Review of the manuscript "The impact of coupled 3D radiative transfer on surface radiation and cumulus clouds over land" by Tijhuis et al., 2024

In this manuscript, the authors study the effects of 3D atmospheric radiation transfer (RT) on the surface's energy budget and cloud's properties for cumulus cloud fields over land. They analyze a unique dataset of a dozen Large eddy simulations (LES) with online calculations of 3D RT in the solar spectrum. This is unique since all LES simulations use 1D RT to decrease computational load. Here the issue is solved by using GPU for enhanced calculations.

The novel dataset allows studying the effects of 3D radiation transfer on clouds' dynamics and general properties like cloud cover, thickness and water density.

I think that the paper is suitable for publication in ACP, the analysis is complete and convincing, and the paper is clearly written. I believe that after a few clarifications the paper will be ready for submission, hence, I suggest a minor revision.

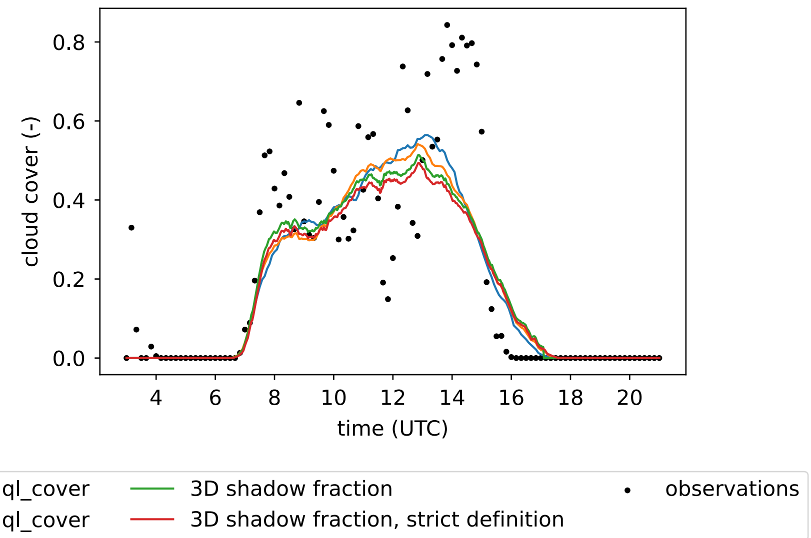
We want to thank the reviewer for the kind words about our manuscript and for taking the time to review our manuscript. We address the suggested clarifications below (in green text).

Major comments:

1. L.88: Can the authors explain why they chose a skin heat capacity of zero for the interactive surface and how realistic it is? To a none expert in the matter, it sounds like this could cause quick and unrealistic warming of the surface that can highly influence shallow convection.
   Little is known about what the most realistic skin heat capacity is for a grassland and what its impact on shallow convection is. Van Heerwaarden (2011) investigated the sensitivity of simulations of a convective boundary layer to the skin heat capacity and found that the heat capacity of the skin layer (which represents the vegetation) has minimal influence, because – even with a non-zero heat capacity -- the surface temperature responds very fast to the radiation, and the fastest fluctuations are anyhow mixed away by the turbulence. An instantaneous responding surface is used in previous LES work, e.g. by Lohou and Patton (Journal of the atmospheric science, 2014) and Gehrke et al. (GMD, 2021). Furthermore, previous work with MicroHH with the same set-up for the land surface model that we use shows that shallow convection is realistically modelled (van Stratum et al., JAMES, 2023). Hence, we chose to use the zero skin heat capacity. Please see also our answer at comment 7 of reviewer 3, where we discuss the land-surface model.
2. L.161-166: This part of the paper is unclear. Please explain if aerosols affect the dynamics of the simulations. In short, how are the microphysical processes handled? Are aerosol radiative effects and horizontal variability coupled to in the simulations?
   We thank the reviewer for pointing out that this part is unclear. In line 152 we mention briefly that we only include the direct effect of aerosols, by which we mean that the aerosols do not influence the microphysics. We will write this

more explicitly. The radiative effects and horizontal variability that comes from the horizontal variability in humidity is included in the coupled radiation calculations (and also in the uncoupled radiation calculations). We will explicitly mention this in line 166.

3. L.180-187: Please explain how cloud cover is defined. This is a tricky definition that can make comparisons between different datasets complicated (especially models to observations). Since the 1D and 3D don't show much difference, I suggest showing the sensitivity of 3D to different definitions or choices of thresholds.
   We will add in these lines an explanation about how the cloud cover is defined and we will make it clear that we only make a rough comparison between the observations and the simulations, as these comparisons are tricky and we only aim to show that our simulations represent realistic conditions, we don't claim or aim to have an exact match. In addition, we shortly investigated the impact of the used definition in one of our 3D simulations as suggested by the reviewer and we will briefly mention the result hereof in the paper.
   The figure below shows the cloud cover following the model definition (labelled ql_cover) for the example day from figure 5. One alternative option to define cloud cover in the simulations with 3D radiation is to look at the clouds under an angle. We can determine the cloud cover along the angle of the sun from the surface area that is shadowed with a shadow being an area with the global radiation less than 120 W m^{-2}. With this definition the cloud cover is higher when the solar zenith angle is large, and the cloud cover is lower when the solar zenith angle is small, but the differences are limited to +/- 0.04. With a stricter definition (global radiation less than 60 W m^{-2} the cloud cover reduces, but the difference between the two thresholds is always smaller than 0.025. Therefore, for our rough comparison between the observations and our model a different definition leads to the same conclusion.



4. L.320: It is reasonable to assume that changes in cloud properties like cloud cover and optical thickness are most important for global surface radiation and scene albedo. The current version of Fig. 3 suggests that other processes

influence cloud cover more than the radiative transfer scheme (RT). This raises the question of how important 3D effects are for surface or total energy budget. Can the authors elaborate on this in the discussion and maybe even compare the bias caused by using 1D RT with other known biases and uncertainties in cloud or atmospheric modeling (like the choice of advection scheme, model resolution, microphysical scheme, etc.,)?
We agree with the reviewer that figure 3 shows that the cloud cover is not strongly influenced by the 3D radiation and that other known biases and uncertainties might have a larger influence on the simulated cloud cover. We will mention that the cloud cover might be more sensitive to other model choices such as the choice of advection scheme, model resolution, microphysical scheme when we discuss figure 3 (last paragraph of section 3). However, it is beyond the scope of our paper to investigate how sensitive the clouds are for these choices and subsequently how sensitive the energy budget is to these choices.

Minor comments:

1. L.147: I suggest using different names for the decomposed effects. Uncoupled is quite confusing and at the start can also be interpreted by the reader as 3D-1Drad3D. I would suggest something like Radiative-only. The cloud effect could be referred to as the 3D-coupling effect or dynamic effects.
We understand the confusion about uncoupled as we use it both for uncoupled radiation computations and for the uncoupled effect. Therefore, we will rename the uncoupled effect to radiation effect. We feel like the name cloud effect is not causing confusion and can therefore be kept. In addition, we argue that it is important to have short and easy descriptions of these effects to keep the text readable, hence radiation-effect and cloud-effect work well.
2. L.198: Can the authors please explain why they chose to use the characteristic length scale and what is its physical meaning? Why wasn't a simpler measure of cloud size like mean size used?
We will elaborate in the text on the meaning of the characteristic length scale. The advantage of the characteristic length scale is that it gives an indication of the size not just for the clouds, but for structures in general. Therefore, the larger characteristic length scales for vertical velocity in the boundary layer and specific humidity above the boundary layer show that the turbulent structures become larger, which inevitably means results in larger clouds. The disadvantage of the cloud size is that more assumptions are needed to determine it. One needs a tracking algorithm to identify all the clouds for which different options exist (see Heus and Seifert, GMD, 2013 and references therein). After tracking the clouds size needs be defined, which also can be done in different ways (see Mol et al., JGR Atmopheres, 2023).
3. L.209: I wonder what are the effects of these findings on the scene albedo (top of the atmosphere upwelling fluxes). If cloud cover is the same but the clouds are thicker, does it mean a larger cloud radiative effect?
We agree with the reviewer that this is an interesting question and a nice addition to the paper. Therefore, we will add the top of domain upwelling fluxes in Figure 6

and add a short discussion of these results to the text. The similar cloud cover but thicker clouds indeed result in more radiation going upwards at the top of the domain (cloud effect). However, this is compensated by the radiation effect causing the net difference in upward radiation at the top of our domain to be close to zero.

4. L.224: Please explain what is the displacement distance. Does it change with the radiative transfer scheme? If LWP is higher then clouds might live longer and be more advected.
   The displacement distance is the horizontal distance between a cloud and its shadow, derived from the domain-averaged cloud base height and the solar zenith angle (as described in section 2.2.2.). As we only used the term displacement distance a few times, we will write it out where we use it. This is only a relevant quantity for the simulations with 3D radiation, as with 1D radiation the shadow is always directly underneath the clouds, hence it is not horizontally displaced. The displacement distance is not related to how far the cloud moves during its lifetime.
5. L.241: Can the authors explain how the spread is quantified? Since the presentation is of only 3 cases, statistical measures are ambiguous, could be better to simply plot all three time series.
   Since we have 3 repetitions of the simulations with coupled 1D and 3D radiation, we can make 9 combinations for the coupled effect and cloud effect. For the radiation effect there is indeed only three. We will explain this better around line 241 and we will adapt the caption of the figure.
6. L.149-255: It took me a minute to understand the discussion about the splitting methods. Might be clearer to mention the two methods by referring to Fig.1 or showing it mathematically (e.g., 3D-1Drad3D vs. 3Drad1D-1D).
   We will add the mathematical description here and refer to figure 1.
7. L.322-326: I think that the authors can show the role of 3D radiative transfer on Earth's energy budget with not a lot of extra effort. What are the changes in atmospheric heating and top of the atmosphere fluxes? Does decreased diffused radiation on the surface means increased heating rates in the atmospheric or higher scene albedo at the top-of-the-atmosphere? Might be worth to have even a short discussion on this as well.
   We thank the reviewer for this nice suggestion. We will add the top of domain upwelling fluxes in Figure 6 and add a short discussion of these results to the text. To link the results at the surface to what happens at the top of the domain, one should not just consider the diffuse radiation, but also the direct radiation that is scattered by the surface and goes back up (as diffuse radiation), hence the top of domain radiation is most closely related to the global radiation.
8. L.336: Is there a reason to assume that the findings of this paper will be different away from the mid-latitudes? Dror et al., (IEEE, 2020) showed that a dominant subset of such clouds doesn't have a strong latitudinal dependence.
   We thank the reviewer for bringing this paper to our attention. Our main reason to expect a different effect at different latitude is (as mentioned) that other latitudes have other solar zenith angles, which will impact the 3D effects. However, we agree with the reviewer that the results of Dror et al form a reason to

expect similar results in other regions, hence we will mention both possibilities in the paper.

Technical comments:

1. L.17: In cloud and weather modeling communities Cloud resolving models are usually referred to course resolution models on a scale of 1 km.
   For these models it also holds that the cloud and its shadow will not be located in the same grid box, hence 3D effects are relevant. We will rephrase this such that it refers to both cloud resolving models and large eddy simulations.
2. L.90: Worth mentioning that RRTM is 1D, and explain, even in short, the ray tracing concept and the novelty of the GPU usage (in Veerman et al., 2022 line 90).
   We will mention that RRTMGP is 1D and we will explain briefly the novelty of the ray tracer of Veerman et al., 2022.
3. L.160: This is not very clear, does aerosol vertical profile change with time in simulations?
   Yes, it does, we will formulate this clearer.
4. 5: adding y-axis labels as in Fig.6 would make the figure clearer.
   We left out the labels in figure 5 on purpose. Figure 5b shows 3D-3Drad1D (as in figure 6), but also 1Drad3D-1D. Similarly, figure 5c shows 3Drad1D-1D as in figure 6, but also 3D-1Drad3D. Putting all of this in the y-axis labels makes it to our opinion only less clear.
5. 6 captions: Which dataset is presented, worth mentioning it's for all 12 days.
   We will mention this as suggested.

Additional references

- van Heerwaarden, C. C. (2011). Surface evaporation and water vapor transport in the convective boundary layer. Wageningen University and Research. (https://edepot.wur.nl/169077)
- Gehrke, K. F., Sühring, M., & Maronga, B. (2021). Modeling of land–surface interactions in the PALM model system 6.0: land surface model description, first evaluation, and sensitivity to model parameters. Geoscientific Model Development, 14(8), 5307-5329.
- Heus, T., & Seifert, A. (2013). Automated tracking of shallow cumulus clouds in large domain, long duration large eddy simulations. Geoscientific Model Development, 6(4), 1261-1273.
- Mol, W. B., van Stratum, B. J., Knap, W. H., & van Heerwaarden, C. C. (2023). Reconciling observations of solar irradiance variability with cloud size distributions. Journal of Geophysical Research: Atmospheres, 128(5), e2022JD037894.