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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The incorporation of the Tripleclouds concept into the δ-Eddington two-stream radiation scheme: solver characterization and its application to shallow cumulus clouds" *by* Nina Črnivec and Bernhard Mayer

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This paper presents the implementation of the "tripleclouds" treatment of cloud inhomogeneity into the libRadtran radiative transfer package. This includes the development of a new "maximum-random" overlap technique to represent the core-shell model of clouds. A new solver is also developed for the treatment of two-stream fluxes in three regions within the column (although it is unclear from the description whether this

method is novel or essentially a reimplementation of a previously documented method). The new tripleclouds formulation is tested with application to a shallow cumulus cloud field from an LES simulation, by comparison with a full 3D Monte Carlo scheme, an independent column approximation method, and a homogeneous cloud fraction method. This comparison is particularly interesting for having a full 3D model as the control and seeing how the error introduced by the treatment of inhomogeneity compares with the error from neglecting 3D effects.

The paper is generally very clearly written with some useful schematics. I would recommend it for publication subject to some minor revisions commented on below.

Specific comments:

1) Introduction: I would suggest that one disadvantage of the tripleclouds method, compared to the other cloud heterogeneity methods described, is the computational cost of the tripleclouds solver. Lines 72-74 mention that the value of the tripleclouds scheme would be increased if fewer spectral intervals were used. Perhaps the main point to mention here is that in order to limit MCICA noise when there are a small number of spectral intervals, oversampling of each interval would be required, which would increase the cost of MCICA to a similar level as the tripleclouds solver.

2) Lines 77-78: the initial implementation of the "tripleclouds" scheme from Shonk and Hogan 2008 was in the Edwards-Slingo (now "Socrates") model that is also a delta-Eddington two-stream scheme. I would suggest the novel focus of this paper is the implementation and adaptation of the method in the libRadtran package in particular.

3) Section 2.3.2 conventional GCM representation: did you have an optical depth threshold to determine the cloudy part of the domain? Might the results improve if you did? The determination of cloud fraction in a GCM is quite model dependent I imagine and possibly tuned to give the best emergent cloud properties. It probably doesn't represent the total cloud fraction down to the very thinnest cloud.

4) Section 3.1: thermal emission is neglected in these equations and could be simply added as an extra source term in equation 4 and 6, even if it is to be neglected in the further equations.

5) Line 249: As a suggestion, I think the overlap (transfer) coefficients should correspond to a level rather than a layer as they determine the transfer across the boundary between layers. It would then be useful to add the level being referred to for each T in equations 10, 11, and 12. Note then that eg. T_upˆck,cn(i) = T_downˆcn,ck(i), so the up and down arrows are perhaps redundant and the notation could simply indicate the upper cloud region, lower cloud region.

6) Section 3.3: While the formulation of the overlap rules is fairly clearly outlined here I think it would be better to provide the generalised formulas for the overlap between different regions rather than just the example case given. Especially as I think this method might be one of the key novel developments in this scheme. It would be particularly interesting to see how this new overlap scheme performs in comparison to a standard maximum-random approach which does not follow a core-shell model (i.e. a scheme where each region is maximally overlapped with itself but the overhang randomly overlapped with the other regions).

7) Section 3.4: I think this section requires further explanation with regard to how exactly your solver is implemented. Ideally, this should be explained in relation to the concept of entrapment explained in Hogan et al 2019. The method implemented in Shonk and Hogan 2008 corresponds to zero entrapment whereas the original Edwards and Slingo / Socrates method described in eqn 15 of Shonk and Hogan 2008 corresponds to maximum entrapment. It looks to me like your method also corresponds to maximum entrapment. It would be useful to indicate how your method differs from this.

8) Figure 9: This schematic is not entirely clear: I think the large downward radiation arrow should actually indicate the flux coming from just the upper dark blue region.

9) Section 5.1: At large zenith angles your TC schemes tend to approximate the 3D

heating better than the ICA: could this be due to your effective treatment of "maximum entrapment" in your TC solver, whereas the ICA effectively treats "zero entrapment" (from Hogan et al. 2019)? The effective treatment of 3D effects in your method should be discussed, otherwise the improved treatment of TC over ICA can only be interpreted as a cancellation of errors.

10) Section 5.1: The use of a constant FSD of 0.75 in these experiments muddies the comparison a bit as you are convoluting the error in using the constant FSD with the error introduced by the method to generate the LWC pair. You could repeat the experiments using the actual FSD in each layer to isolate error in the LWC pair method.

11) Section 5.2: The performance of the TC scheme for surface thermal flux should probably be compared with the ICA as the achievable benchmark as the entrapment implicit in your scheme would not have a large effect in the thermal and you scheme is effectively approximating the ICA.

12) Appendix A: this looks like something that would be better left to a user manual rather than a journal paper - with development of the package I suspect these instructions would change and the user manual could be updated accordingly.

Technical corrections:

1) Line 184: stemms -> stems

2) Line 434: Hill 2015 is referenced but is not in the reference list (Hill et al 2015: A regime-dependent parametrization of subgrid-scale cloud water content variability). This paper could also be referenced at line 480/481 in the conclusions.

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