Review of "A climatology of open and closed mesoscale cellular convection over the Southern Ocean derived from Himawari-8 observations"

by Lang et al.

submitted to Atmos. Chem. Phys. Disc.

Summary

The study by Lang et al. presents a first classification and analysis of mesoscale convective organisation in low level clouds on the sub-daily timescale in the Southern Ocean. Utilising geostationary observations by HIMAWARI-8, the authors use a retrieval insensitive to cloud phase unlike previously used retrievals utilised for cloud classifications. The analysis demonstrates the skill of their convolutional neural network approach in identifying open and closed cells. The climatology and spatial relation of open and closed cells to cyclone and cold front activity within this dataset are presented. While some differences to previously published datasets are identified and discussed, the overall preferred occurrence of mesoscale organisation in marine stratocumulus within the cold sector of cyclones and marine cold air outbreaks is confirmed. In addition the importance of the polar front in constraining open- and closed-cell clouds is discussed.

Recommendation

Low-level stratocumulus clouds are the dominant low-cloud type in the midlatitude Southern Ocean region, with many of them being mixed-phase clouds. Many facets of these clouds in this remote region of the world are largely unexplored. At the same time global climate models have been shown to struggle to accurately simulate their cloud-radiative effect. Previous work had emphasised the importance of cloud fraction and mesoscale organisation for cloud field albedo.

The results of this study thus address one of the key uncertainties of low-level clouds in the Southern Ocean and provide new insights regarding their occurrence and the underlying processes driving cloud organisation.

The manuscript is very well written and structured. Furthermore, their findings are discussed in a concise and comprehensive manner. I only have minor queries regarding aspects of their analysis. Once addressed, I can recommend this manuscript to be published in ACP.

General Comment

My main concern in your study is with respect to the training data. As this is a defining aspect of the quality of your CNN, I would like a more comprehensive discussion of:

i) how you generated these data

ii) how you may have introduced an implicit sampling bias by your criteria of identification and scene selection.

P3L82ff: How did you build your training dataset exactly? Why did you choose a combination of all of these variables and how did you implement it?

P4L110: Why did you use brightness temperature as the variable for training the CNN? And why did you train the CNN in channel 11, but identify the training dataset in channel 10?

P4L117: This means that each point is used multiple times in a classification. Once as center point and the other times it is part of the classification for its neighbouring 7 points in each direction. Are these overlaps considered in your overall classification? Or is each point only classified once as center?

P5L128ff:
Were your cloud scenes only identified by only one person? Are their concerns with objectiveness in scene identification (e.g. Stevens et al. 2020)?
Stevens, B, Bony, S, Brogniez, H, et al. Sugar, gravel, fish and flowers: Mesoscale cloud patterns in the trade winds. *Q J R Meteorol Soc*. 2020; 146: 141– 152. https://doi.org/10.1002/qj.3662

Based on your description you identify open-cell as "stringy" clouds. Does this mean that you only picked scenes of low cloud fraction as open cells? Do you observe most cloud fractions in both regimes (as in McCoy et al. 2017) or are they distinctly separated? I.e. you only sample low-cloud fraction open cells and high-cloud fraction closed cells?

How are your 400 independent cloud scenes split across open, closed and "nothing"? And how did you split these scenes into a seperate training and evaluation dataset?

P9L267:
These findings with respect to a diurnal cycle are really interesting and completely novel. It seems consistent with the effects of increased SW insolation, partially compensating cloud-top radiative cooling.

This links back to my question posed on cloud-fraction sampling in your scene identification. It is conceivable that scenes with a higher cloud fraction are more susceptible to this process. Thus, if your identified open-cell clouds are generally characterised by low cloud fraction and little detrained cloud, your results for open-cell stratocumulus may be impacted by this selection?

Specific Comments/Edits

P1L17: "These biases…" Results by Zelinka et al. (2020) suggest that this bias has been "fixed" in many of the new generation climate model runs. While it is not clear how physical the individual approaches of the individual models are, the drastic shortwave bias seems to have been compensated for in some of them. This may be worth mentioning here for completeness.
Reference: Zelinka, M. D., Myers, T. A.,McCoy, D. T., Po-Chedley, S.,Caldwell, P. M., Ceppi, P., et al. (2020).Causes of higher climate sensitivity in CMIP6 models. Geophysical Research Letters, 47, e2019GL085782. https://doi.org/10.1029/2019GL085782.
P2L35: "Overnight…" I am not sure there is a strong diurnal cycle in surface moisture fluxes. Isn't it more the absence of solar heating which partially compensates the LW cooling driving turbulence, that allows these clouds to recover?

P4L109: In line of transparancy it would be helpful if you state which CNN you used (i.e. python package, ect.)

P5L140: The figure suggests that the maximum accuracies for training and validation are at 99%. Please clarify/rephrase.

P5L142: What are the 89% the average of? In your table it says 89% for open and 93% for closed.

P5L143: Following Figure 5, open MCC are far more frequent than closed MCC, so how come they have the lowest training sample size?

P5L148: Are these surface winds? Near-surface winds? Please clarify.

P5L150: I fully agree with your statement. You may present it more convincingly by using a lower wind speed threshold in your figure, or showing a different height of wind speed. Its difficult to speak of "frequent", when the highest frequency is about 2%

P6L156: Doublecheck hyphen. Should it be “warm-water-cool-air contrast”?

Figure 3c/P6L161ff: It may be nice to add a line to mark the location of the polar front for clarity. Reading on it becomes clear that you actually plot the polar front in Fig. 5.I suggest to either link description of polar front location to that figure, or add the location in the SST gradient plot.
P6L168: This is not clear to me. As I understand you also use the SST gradient exceeding a threshold like in Dong et al 2006? Please clarify.

P7L198: How do these numbers relate the the overall number of identified open and closed cell scenes? Otherwise its hard to get the context of what fraction of these regimes were actually associated with cold fronts.

P7L212: It may be helpfull for reference to know how large these images are? I.e. how many 16x16 point segments are in one image? Apologies if I missed it. I am assuming its several, since you identified 25'654 open-cell systems associated with cold fronts alone.

P8L239: Stippling the region of local minimas in both plots, may be a helpful visual aid of your description and make your point clearer.

P9L252: I agree with this conclusion on uncertainty. In my mind this is also evident in Fig. 4b, where clouds in the Southeast corner of the domain are still classified as open-cell.

P9L253: The seasonal cycle was already addressed in previous work (e.g. McCoy et al. 2017). Its worth adding a comment about how consistent your findings are.

P10L303: Please also provide a distance for the closed-cell location in this comparison for clarity.