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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An analysis of cloud overlap at a midlatitude atmospheric observation facility" *by* L. Oreopoulos and P. M. Norris

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Review of: An analysis of cloud overlap at a midlatitude atmospheric observation facility

by L. Oreopoulos and P. M. Norris

This manuscript presents an analysis of cloud fraction and condensate overlap as required by global atmospheric models in order to describe the geometric structure of unresolved clouds. Data from the ARM-SGP site were used. The analyses are comprehensive and both corroborate and extend previous results. As the results stand, however, without having a radiative sensitivity analysis to judge, readers are unable to decide how important some of the results are. Nevertheless, the manuscript was written well, there appears to be no errors, and the results should be of value for anyone

interested in describing unresolved cloud characteristics for use in global models. It is recommended that the manuscript be published subject to minor revision.

Specific points ————

pg 598; line 23: I disagree with the opening sentence. At this stage, cloud heterogeneity is not ignored in several areas of atmospheric research; especially global modelling, which is what the authors have in mind. I agree that it is not treated with much sophistication, and the issue the authors' are addressing in this manuscript is a case in point, but to say it is "generally ignored" is no longer true. In their next sentence they explain why they think it is generally ignored. Here they overstretch it too: i) it is not a computational burden given what one can reasonably expect to do; ii) while advanced means of setting the necessary parameters are lacking, best estimates are being used as a temporary measure; and iii) it is understood now how to convey meaningfully information about unresolved cloud fluctuations to at least the 1D radiative transfer process.

Having said this, this study is still perfectly valid... they should just tone-down their attempt to make this area of atmospheric science appear as though it is still totally benighted (which they more or less come around to saying by the end of the paragraph).

pg 601; discussion at top: The authors did not mention that for ground-based observations a potential issue is variable advection with height. Given the speed of satellites they are not directly subject to this (e.g., shearing effects are certainly there to be observed, but wind direction need not be aligned with satellite motion). One would expect wind-shear to add to the weight given to random overlap? Could this be systematic (for certain times and places)? On the other hand, perhaps it should be included in a GCM parametrization given the size of the grid-cells? But if it is largely avoided by satellites (and azimuthally-averaged) and factored in explicitly for ground obs, which one is correct (assuming their results differ)?

pg 601 and 602: I wonder if computing alpha etc... for all combinations of layers is going too far? Will a GCM parametrization ever be able to meaningfully address this problem to this level of detail (given the inherent uncertainties and lack of information they have to work with) - likewise for rank correlation? From the analyses performed here, the reader has no sense of how much one has to capture in the description of cloud structure to make 'satisfactory' estimates of radiative flux profiles.

pg 604; discussion at top: How large (i.e., important) is the radiative effect of representing cases whose overlap is less than random as random (i.e., negative values set to zero)? It might be small and not worth worrying about (especially coupled with frequency of occurrence)?

pg 605; line 28: "The choice of domain size affects the alpha profiles significantly". Judging from the lower plots in Fig. 1, domain size doesn't seem to be all that important???

Fig. 2. I'm not all that worried about negative rank correlations for separations greater than $\sim$4 km. First, there can be a substantial amount of cloud between layers separated by these distances. As such they can be radiatively quite separate and thus radiative transfer would be insensitive to the ranking. Second, radiation diffuses much after interacting with clouds separated by these distances. This tends to reduce the importance of details of rankings. In a sense, placing much importance on the details of ranking (and even alpha to a lesser extent) stems from the 1D ICA framework. Lightening up on the details (especially for large separations) and allowing things to be more random acknowledges somewhat, in an admittedly imprecise way, that we are actually working to simulate 3D radiative transfer not 1D.

pg 607; line 28: When the ensemble averages were computed were the alphas and ranks weighted (e.g., by total cloud fraction, or were they given equal weight)? Should one be concerned with weighting individuals to come up with monthly-means? I suppose it depends on what one intends to do with the month-means... construct a parametrization or just show results?

pg 610; line 20: "... changes substantially...". Is a change from 2 to 2.8 all that 'sub-

stantial'? What impact does it have on computation of radiation fields?

Figs. 6 and 7: The discussion surrounding these figures is interesting and novel (and reasonable). One wonders, however, just how important details of rank correlation are for these cases (especially if the variances of CWC are small, as they often seem to be for near-overcast layers)? Again, it seems to come back to the importance that these structural details have on radiative transfer.

pg 615; last line: It seems to me that you could perform the tests that you just outlined in your description of a possible research path without RIPBE data - Just apply your RT code to the 2D cross-sectional segments and then to corresponding fields generated stochastically as you just described. Since the RT is performed the same way for both, that leaves differences in the cloud fields themselves (which is easy to assess) and subsequent radiative impacts (which is equally easy to assess). It may be more hassle than it's worth to get into details about water vapor profile, surface albedo etc... At this stage you are not so concerned about comparing to observed radiative fluxes, right?

Final general comment: The manuscript is interesting and addresses a legitimate concern. One is left hanging, however, without a sense of how radiation will respond (after all, that is where the authors are coming from and largely where they're going). Hence, while it is difficult to find fault with the analyses and results as reported, which is not at all surprising given the high credibility of the authors, they do leave the reader with a sense of "what to do now, and what next?".

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