Authors’ reply to comments by referee #1 (Lazaros Oreopoulos)

We thank the referee for carefully reviewing the manuscript, providing the valuable comments and suggestions, which helped improving the original manuscript version. We incorporated the vast majority of suggested improvements. In the following the referee comments are presented in blue and the authors’ reply in black. Please note that the line and figure numbers refer to those in the original manuscript. Changes in the revised manuscript are marked with quotation marks and additional indent.

Here are some specific comments:

The assumption that the thicker part of the cloud will be towards the center is reasonable, but demonstrating that with fig. 3 is almost irrelevant because that figure shows the centers of multiple small clouds, which the GCM does not represent. The GCM implicitly only has a single cloud in a 50-100 km grid cell.

It is true that the GCM only has a single cloud in a grid cell, but in reality such cloudy layer would mostly consist of several clouds. Fig. 3 therefore shows the realistic shallow cumulus cloud field consisting of multiple clouds, which would all be subgrid clouds from the GCM point of view. Each of these clouds conforms to the core-shell model, where the optically thicker part is located in cloud interior. The averaged effect should be captured in a GCM. (See also line 103: “Clouds in a cloud field have multiple cores, whereby their aggregate effect can be modelled with a core-shell model.”)

It is not clear how clouds overlap is treated when the cloud layers are separated by a clear layer. Is it random overlap then? How would one of the Fig. 8 panels look if there was a single clear layer between the two cloudy layers? So, is the overlap only considered for neighboring cloudy layer pairs? Are pairs of cloudy layers that are distant completely independent even if there is no clear layer in-between? In other words, only pairwise coupling of fluxes is considered? Generalized (exponential-random) overlap can overlap any pair of clouds, but of course explicit radiative treatment is messy (if not impossible) without subcolumns.

Yes, if cloudy layers are separated by a clear layer, they overlap randomly. This is stated in lines 273-275: “We apply the widely used maximum-random overlap assumption (Geleyn and Hollingsworth, 1979) for the entire layer cloudiness (sum of optically thick and thin cloudy regions), where adjacent cloudy layers exhibit maximal overlap and cloudy layers separated by at least one cloud-free layer exhibit random overlap.” Correct, we have only considered pairwise overlap. This is expressed in line 309: “Pairwise overlap as employed here ensures that the matrix problem is fast to solve.” So the maximum overlap is applied in pairs of adjacent layers for the entire layer cloudiness as well as additionally for the optically thicker part. In order to further emphasize and clarify the latter issue we changed the sentence in line 277 to:

> “We additionally assume the maximum overlap of optically thicker cloudy regions in pairs of adjacent layers.”

Similarly, as for the entire layer cloudiness, the random overlap is automatically fulfilled also for the optically thicker cloudy regions separated by at least one cloud-free layer.

I’m not convinced that this method is better than McICA because the latter can operate on any subcolumns which can be generated with more realistic rules for overlap and LWC subgrid variability (and its overlap). I mean, if exponential-random agrees better with observations, why not try to use it? The authors state that the McICA noise may be undesirable and impactful, and that’s perhaps true, but perhaps this is less important than achieving smaller systematic biases? I don’t buy the argument in lines 72-74 that fewer spectral intervals will make McICA worse. This seems to assume that you have to produce only as many columns as g-points so that each column is paired with one g-point, but this doesn’t have to be the case. One can easily generate Nc-multiple of g-point subcolumns (i.e., a total Nc*Ng subcolumns) of so that the same spectral point operates on Nc subcolumns. This will reduce the noise. The fewer g-points, the less the McICA noise, actually.

We are not saying that the TC is better than the McICA, rather a possible alternative, which however requires further evaluation. You are right – whereas our current overlap formulation should be well suited for the present shallow cumulus case, it is inadequate for vertically developed cloud systems in strongly sheared conditions. Therefore we plan to generalize the overlap rules in the next step. As explained in the text the impact of the McICA noise can be harmful (inducing undesired feedback loops) – for example for low clouds, which are essentially maintained by local cloud top radiative cooling. Thereby the TC might be a better option, eventually also leading to smaller systematic errors in such critical cases. We have however additionally emphasized that the McICA is computationally faster than the TC. The argument about fewer spectral intervals worsening the performance of the McICA is summarized after Hogan and Bozzo (2016) as stated in the sentence. We have however improved this part as suggested also by referee #2:

> "In contrast to the McICA, which is still operational also at EMCWF due to ist higher computational efficiency, the TC scheme does not produce any radiative noise. As suggested by Hogan and Bozzo (2016) this superiority could become even more valuable in the future if an alternative gas optics model with fewer spectral intervals than the current RRTM-G (Mlawer et al., 1997) will be developed, since this would increase the level of the McICA noise, but it would not affect the Tripleclouds. In other words, in order to limit the McICA noise in this case, oversampling of each interval would be required, which could increase the computational cost of the McICA to a similar degree as that of the Tripleclouds scheme."

The fair comparison of the McICA and the TC is beyond the scope of this study, but is should be carried out in the next step.

Line 39: Barker (1996) is not the best reference in this case. That paper deals with horizontal inhomogeneity of single cloud layers, therefore irrelevant for GCMs. A much better reference is Oreopoulos and Barker, QJRMS (1999) which deals with multiple vertically overlapped cloudy layers each of which has a gamma distribution of LWC. That scheme was specifically designed for GCMs and was actually deployed on a couple (no papers exist though), but was quickly superseded by McICA.

Thank you for this suggestion. We changed the reference to Oreopoulos and Barker (1999).

Lines 27-29: When reference is made to the maximum-random overlap assumption, everyone assumes that there is a unique implementation, but that's simply not true! Indeed, there are various flavors. Geleyn and Hollingsworth (1979) may actually be the best one. But there is actually the cloud "block" implementation of max-ran, which is visually captured in Fig. 10 of Chou et al., JAS (1998). In GH79, two cloudy layers that have another cloudy layer in-between are still assumed to be maximally overlapped for the common portion they have with the in-between layer, but the portions of the layers that correspond to the clear fraction of the in-between layer are randomly-overlapped. In the Chou et al. representations these clouds would be maximally overlapped if they belonged to the same high, middle, or low block. Only the blocks themselves are randomly overlapped. But the Chou et al. (1998) is still called a max-ran scheme, yet is it very different than GH79! Also, incidentally, the Morcrette and Fouquart paper does not discuss or advocate for max-ran overlap. Rather, it compares, max, min, and two versions of random overlap.

We kept the original reference of Geleyn and Hollingsworth (1979), as we also think it is the most appropriate in this case. Furthermore, we removed the reference of Morcrette and Fouquart (1986) in the context of advocating the maximum-random overlap.

Line 306, leave it to the reader. Well, it's not very common to ask such a thing! Why don't you include these other three cases in the Appendix?

Thank you for pointing this out. We added an extra Appendix section and changed the sentence to:

> "The derivation of overlap coefficients for other three geometries involves analogous considerations, whereby the resulting formulas as well as their generalized formulation are given in Appendix B."

And a few minor corrections:

Line 61: "pioneering"

We agree that “pioneering” is not the correct term. We changed the sentence to: “In the primary work of SH08 ...” in line 61. We further removed the word “pioneeringly” from line 16.

Line 79: “exertion”? You mean version?

Apparently the “exertion” was not the best wording. We changed it to “incorporation”, which also makes this sentence consistent with the paper title.

Line 88: “pairs”.

Changed.

Table 1: Odd to call the third experiment “TSM”. All experiments not conducted with MC are TSM. So strictly speaking, you have TSM-ICA, TSM-HOM, and TSM-GCM. You could have also conducted the experiment in the middle field with MC, i.e, MC-HOM. Would still have 3D effects because of finite cloud sizes, but no effects due to internal cloud heterogeneity.

You are right, these experiments could be named “TSM-ICA”, “TSM-HOM” and “TSM-GCM”, but we prefer to name them as short as possible, assuming it is clear they have all been performed with the two-stream method as described in the text. Therefore the first one of the three TSM experiments is simply called “TSM” (to distinguish it from the Monte Carlo ICA experiment, which is termed “ICA”), whereas other TSM experiments are abbreviated to “HOM” and “GCM”. Yes, we actually conducted the MC experiment on the cloud field with removed horizontal heterogeneity as well, but it is not presented in the paper, since it does not bring any other conclusions.

Line 376: “validity”

Changed.

Lines 378-379: No discussion of the Fig. 12 results?

Thank you for this suggestion, we extended the paragraph as follows:

> “Further, to test the sensitivity of TC radiative quantities to the assumed form of the subgrid cloud condensate distribution, we employed the FSD method in conjunction with all three distribution assumptions (Gaussian, gamma, lognormal). The resulting LWC profiles are shown in Fig. 12, demonstrating that the LWC pair characterizing the two cloudy regions is clearly sensitive to the distribution assumption, when mean global FSD estimate is used as a proxy for cloud horizontal inhomogeneity degree.”

Line 419: You mean rightmost column?

We simplified this parenthesis to contain only the figure number, as one should actually compare the middle and rightmost column.

Line 476: Or there was no bias reduction at all!

We changed the sentence part to: “... the nighttime bias was slightly enlarged, ...”.

Lines 485-486: Another marine BL cloud classification you may want to mention is this: https://www.atmos-meas-tech-discuss.net/amt-2020-61

Thank you for providing this reference, which we included in the text:

> “The classification of rich spatial patterns into various mesoscale cloud morphologies can thereby valuably be performed with deep learning algorithms (e.g., Yuan et al., 2020).”