Atmos. Chem. Phys. Discuss., 7, S4566–S4575, 2007
www.atmos-chem-phys-discuss.net/7/S4566/2007/

Atmospheric
Chemistry
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Discussions

Interactive comment on “A study of the effect of overshooting deep convection on the water content of the TTL and lower stratosphere from Cloud Resolving Model simulations” *by* D. P. Grosvenor et al.

D. P. Grosvenor et al.

Received and published: 31 August 2007

Responses to referee 1.

First, the method to calculate the radar reflectivities has to be described.

This has been included in an appendix.

Second, the comparison with radar is hopeless as the radar detects an organized convective system with several plumes while the model simulates a single warm bubble.

Unfortunately, it was not possible to simulate such organised storm complexes

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in 3D because of computational power restrictions. Whilst it is true that interactions between cells within organized systems can affect the dynamics of individual cells it seems likely that, since a variety of strengths of cells were simulated, the simulated single cells are representative of some of the cells that form and decay inside the observed complexes.

Specific comments p7278, l5. Write HIBISCUS (instead of HIBISUCS!)

This has been changed.

p7278, l7. Longitude of Bauru should be given.

The longitude has been added.

p7278, l16. The sentence "Moistening is produced in all cases, convective vigour is not a factor in whether moistening or dehydration is predicted" is misleading. Without any doubt a shallow convection will have no moistening effect on the water vapour in the lower stratosphere.

This has been changed to: “Moistening is produced in all cases, indicating that convective vigour is not a factor in whether moistening or dehydration is produced by clouds that penetrate the tropopause, since the weakest case only just did so.”

p7278, l16. The wavelength of the S-band of the radar would be given precisely (around 10 cm, I presume).

The radar frequency is 2.8 GHz and this has been noted in the paper.

To what the 10-dBz echo top corresponds? Does it mean that a significant content of graupel backscatters the radar signal? Which one? Do other hydrometeors contribute to reflectivities?

Using the Marshall Palmer reflectivity-rainfall relationship (Marshall Palmer, 1948) 10 dBZ = 0.15 mm/hr rainfall rate. At 10 dBZ or less it is usually non-

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precipitating droplets, at most drizzle that are being detected and so graupel is unlikely to be contributing. In the upper troposphere it is likely to be the other ice hydrometeors (ice and snow) that are being detected.

p7283, section on the CRM. It is stated that the model allows supersaturation. So how does it work? A few sentences on the conversions parameterized in the ice scheme would help.

Some information on the scheme has been added.

p7285, l21. Again, what is the significance of the 20-dBZ threshold in terms of hydrometeor contents?

20 dBZ corresponds to 0.6 mm/hr rainfall rate. There is no significance in terms of hydrometeors but this value was found to be the most suitable threshold for TITAN to identify the cell centroid for tracking the multi-cell complex during its life-time.

p7286, l7. An indication of CAPE for each warm bubble used to initiate convection should be given to quantify the vigour of convection. What about the wind profile used for the simulations?

CAPE values over the heating area have been added to Table 1. The wind profile is indicated by the wind barbs in the sounding (Fig. 1).

p7287, l29. The longer time for which the 40 dBZ contour was reached should be given.

This information has been added.

p7289, l8. It is written that "the model may be predicting too many particles of high mass", but it should be also due to an overestimation of the density of the particles.

The usage of a triple moment microphysics scheme whereby the density of graupel is predicted and carried by an additional model field made little difference to

the result, so this seems unlikely.

How are calculated the reflectivities? What kind of hydrometeors do you take into account? What about the particle distributions? And what are the dielectric properties?

This information has been added in an appendix.

p7292, l5. As reflectivities are due to precipitating hydrometeors, it should written that the "cases show qualitatively similar precipitating hydrometeor contents" rather than clouds (that are made of non-precipitating hydrometeors).

Hydrometeors observed near cloud base will certainly be precipitating, but any hydrometeors above cloud base could also be in an updraught, being carried upwards (especially near the echo core), possibly forming hailstones or being taken towards the anvil and blown out of the storm, usually as small ice crystals. They are likely to remain there or evaporate without precipitating. Some of these non precipitating particles will be detected by the radar and so it is likely that not all of the return signal is due to precipitating hydrometeors.

p7292, l25. It is difficult to be convinced of the "reasonable agreement" between observed and simulated reflectivities as the axes and the colour scales in Figs. 5 and 7 are different.

The colour scale on Fig. 5 has been changed to match the scale on the simulated reflectivity plots.

p7293, l16. I was not able to read the minimum values in Fig. 8. Please give the values.

They are 0.57 ppmv for 3-D and 0.62 ppmv for 2-D. These have been included in the text.

p7293, l22. "Considerable amounts of ice". What are the amounts? It would be of interest to show the cloud envelope.

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The cloud envelope has been indicated by including contours of the total ice mixing ratio on top of Fig. 8. Does this suggest some failures in the representation of ice by the microphysics scheme? This is unlikely to suggest any failure of the microphysics scheme since it would be expected for large amounts of ice to be transported upwards from below where water vapour is much more abundant.

p7297, l28. The cross sections in Fig. 13 should be zoomed in the region of interest.

This figure has been changed.

p7298, l29. "inaccuracies in the model microphysics". The discrepancies with the radar reflectivities can be also due to drawback in the dynamics (with not mixing enough) or in the calculation of the reflectivities (see comment for p7289, l8).

The possibility of dynamics being the cause is not mentioned in this paragraph because it discusses the 10 dBZ echo top heights being lower in the model compared to many of the observed clouds. It would seem likely that a lack of entrainment in the boundary layer would make the clouds more vigorous therefore making this problem worse. A sentence on inaccuracies in the model dynamics possibly making the radar reflectivities too high has been added later at the end of the sentence on p. 7299 L11.

p7299, 1st par. Snow also contributes to reflectivities.

In these simulations graupel was found to be the main contributor to the simulated reflectivity.

p7300, l5. typo.

This has been corrected.

p7312, l26. What are "more natural techniques"?

The following is added to the end of the pargraph ending p.7312, L27: “One example would be to use a mesoscale model to simulate the synoptic environment

in order to look at storm development through convergence and moisture transport from tropical regions, as occurs via the SACZ."

Responses to referee 2.

1. As indicated above, the paper is rather long. May be the authors can trim some of the very detailed discussions on the 2-D vs. 3-D results? While it is nice to have such detailed comparison between these two sets of results, I have already been convinced half way into the reading that 3-D is better than 2-D and am wondering whether or not some of such comparisons can be done in form of a table instead of detailed descriptions.

It is difficult to remove any of the 2D/3D discussion without affecting references to it in other parts of the paper. This would therefore require a large re-structuring of the paper if it were not to detract from the narrative and so it is felt that the paper would be better left in its longer state.

2. The fairly large disparity between the horizontal and vertical resolution is a concern (2000m vs. 75–125m). Wouldn't this cause a problem in the advection results in the CRM? Some discussions of this and its possible impact on the results are probably necessary (although I am not requiring a full sensitivity study).

This is covered in the response to point number 4 from referee 3.

3. CRMs using hot bubble technique normally requires a spin-up time which depends on the model scheme. The results in early time steps (say before 15 or 20 min) are often disregarded as these do not necessarily reflect the stable characteristics of the cloud being simulated. It is desirable that the authors discuss the spin-up properties of the model used here.

To address this point the following is added in the conclusions: "The spinning up of the model wind and moisture fields to produce turbulent motions on a variety of scales and to allow mixing of moisture within the boundary layer may be an

important process as described in (Carpenter et al., 1998) since it might affect the entrainment of environmental air into the developing warm bubble. However, a finer horizontal resolution is required before this can be properly simulated and hence errors due to the latter are likely to overshadow any arising because of issues involving spin up time." (Also see point number 3 by referee 3)

4. P. 7297, 2nd paragraph: "The increases in total water were mainly due to increase in water vapour"-presumably this comes from the ice parameterization scheme used. If the scheme produces fairly large ice crystals, then of course most of them fall out. On the other hand, it is also possible that the ice crystals produced at such low temperatures are very small and they can be injected into the TTL and LS directly. Perhaps a few sentences about the ice size distribution are in order here.

The possibility of the ice size distribution produced by the model affecting the differences in the balance in vapour and ice between the 2-D and 3-D cases is mentioned here. A forward reference to section 3.4 is provided where the importance of the ice size distribution in general is discussed along with the potential effects due to the model microphysics. A forward reference to the conclusions section is also provided where the possibility of a lack of production of small ice crystals through homogeneous aerosol freezing affecting the ice precipitation and moistening/dehydration balance is discussed.

5. Most of the comparisons with observation were based on radar reflectivity features. The S-band radar echo mainly reflects distributions of large hydrometeors (e.g., raindrops, graupel, large snow flakes) but not smaller ice particles. Are there other types of data (e.g., satellite) that can be used for the comparison purpose?

The referee is correct in saying that there are other types of data that would allow the examination of the smaller ice crystals. One example is a comparison between the Meteosat Second Generation (MSG) satellite measurements of brightness temperatures (BTs) and model simulated BTs as in Chaboureau et

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al. (2007). The following sentence mentioning this possibility for future work is included in the paper at the end of the paragraph ending p.7312, L27: "Use of satellite data, such as in Chaboureau et al. (2007) where observed brightness temperatures (BTs) were compared to those simulated, would facilitate comparisons of the smaller ice crystals. This would allow further probing of the discrepancies between the model and reality and might shed some light on the reasons for the differences in radar reflectivities as well as discerning whether these differences affect the processes at cloud top."

6. Caption of Fig. 6 (P. 7730) should indicate that this is the 3-D simulation result.

This has been changed.

7. Some figures have such small fonts in the axes that are very difficult to read (e.g., fig. 7, 13, 15, 18, 21). Larger fonts should be used.

The fonts on these figures have been enlarged.

Responses to referee 3.

1. Abstract, line 25: When the tropopause height is given, it should be stated, which definition is used, e.g. cold-point tropopause or WMO-tropopause (see also next comment) 2. p.7284, line 10: There is no height scale on Fig. 1, so it is not clear which feature is at 15.9 km. It would be helpful to state the corresponding pressure. Is this the cold point to within model resolution? A forward reference to the discussion on p.7289 might also help.

The statement has been changed to define 15.9 km as both the WMO and cold point tropopause, which is also the model cold point to within model resolution. The pressure of the tropopause has also been included and a forward reference provided to the discussion on the TTL position.

3. p.7286: A problem with the bubble initialisation that is not mentioned in the paper is the potential lack of turbulence early in the cell's lifetime, leading to a systematic

underestimate of entrainment in the lower troposphere (see Carpenter et al., JAS, 55, 3417-3432). In contrast to the coarse horizontal resolution, which changes the balance between resolved and parameterised mixing with unknown results, this effect will give a systematic bias towards excessive vertical velocities. This should be noted in the paper.

The paragraph on this page p. 7286 line 4 has been changed to discuss this limitation due to model resolution and the Carpenter paper is referenced.

4. p.7286: The balance between horizontal and vertical resolution (2000m vs. 75m) is probably not optimal. The downward heat flux due to numerical dissipation of gravity waves noted by Kuang and Bretherton will not have time to affect the temperature greatly in these short simulations. On the other hand, the coarse horizontal resolution implies that there will be strong horizontal diffusion across the highly tilted isentropes in the lower stratosphere, which will be regarded as a net vertical mixing by the end of the simulation when the isentropes have returned to horizontal. More sensitivity studies would be required to sort this out, which I would regard as beyond the scope of this paper, but it will be difficult to progress beyond the current state of the art until the realism of the entrainment processes in this class of simulation is established.

The large ratio between horizontal and vertical grid size and its potential effect on vertical mixing is mentioned.

5. p.7294: It would be interesting to see a time-height plot of total water, along with or instead of Fig. 10. Much of the discussion centres around when the moisture precipitates out of the stratosphere, but that cannot be seen from the figures provided.

A time-height plot of the total water has been included along with Fig. 10.

6. p.7300: Much of the discussion in section 3.3 seems to miss the point, since it addresses moisture transport into the TTL. Since this air is likely to experience dehydration if it passes through the cold point in the Brewer-Dobson circulation, the magnitude

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of the transport in lower levels is not particularly important. This is in contrast to the direct transport of moisture into the stratosphere, which is of great significance since it is decoupled from the cold-point temperature. This is mentioned later, in the conclusions, but this does not seem sufficient - I would disagree with the final sentence of the section (p. 7302 lines 27ff)

A sentence on the possible cold point dehydration of this lower altitude air has been added. It is felt by the authors that moisture below the cold point may still lead to stratospheric moistening since the cold point can be sub-saturated (as in Bauru), especially at higher latitudes. A sentence to make this point had also been added.

7. Figure 20 is not referenced in the text

A reference to this figure is added in the discussion on the observations of high level particles (section 3.5)

References

Carpenter, R. L., Jr., Droegemeier, K. K., Blyth, A. M.: Entrainment and detrainment in numerically simulated cumulus congestus clouds. Part I: General results., Journal of the Atmospheric Sciences, 55, 3417-3432, 1998.

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