortwave radiation. Overall, there is an almost exact compensation such that the mean surface shortwave radiation in simulations with 1D and 3D radiation is very similar. This finding is based on multiple simulated days.

The paper is clear and well written. I learned a lot from reading this paper. I would like to congratulate the authors on their significant new findings. The conclusions are mostly convincing, and not necessarily expected. I think this paper will spur further studies and will become a well cited reference in the coming years. I have a few suggestions to further strengthen the study, as outlined in my comments below. After addressing these comments, I believe the study will be appropriate for publication in ACP.

Specific comments

Title: I suggest changing to "The impact of coupled shortwave 3D radiative transfer on surface radiation and cumulus clouds over land" because the authors did not consider coupled longwave. This is already mentioned in the manuscript, but it would add clarity to put this extra word in the title.

Introduction: Several references are made to "cloud resolving models". I think the authors are only referring to large eddy simulations (LES). Often, the term "cloud resolving model" is used to refer to a model that is run without a convection scheme. These models can still have horizontal resolution of a few km, but I don't think that's what the authors intend. I recommend saying LES from the beginning, or even better explicitly state the range of horizontal resolutions that are used in the cited studies.

L47-49: It is interesting that the study from Jakub and Mayer claimed that cloud organization (cloud streets) occur with coupled 3D radiation, but the studies by Veerman et al. and the present study do not seem to identify or even mention cloud organization. I think it is worth mentioning this difference in the introduction and/or conclusions as a potential discrepancy that exists in the literature.

L75-76: It would be helpful to expand on what is meant by "the days where the simulated cloud cover visually matches the observed cloud cover". What is the criteria for a match? For example, does the match consider only cloud cover or also cloud size, shape, organization, etc?

Section 2.1: I recommend listing the exact dates that were used for the clear-sky (13 days), cloudy (20 days), and subset cloudy (12 days). Or put them in a table. This is needed for reproducibility reasons.

L82: I do not see any supplementary materials uploaded. Can the details of the LES model be referred to previous literature?

L88: A couple of sentences explaining what the land surface model is actually doing would help here. How realistic is the assumption of an instantaneous surface response? I am concerned that this assumption might make the coupling with the clouds too strong. If, in reality, there is some delay of the surface response, then the clouds could evolve or be advected before they "feel" the surface immediately below them. I would have thought that the land surface model needs to account for the processes that determine the response time of the surface such as heat transfer between vegetation and soil layers, and stomatal opening/closing.

L90: Can some justification be provided that the radiation calls once every minute are sufficient? The appendix shows that the wind speed often exceeds 5 m/s at 10 m altitude. The wind speed at cloud altitude is probably even larger. Taking 5 m/s as a typical wind speed, in the 1 minute between radiation calls a cloud would be advected 300 m. Figure 4d shows that 300 m is comparable to the length scale of the simulations. This means that an individual cloud will move a distance that is comparable to the size of its shadow before the position of the shadow is updated in the simulation, which seems quite "jumpy". Given that the study depends critically upon the land-atmosphere coupling imposed by the evolving pattern of cloud shadows, I think the reader needs some more convincing on this decision.

L108-112: This hypothesis would benefit from some discussion of the time/length scales of the boundary layer mixing. The surface is not instantaneously connected to the cloud immediately above it. It takes time for perturbations in surface fluxes to be transported up to cloud base. And during this transport there must also be some mixing that occurs, such that the variability at cloud base is not as sharp as the surface. For the clouds to be influenced, I think it has to be the case that: 1. the timescale for mixing to cloud base is shorter than the timescales of individual cloud evolution and movement, and 2. the surface discontinuities are not simply mixed away during transport to the cloud layer. For example, one recent study that considered how these types of clouds change during a solar eclipse suggested that the fastest timescale for surface air parcels to be transported to cloud altitude is around 15 minutes (https://doi.org/10.1038/s43247-024-01213-0). It is not clear that this is shorter than the timescales of cloud movement and growth/decay, which leads me to question this hypothesis.

L158: Give -> Given

L161-162: At large RH, above 90% or so, even a small change in RH can result in a large change in optical properties due to the non-linearity in aerosol extinction as a function of RH. Are the optical properties defined per 10% RH even at high RH? If so, this likely introduces an important source of error for aerosol optical properties in the vicinity of clouds. These errors will not be evident in the clear sky cases that are used for validation, but they will be present on cloudy days because RH will approach 100% toward cloud edges.

L172: It should be noted that the definition of cloud cover in observations and models is slightly different here. A scanning or wide-field view instrument will detect and include cloud sides as part of the overall cloud cover. In contrast, cloud cover in the LES model is defined only from a zenith view perspective. This will generally lead to an overestimate of cloud cover in observations relative to LES, unless an instrument simulator is used within the LES to ensure sampling consistency (which I don't think is done here). This fact also has implications for one of the main results of the study, that the clouds are deeper in 3D coupled radiation but the cloud cover is the same. This may be true with the model definition of cloud cover. But from an observational perspective, the cloud cover could actually still increase because the deepening of clouds results in more of the sky becoming obscured at oblique views of the instrument. This is worth commenting on in the conclusions.

L211-223: Can a statistical significance test be done to determine whether the correlations presented are significant? That would make the presented correlations more convincing.

L211-231: Similar to my comment above about the hypothesis for cloud changes, I think this discussion would benefit from considering the time and space scales involved. If the hypothesis holds, the correlation should be highest for a combination of the factors explored: when the wind direction is aligned with the sun angle AND the shadow displacement divided by the wind speed is similar to the cloud base height divided by the updraft speed. Could the authors look at this explicitly and see if they find a connection? I also wonder if it is possible that this combination of factors could lead to a suppression of cloud development in 3D in the case that clouds are systematically moving toward their shadows in 3D. Do the authors see any evidence of this? If not, does this provide evidence to reject the proposed hypothesis?

Fig. 6: Are the box plots showing the mean across the entire day or at a specific time during the day? I might have missed it but I don't see this mentioned anywhere.

Section 4.2.1 and Fig. 5b: The global uncoupled effect is always positive, meaning that the diffuse effect dominates throughout the day. The Gristey et al paper that is already cited showed that the global effect can be negative at the end of the day on some days, because the direct effect can dominate at oblique sun angles. I am curious, do the authors see this on any of their simulated days, or is the global effect positive for all times of day and all cases? This would be an interesting similarity or difference to note in the paper.

Schematic figure of key result: I think this paper would really benefit from a schematic figure that captures the main result in the abstract ie. the almost exact compensation between uncoupled and cloud 3D effects. I encourage the authors to consider creating a schematic that represents this result in an intuitive and concise way. Figure 1 achieves this for the methodology. I am thinking of something similar to Figure 1, but for the results. This type of figure can help to engage a broader audience and increase the impact of the study.