Review of "The Impact of Cloud Microphysics and Ice Nucleation on Southern Ocean Clouds Assessed with Single Column Modeling and Instrument Simulators"

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Recommendations: Major Revision

1 General Comments

This paper describes an experiment comparing two microphysics schemes of differing complexity by testing in a constrained single-column model framework. The results were compared against field observations over an extended period of 75 days. The observations were made in the Southern Ocean where mixed phase cloud processes are important feature of the atmospheric system, and one which is currently poorly represented in numerical weather prediction models. As such this paper makes a useful contribution to the field by evaluating how both a single-moment (1M) and double-moment (2M) microphysics scheme performs in this region. The work takes a broad overview across the full time range, giving an insight into the problem, whilst also looking in detail at the diurnal cycle. Detailed study of various process rates in the 1M and 2M schemes are trialled and compared.

The observations data come from a 75 day period in Austral spring, on a remote Island in the Southern Ocean. Instrument simulators are employed to convert the model predictions into a form that can be directly compared to observations, e.g. hydrometeors are passed through a radar simulator code.

The major conclusions are that both the 1M and 2M schemes have some skill in representing the observed cloud and rainfall properties, both at the surface, top of atmosphere, in in the vertical cloud column, especially for bulk properties such as mass concentrations and liquid water paths (LWPs), including supercooled liquid water (SLW). The 2M scheme is shown to add value for radar reflectivities - hence clouds structures, but at the expense of somewhat overestimated LWP.

The results of a suite of tests are then presented, including time step sensitivity, process rate comparisons between schemes, and numerical formulations of some aspects of the microphysics - namely sedimentation, along with sensitivity tests of number concentrations of cloud condensation nuclei (CCN) and ice nucleating particles (INP).

The paper could be improved by adding clarity and perhaps a re-formulation of the aims and conclusions of the paper. It is not clear who the intended audience is, either Southern Ocean specialist, cloud model developers or modelling centres (numerical weather prediction (NWP) or climate) wanting guidance when choosing between 1M and 2M cloud microphysics. It might be all three but, some rewriting to tighten up the aims and conclusions is suggested. For example - Line 71 suggests

there will be guidance on where 2M schemes may be necessary - but this isn't directly concluded upon later. it might be the case (as is in fact stated by the authors) that full 3D simulations are required to go beyond the results in this work, something which would clearly require a step-change in compute resource. As well as highlighting that changing various parameters and assumption(e.g. number concentrations of aerosols) can alter the results, the work would benefit from a deeper commentary looking at the relative importance of the various aspects tested. otherwise this paper reads like a description of a test framework, as a forerunner to full 3D model simulations.

2 Specific Comments

2.1 Title

Mention of the comparison of 1M and 2M microphysics schemes would be more informative of the content of the paper. Also mentioning that the key difference is enhanced SLW in the MG3 2M scheme would point to one of the key results.

2.2 Abstract

it is stated that both dynamic and aerosol through microphysics are important, but the model tests only use single-column, and so it isn't clear what the "dynamic" refers to here - is it just the tests where the relaxation to prescribed temperature was turned off?

2.3 results

As a general comment - can the number of figures be reduced? For example, as mentioned elsewhere - can Fig. 6 be combined with 11 and 12 as anomalies?

Are all process rate figures needed (Figs. 8, 9, 10, 12)- they are also difficult to read. perhaps there could be a cloud liquid ice 2x2 and a recipitation rain and snow 2x2 panel?

- Lines 186-198 are repetition with the figure caption, read like methodology, and could possibly be tidied up or consolidated.
- Lines 212-216 - Stated that there is too much high cloud in IFS, resulting in poor TOA LW performance. this is followed up on in Sect. 3.1.3. it is mentioned that there are Line 277 states that IFS produces more of the low reflectivity values at altitude than MG3. But IFS also has a shoulder of higher reflectivity values (Fig. 5 middle right) compared to MG3 - could these values not be responsible for the poor TOA LW performance?
- Lines 229 onwards - There is discussion around the limitation of CERES SYN at night - should this data even be shown (possibly yes, to highlight the issues)?
- The use of MODIS at high latitude is interesting, and shows some evidence of a diurnal cycle - soon after sunrise - is this also an artefact, or believable? There is not much discussion on this particular aspect.

- Line 334 - A paragraph here to introduce the sensitivity tests in Sect. 3.2 would aid clarity of the following sections.
- Line 405 - the timesteps tests for IFS seem unnecessary.
- Figures 7, 8, 9, 10 - can resultant budgets be included here? For example Line 358 onwards, discussion of Bergeron process indicates that some processes not fully compensated for between IFS and MC3 - this could be shown visually too.
- Section 3.2.2 - authors comment that there aren't huge differences in outcomes, perhaps only that MG3 has more SLW. Does this section add a huge amount to the overall paper?
- There are significant differences in model results when scaling either CCN or INP by factors of 4, as might be expected, but it's not clear if these values a physical / climatologically reasonable or just for illustration. Do these test point to the need for use of 2M in NWP and climate simulations or not? Varying parameters individually will give different results, but the true test is covarying parameters, in a 3D model - which whilst commented on in the summary, does beg the question as to what these tests are trying to illustrate.

2.4 Summary and Conclusions

This section would benefit from editing, in particular to link more closely to the abstract and aims. One aspect seems to be the importance of sub-grid representation of precipitation and cloud fraction, which is talked about in the results text.

The paper shows that both IFS and MG3 can represent portions of the cloud fields, that surface radiation is OK, and that where data are good the LWP predictions are good, although perhaps MG3 is too large, even though this doesn't feedback on to SW biases at the surface.

3 Technical Corrections

- Line 205 - "TOA flux" should be "TOA SW Flux"
- Units should be represented as negative powers where appropriate and have a space between elements
- Line 360 - incorrect abbreviation of 'accretion' (also elsewhere, plus others not defined)

3.1 Figure comments

3.1.1 General

There are numerous formatting styles in use for figures in this manuscript and general inconsistencies that could be rectified.

- Many multiple panel plots need sub-panel labels, (a), (b), (c), etc, and the text updating accordingly
- Suggest that panel titles are removed and moved into the captions

- suggest moving variables names and units from panel titles onto y-axes
- units should be written as negative powers as appropriate, and spaces between elements (also in the body text)
- units should be enclosed in square brackets consistently
- Figures 6, 11, 12 only have y-axis titles on the top row
- Figure 2 needs the dashed-line describing in the caption

3.2 specific

- Figure 1 (and other time series) - could the time units be in days?
- Figure 1 - can the xtick but in 6 hourly (or 4?) intervals for 24 hours?
- Could Figure 6 be combined with Figs. 11 and 12 in to a 5x3 panel plot, and the figures 11 and 12 presented as anomalies, as the results are very difficult to visualise
- Figures 8 through 13 - many of the abbreviations of process rates are not defined, or incorrectly defined (typographical errors). Could a lookup table be provided perhaps?