

Interactive comment on “The impact of atmospheric dynamics on vertical cloud overlap over the Tibetan Plateau” by Jiming Li et al.

Anonymous Referee #2

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This paper looks at cloud overlap over the Tibetan Plateau. Studies have calculated cloud overlap globally before, although resulting parameterisations have not been valid for clouds over the Tibetan Plateau, so the authors use satellite radar and lidar data to build up two-dimensional slices of cloud and evaluate the overlap parameter (based on the description of Hogan and Illingworth, 2000). They also use ECMWF data to describe the meteorological conditions and derive along-track wind shear and conditional instability and investigate the local effect of overlap parameter on these quantities. They find overlap here to generally be minimum for discontinuous clouds and distant layers in continuous clouds, and define a new overlap parameter that varies between minimum and maximum overlap. The decorrelation scale of this parameter is then regressed onto wind shear and conditional instability to produce a new equation for

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overlap as a function of these quantities over the Tibetan Plateau that performs better than many standard methods.

(I should point out at this point that I wrote much of this before reading the comments from Reviewer #1 and, if indeed the authors are using the wrong variable, some of the following comments may well not be relevant after changes according to this.)

Generally, I thought the main points of the paper were interesting, and the authors' new equation for decorrelation scale is a useful result. However, the presentation of the work needs a great deal of attention. This paper came across as a list of everything the authors did, with a description of every result, yet little scientific insight as to the implications of what they found. The result of this was that the reader is faced with through the details of several minor results (in particular those presented at the end of section 2) before reaching the key results in the later parts of section 3.

The highlight of the paper for me was the new overlap parameterisation and the comparison with the other overlap methods, which was presented in section 3.3. To draw more attention to this, I strongly recommend elaborating on the detail of this part of the experiment and heavily trimming some of the less important early results (again, those at the end of section 2). I would also recommend tidying up the results section. The authors tend to provide a great deal of information about what a figure shows, but significantly less on its interpretation.

In summary, I think the results are interesting (although the authors should definitely check the comments from Reviewer #1), but the text needs to be heavily edited and restructured before this paper is suitable for publication.

Here are a few specific comments I had while reading.

At a few points throughout the paper, there are allusions to the climate changes experienced by the Tibetan Plateau, but little detail on what these changes are, and indeed how misrepresentation of cloud overlap could affect predictions of these changes.

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Line 73: overlap parameterisations, rather than overlap states. They are not really states that occur in the atmosphere, but parameterisations that we apply to them in models.

You alternate throughout between cloud cover and cloud coverage. Cloud cover is more commonly used.

Lines 75 onwards: you introduce maximum, minimum and random overlap, but do not state exactly how they are calculated. Describing the cloud cover for two layers under each overlap assumption is useful, but a proper physical definition of what the overlap schemes mean will help. (This could also belong in the data/methods section.)

Line 88. “Accurate” perhaps isn’t the word you mean – this implies that the radar always calculates the exact masks of the true clouds.

Line 94: “space-based” is better than “space-borne” (the satellite is not carried along by space!).

Lines 135 onwards. This section was not clear, and I needed to read it multiple times to get an idea of what you meant. For a start, I assume that the CloudSat profiles are columns of zeros and ones for clear and cloudy, which are then combined to give two-dimensional scenes? Are CloudSat bins the same as CloudSat profiles (presumably the bins are height layers within the profiles)? And what are the horizontal and vertical resolutions referring to? This all needs to be made clear or the reader will struggle to follow.

Section 2.3. The first time I read this, again it was not clear here why and how is the domain divided into different domain sizes. You should probably describe this more explicitly. I assume the data is simply divided up into sections of these different lengths? You also define two terms (“spatial sampling size” and “domain size”) for this quantity, but then proceed calling it something else (“spatial sampling scale”).

Line 205. Going back to my main points above – I don’t understand why this result is

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in the paper in this much detail. Figure 2 shows that there is little difference in terms of overlap statistics whether the threshold is 99% or 50%, but I don’t see how this particularly justifies the use of the 99% threshold. I wonder if this result is even worth a mention – removing it would mean that the definition of the threshold is no longer necessary.

Several of your paragraphs are far too long. A reader will be daunted by paragraphs that span multiple pages. I recommend breaking these paragraphs down into manageable sections that describe one element of the study.

Figure 3. Putting panel (c) to the top right would match the position of the equivalent panel in Figure 2.

Line 235 to end of paragraph. I am not sure what you mean here – I think it is about the domain size needing to be greater than the cloud scales at each level. But now there is another threshold that is introduced of 50% that is different in definition to the previous threshold. Basically, as far as I can tell, the overview of the latter parts of section 2.3 is to pick a set of values for the two thresholds and the spatial sampling scale. This should probably be partitioned off into a separate section.

Sections 3.1 and 3.2. These were very difficult to read and need reworking – the material within them is fine, but the key results need to be emphasised and worked into more of a scientific story.

Table 1 and Figure 8: which of the overlap schemes in the table are yours? By the looks of it, it’s 5 and 6, but it wasn’t immediately obvious. Why not give the schemes names that highlight those that are yours, then when you compare the performance of the schemes over the Tibetan Plateau, it is easier to see that yours perform best.

Figure 8. This figure would be clearer if the colour bar used white for zero. Then it would be clearer which overlap schemes produce biases.

Section 4. Your conclusion section is just a summary of the results and some future

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ideas, with little extra insight. You need to place your results here into context. You allude at many points along the way to climate change and modelling over the Tibetan Plateau and how radiation budget is affected by cloud overlap issues here – how could this parameterisation help?

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