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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Directional, Horizontal Inhomogeneities of Cloud Optical Thickness Fields Retrieved from Ground-Based and Airborne Spectral Imaging" *by* Michael Schäfer et al.

Anonymous Referee #3

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This study compares a number of different metrics that describe horizontal cloud inhomogeneity. These metrics all derive from different papers, although the authors here calculate them all for the same set of cloud fields, which are derived from ground-based and aircraft measurements of cirrus and stratus from two field campaigns. The authors start with the traditional one-dimensional metrics, but then evaluate metrics that describe the clouds in two dimensions, which allow for differences in inhomogeneity parameter that is due to different structures, for example, along-wind and across-wind.

The paper is generally well-structured and the figures clearly lead the reader through the material. Some parts of the text, however, are not easy to follow and need revising – see the various comments below. However, and this is perhaps my greatest comment

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Discussion paper

on this paper, there is no real context to the work, no stated reason why the study is being carried out, and at the end there is no discussion of the implications of this study on the scientific community. More context and discussion needs to be added before the paper is ready for publication.

Comments. . .

Abstract – there are too many technical terms in the abstract. While reading it, it was not clear to me what the “decorrelation lengths” and “scale breaks” refer to (lines 11 to 14), and without further context, I did not know what the authors meant by “directional cloud inhomogeneities”. All became clear having read the paper, although this defeats the point of the abstract – it should be a standalone block of text that is entirely contained. These terms need clarification in the abstract.

Section 1 – the authors cover a great deal of literature in this section, although in places in a somewhat haphazard manner – they may want to have another think about the structure of the section. See following comments.

Lines 41 to 42 – to me it is unclear what the sentence “a variability of cirrus albedo of up to 25% due to spatial cirrus inhomogeneity” means. I assume it refers to a plane-parallel bias, although the sentence appears in advance of the discussion of the plane-parallel approximation (from line 46).

Lines 46 to 60 – this paragraph is very muddly and confusing. It starts off introducing the plane-parallel approximation, before jumping to the independent pixel approximation, which they say is used in 1D radiative transfer calculations, but it follows after a mention of GCMs, perhaps suggesting that the IPA is used in GCMs. This paragraph should be reworded such that the distinction is clearer. The paragraph continues to cite 50% and 8% errors from two different studies, although no comment is made as to why these are so different.

Lines 64 to 68 – there is some confusion in this paragraph. It starts with a mention of

Monte-Carlo simulations, and then suddenly into cloud overlap schemes, which are a completely different area to horizontal cloud inhomogeneity. The authors also assert that Tripleclouds is a cloud overlap scheme – this is not true: it is a method for representing horizontal cloud inhomogeneity in GCMs. Additionally, the scheme is called "Tripleclouds", not "Triplecloud". The following description of Huang and Liu's (2014) method is also not clear. This part of the literature review may need restructuring – as the paper is dealing with metrics of horizontal inhomogeneity, it would be better to introduce methods of accounting for horizontal inhomogeneity in a more clear manner – plus the authors may want to include the Monte-Carlo ICA method of Pincus et al (2003).

Line 75 – why has the ECMWF mode been singled out as an example here? Surely all models need sub-gridscale parameterisations of cloud structure.

Line 84 – the authors say that 1D measures of inhomogeneity can lead to unrealistic results. To describe them as unrealistic does not really feel fair. A 2D metric of horizontal cloud structure is more descriptive of the cloud field, but 1D metrics can still provide information about the "bulk" structure of the cloud field over all angles.

Lines 90 to 94 – the entire scope and contents of the paper are summed up into a short paragraph of four lines. The contents of the paper could be described in a little more detail. Also, this paragraph (and indeed section 1) is lacking in aims and motivations of the study – indeed, all it says is what the authors will do in their paper, with no science questions. This needs expanding.

Lines 99 to 100 – this is not a particularly important point, but it would be great to get a better idea of how the scans are made – for example, are they made from a long time series of 1D scans across a path, or are they full 2D scans that overlap? The papers referred to here probably describe this fully, but it would be nice to have a little more information here.

Lines 115 to 121 – the wording of this argument is not particularly clear. I think the authors are saying that optical depth is better to use for this analysis because it is not dependent on scattering angle, while radiance is more directional. This could be better explained.

Line 142 – the authors say that the normalised inhomogeneity parameter can exceed one in some cases, and that this could lead to misinterpretation. I am not sure what they mean by misinterpretation – mathematically, it can happen.

Page 7 – through much of this page, the authors compare the different measures of 1D inhomogeneity from their study with values from other studies and say that they "compare well" and similar. It should be noted that these inhomogeneity parameters can be highly related to the domain and pixel size of the observations in the studies (see Shonk et al 2010, part one). To say that values "compare well" is probably only fair to use when comparing with studies that use very similar domain/pixel sizes. The same is also true in the conclusions in lines 398 to 399.

Line 204 – the authors take the square of the autocorrelations to get an absolute correlation, which is fine, although I would contend that finding negative correlations is not necessarily ambiguous.

Line 209 – if the positive and negative signs indicate the shifting direction, I would say they do have physical meaning.

Figure 3 – this caption could do with a little more information. The caption is entirely written in mathematical terms – it is clear what these mean by reading the text, but defining them briefly in the caption could be helpful.

Figure 4 – why are the red and blue bars stacked? This gives the impression that the authors are summing together the L_x-direction and L_y-direction decorrelation length scales – by the looks of Table 2, this is not the case. I assume they have been arranged in this way because the blue bar is always taller, implying that the decorrelation length in the L_y-direction is always greater. (Which itself is an interesting question that is not

addressed.)

Lines 256 to 257 – the authors provide a conclusion for section 4 with the sentence “A comparison can only indicate . . . inhomogeneities.” Many aspects of this sentence are unclear – indeed, I am not sure what the authors are concluding.

Lines 348 to 352 – the authors introduce the reduced noise terms, but again perhaps a little too technically for someone who does not know how to do it – they may want to consider revising these few lines to make the method clearer.

Line 375 – is the CARRIBA cirrus “more homogeneous” than the VERDI stratus? Earlier, the authors say that cases C-02 and C-03 are more homogeneous than C-01 and C-04, and in Table 2, we see that they have larger decorrelation length scales. But the decorrelation scales from the VERDI stratus are smaller than those from the CARRIBA cirrus, which surely means that the cirrus are more inhomogeneous than the stratus? Or am I missing something?

Line 408 – I think this first sentence is saying that, when determining cloud inhomogeneity, it is important to consider the full horizontal structure of clouds via a 2D metric rather than a 1D metric. The authors may want to revisit this sentence – at the moment, it seems to imply that performing 2D analysis shows that 1D analysis works.

Lines 443 to 444 – the final conclusion seems to be that, despite having gone to all the trouble of calculating several different measures of 1D and 2D inhomogeneity, “the directional structure of cloud inhomogeneities generally should be taken into account”. This feels like a weak concluding comment. For a start, what is meant by “generally”? Clearly some clouds will have less directional dependence of cloud inhomogeneity, but this does not mean it is not useful to measure it. How is best to determine this two-dimensional inhomogeneity? Which of the methods compared performed best? Which is easiest to calculate? Which is most useful if, say, 2D inhomogeneity parameters were to be included in a climate model? Also, how easy is it to obtain observational cloud data to extract these 2D cloud fields – are they readily available, or should we be carrying out more of these field campaigns to build up a better idea of cloud inhomogeneity? And is there any cause to investigate 3D cloud inhomogeneity metrics? These are a few questions that popped up in my mind having read the paper – it is disappointing that the authors have not included any such consideration on the implications of these results on the scientific community, and how the work could be taken forward.

Specific comments. . .

Line 9 and 97 – should "VERical" read "VERtical" (or is it spelt like that in the project name)?

Line 105 – ". . .full width at. . .", not ". . .full with at. . .".

Table 1 – the four cirrus cases are all labelled "C-01" – should these be "C-01", "C-02", and so on?

Line 192 – "both dimensions", not "both dimension".

Figure 4 – "Decorrelation" on the y-axis has been written as "De-Correlation".

Line 239 and 247 – the value of 3154 m is shown as 3153 m in Figure 3.

Line 262 – "in a horizontal direction", not "in horizontal direction".

Lines 304 to 305 – "E_a and E_b" has been written as "E_a and E_a".

Lines 328 and 329 – "inhomogeneity" has been written twice as "inhomogeniety".

Line 337 – "too small", not "to small".

Line 346 – "overlaid", not "overlayed".

Line 360 – "decreases to 2.2" should be "decreases to –2.2" (based on the numbers on Figure 6).

Figure 7 – the caption seems to be a copy of the caption of Figure 6.

Line 408, 411 and 435 – the authors switch here between P_τ and P_τ^2 – is there any reason for this?

Line 473 – “Green-function”, not “green-function” (I think they are named after someone called Green).

Line 480 – “absorption”, not “absorbtion”.

Lines 524 and 526 – check the spelling of “Schroeder” vs “Schröeder”.

Line 538 – “Earth’s”, not “Earths”.

Finally, the authors have started several sentences throughout the paper with numbers and variables – this is something that may get picked up later in the review process, but they may want to revise these sentences so that they start with words.

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