Response to interactive comment by H. W. Barker

Comments by Barker appear in *italic*

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).

We did not mean to give the impression that cloud heterogeneity is completely off the sights of atmospheric science, and more specifically, GCM, practitioners. Our intention in the introductory paragraph was to contrast full-scale 3D heterogeneity that needs to be described by two-point statistics with heterogeneity where spatial coherence does not need to be resolved, i.e., cloud fields defined by one-point statistics. We attempted to argue that while the description of the first kind of heterogeneity is harder, would require more information than is currently available in GCMs, and would demand complex and CPU-intensive algorithms, treatment of the latter type of heterogeneity is feasible to a considerable extent. But in both cases, it has yet to be demonstrated that predicting the degree of heterogeneity from the available model information is something we have confidence in at this point. We make the distinction clearer in the revised manuscript.

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)?*

We are mentioning this issue in the revised version of the manuscript. It is true that wind advection effects are not the same in satellite and ground-based observations. It is common for ground-based observations to assume fixed distances for fixed time periods, i.e., constant wind speed (see Hogan and Illingworth 2000), but this does not mean that it is a perfect assumption.

Wind shear certainly complicates the interpretation of the ground-based observations as 2D spatial fields. However, it is not clear to us how to account for wind shear in comparing satellite versus ground based measurements. Let us consider convective clouds at this time to develop our argument. A satellite observes wind shear impacted cloud fields over a 2D horizontal domain at an essentially instantaneous time, whereas the ground-based instruments observe wind shear impacted cloud fields at a constant location, but over a physically significant elapsed time. However, if the underlying shear-impacted cloud field contains an entire range of convective activity at different stages of development, it is not obvious that the two views (satellite or ground-based) will necessarily be very different in a statistical sense. More specifically, if the effect of wind shear in the underlying field is statistically stationary, the two views will be similar. Of course, if a cloud field contains an array of cloud elements formed at a similar time, or advected from a similar location, then shear will affect the field in a collective fashion over time, and the two views will be quite different. But if instead there is a continuous and protracted generation of convective elements and therefore a wide range of ages, then wind shear will produce effects of varying degree, depending on the age of a convective element. In this case, the effect of wind shear may be statistically stationary. We bring up these scenarios to point out that it may be difficult to make general statements as to the effects of wind shear on cloud overlap — it may depend strongly on the underlying dynamical mechanism for cloud generation.

That being said, if one does make the plausible assumption that wind speed increases with height, the effective spatial scales for a given time period also increase with height. If one would therefore want to perform the overlap calculation on a fixed spatial scale, a continuously decreasing number of 45m/10 sec cells would have to be used in the calculations. But exactly how to do this is not clear to us. It is also not obvious how rank correlations would be calculated in this case. Perhaps one should replicate some of the cells to reach the same number of cells as in the lowest layer? But this would seem to be potentially mixing in non-zenith correlations into the rank correlation calculations. For cloud fraction overlap the calculation of combined cloud fraction would be similarly problematic. It may be simpler to accept the discrepancy in the spatial scales for a fixed time period and try to assess the bias of the calculation. Assuming that the dynamical processes defining the scale of cloud cells act the same way at different heights (an assumption that admittedly may be imperfect) one would expect a smaller likelihood of large (incl. overcast) cloud fractions with increasing height for a fixed time period (because large cloud fractions are increasingly less likely in larger domains). This would further decrease the probability of maximum overlap. So, indeed, our fixed time intervals may be biasing our derived alphas low. Ideally, this bias should be accounted for in the GCM parameterization if it can be somehow estimated (we do not attempt to do so here, because frankly we are not yet confident of how to undertake such an exercise).

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.

We are trying to describe as best as possible the vertical structure of the cloud field. We agree that not all details may matter in whatever calculation uses the information on the vertical structure of the cloud fields, be it radiation or precipitation or some other quantity. A definitive answer can only be given only when these cloud-dependent quantities are calculated, something we admittedly do not address here (for more about this topic see also our reply to a later comment).

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)?

The suggestion to approximate weak minimal overlaps at relatively large separation distances with random overlaps for radiation calculations certainly seems reasonable. Actual calculations would need to be performed to quantify the (hopefully small) impact of doing so.

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???

Presumably, it all depends on what one means by "significantly". We think that the alpha profiles in the bottom of Fig. 1 are quite different. And one can possibly also debate what a "significant" difference is when alpha profiles are described in terms of decorrelation lengths (Fig. 3). Usually the alpha decorrelation lengths for a given month differ by more than a few hundred meters between the different domain sizes. For rank correlation decorrelation lengths we agree that domain size is not having a big impact.

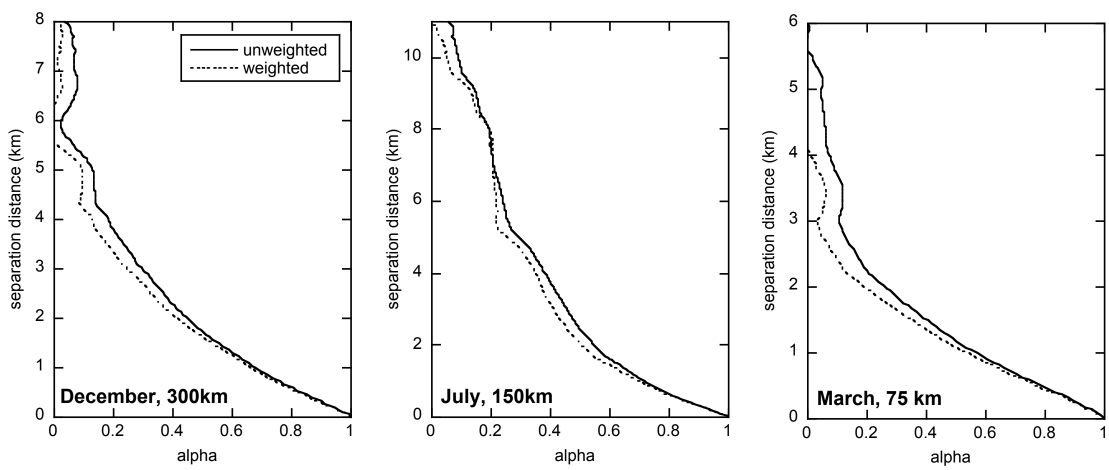
Fig. 2. I'm not all that worried about negative rank correlations for separations greater than _4 km. First, there can be a substantial amount of cloud between layers separated by these distances. As such they can be radiatively quite separate and thus radiative transfer would be insensitive to the ranking. 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.

We mostly agree with these points, and now include them in the manuscript. Thanks.

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?

The results shown are from ensemble profiles where the averages where calculated *without* using combined cloud fraction weights. It is not clear why using such weights would be more physically meaningful. Since larger combined cloud fractions tend to be associated with smaller alphas the ensemble alpha profile would have smaller values overall and the difference between weighted and unweighted profiles would tend to be larger for the larger separation distances where alphas are smaller. This is indeed shown in the figure below where we arbitrarily picked three different months for the three different segment lengths of our analysis. We show only the parts of the plots that are relevant to the decorrelation length calculation, i.e., no negative values of alpha.



pg 610; line 20: "... changes substantially...". Is a change from 2 to 2.8 all that 'substantial'? What impact does it have on computation of radiation fields?

We are on the verge of having a global scale answer to this question with GEOS-5. But one should realize that it will still be specific to the cloud fraction profiles of GEOS-5.

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.*

Again, we believe it may be valuable to gain a better understanding of the behavior and appearance of the cloud field while recognizing that not all details will be important to whatever is calculated from these cloud fields.

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?

One of factors that prevented us from carrying radiative transfer simulatioms was the computational cost of the Chou-Suarez code we used in our earlier paper (Norris et al., 2008, QJRMS). The vertical resolution of 45 m results in atmospheric profiles which have too many layers (267 for up to 12 km) for the radiation code to handle at 10 sec resolution (the computational cost of this particular radiation code increases quadratically with the number of layers). We think we'll have a solution to this problem soon by using a different RT code, RRTMG, for which CPU cost increases only linearly with the number of layers. We have developed the capability of running RRTMG on 1-min RIPBE data, but the quality of these runs hasn't been fully evaluated yet, so we don't consider our implementation ready yet for prime-time. While inclusion of atmospheric effects is not as important for the SW, it is important for the LW and may make a difference on the relative radiative contribution of clouds to the fluxes between winter and summer seasons. Also cloud contributions to the TOA SW fluxes change with the underlying albedo. So, it'd be preferable if the radiative calculations are performed as thoroughly as possible. In conclusion, we felt that the radiative testing was a significant enough undertaking to require a separate study, with a proper radiation code that can handle the high vertical resolution. A variety of tests can be performed then, including also testing effective decorrelation lengths you have described in your prior papers.

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?".

See our response to the previous comment. But overall we accept the legitimacy of the comment. Studies of why the overlap properties described in this paper matter are necessary. We have a plan in mind, but felt that we were not ready to implement it yet.