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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of model resolution on entrainment (inversion heights), cloud-radiation interactions, and cloud radiative forcing" *by* H. Guo et al.

Anonymous Referee #1

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This paper presents results of numerical simulation of a stratocumulus-topped boundary layer, focusing on the impact of model spatial resolution on simulated cloudiness. I think the analysis presented in the paper is superficial. The paper seems to be put together quickly, without in-depth analysis of model results. The proposed mechanism is rather obvious, but I do not think the analysis quantifies in sufficient detail contrasting impacts of entrainment and radiative effects. An important and missing aspect is the comparison with observations. This paper cannot be published in anything resembling its current form.

Major issues.

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1. How the simulations compare to the observations? This aspect deserves at least a small section in the paper. Are the low- or high-resolution simulations closer to the observations?

2. I am not surprised that radiative tendency is sensitive to the model vertical resolution. Since the tendency comes from the divergence of the radiative flux, higher vertical resolution implies larger local tendencies provided that the fluxes away from cloud base and cloud top do not change much. Thus, the only fair comparison would be if the high resolution radiative cooling is averaged into the low resolution grid. Since I am not a radiative expert, I wonder if this problem is well-posed from the radiative transfer point of view. In other words, can one increase vertical resolution further and converge at some point? Or will the local radiative tendency increase without a limit? Of course, for some vertical gridlength, assumptions within the radiative transfer model will break down (individual photons), and so will the bulk assumptions of the cloud microphysics (individual cloud droplets). Should this aspect be at the least recognized as a potentially serious problem for the model convergence? Should this issue be investigated in a 1D model of radiative transfer with a single stratocumulus cloud layer? I think this is a critical aspect that needs to be addressed in the paper.

3. I consider the constant value of the effective radius across the cloud unrealistic. Since the cloud water content increases with height and the droplet concentration is approximately constant, the mean volume radius of cloud droplets should increase with height as well. Since the effective radius is typically proportional to the mean volume radius, assuming constant value of 10 microns does not make sense to me. I am not sure what impact this unrealistic assumption has on model results but I think at least some simulations should be repeated to address this question. A related issue: what is assumed about the single scattering albedo of cloud droplets? Obviously its value may have significant impact on model results, as absorbing aerosols (either externally or internally mixed) have a significant impact on the cloud absorption during the day.

4. The impact of spatial (vertical in particular) resolution on LES simulations of stra-

tocumulus clouds is an aspect that the community was considered for quite some time (especially within GCSS). Initially the focus was indeed on the entrainment rate, but more recently it was broadened up. I do not think the paper gives sufficient credit to those studies. I also do not understand the authors’ criticism of some of the approaches taken in the past. For instance, one does not need to incorporate the radiation transfer model to simulate the effects of radiative cooling. For instance, the approach used in Stevens et al (MWR 2005) will have the same effect of increasing local radiative tendencies. Note that Stevens et al. did observe increase of the mean LWP with increasing model resolution, so did several other studies that need to be referred to.

5. The discussion concerning the impact of such studies on the role of clouds (boundary layer clouds in particular) on climate and climate change needs to be toned down. For instance, there are several aspects that the paper does not mention and which have been shown to play some role, such as the 3D radiative transfer, homogeneity of cloud-environment mixing, absorbing aerosols, strength of temperature and moisture inversion, etc. It follows that the results discussed in the paper need be viewed as tentative as far as the impact on climate is concerned.

6. There are some aspects of the simulations that should be pointed out in the discussion. A) The authors do not show convergence of their solutions (this is related to my point 2 above). B) Are the simulations long enough? Clearly, model fields show drift and the question is what would happen if they are allowed to run for several days. C) Is a single sounding (to which model is nudged) representative of both daytime and nighttime conditions? D) Are the nudging terms similar in all simulations? If not, do they have any role in LWP changes?

Specific comments.

1. I do not like the title. I think the parentheses should not be allowed.

2. P. 20402, upper paragraph, reference to Moeng et al. 1996. I think there are more

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recent studies that should be referred to.

3. P. 20404, middle. I do not understand the sentence: “This abrupt increase originated in the upward…”.

4. Same page, next paragraph (and in one or two other paces): “A suite of …. was…”, not “were”.

5. P. 20405. Please explain how nudging was included to the model equations. In particular, one can either use local values of temperature and moisture or use the horizontally-averaged values. The first method dumps the small-scale perturbations and should NOT be used. The second one is more appropriate as it results only in the shift of the mean value, without dumping the perturbations. Please explain which method was used in the study.

6. Since the atmosphere above the inversion is very warm and dry, are these conditions especially sensitive to small changes of the model resolution (i.e., through the cloud-top entrainment instability)? I think the authors should make an attempt to put their results in the perspective of other studies. For instance, why the DYCOMS case was not selected? The authors would then be able to see how their model measures against the community and the observations, and thus give more credibility to their results.

7. Several comments on Fig 2. First, the mean inversion height changes by at most 20 meters in various simulations, whereas the vertical gridlenth is 10 m or more. How important are such changes? The model shown interesting contrast between the day and night in the temporal variability of the standard deviation, especially when the highest vertical resolution is used (sudden jumps at night and smooth signal during the day). Are these different behaviors understood? The same comment applies to Fig. 3.

8. Section 3.1.3 has to be rewritten after more reasonable analysis of radiative cooling is performed. In my view directly comparing radiative tendencies does not make sense. These need to be transformed into the same grid (corresponding to the lowest vertical

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resolution) and then compared.

9. Fig. 3, related to one of my major points. Differences in the LWP at the end of the simulations are often about a half of what was 24 hours earlier. The simulations should be run for a few diurnal cycles.

10. P. 2049, table 2. I assume there is also horizontal averaging involved, isn’t it?

11. Same page, bottom paragraph. The argument here is just a speculation. One should look at the budgets to see the BL drying.

12. Section 3.2, fig. 5. I am not sure if the bottom panel is needed. Just the mean value in text would be sufficient I think.

13. P. 20411, the very end of the section 3.2. I am not sure why the GCMs estimates are brought here. This is confusing as these values involve global estimates and are 1-2 orders of magnitude smaller.

14. Section 4. I would like to see the results presented in this paper put in the context of observed cloud properties and of previous studies.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 20399, 2008.