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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Understanding the drivers of marine liquid-water cloud occurrence and properties with global observations using neural networks" *by* Hendrik Andersen et al.

Anonymous Referee #1

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I appreciate that the authors have attempted to diversify the ACI investigation field with the use of neural networks. It is often difficult with studies such as this that attempt a new analysis method to create a coherent message. However, I do not think this paper can be published in its current form. My overarching concern in this paper is that the authors do not articulate what the new thing is that they bring to the table besides the black box of a neural network.

I have grouped my concerns about this paper into the following categories:

Statistical evaluation: It is unclear to me why doing multiple neural networks on sub regions on monthly data tells us anything useful about what is going on. I need some sort of confidence that a high R2 model cannot be created by a large neural network

using a collection of meteorological predictors picked at random. Monthly data has the issue of being driven by the seasonal cycle, which will drive almost everything else, and making it regional will mean that the neural network doesn't need to tell us anything particularly meaningful about how the clouds are driven by their environment. The authors should consider using anomalies relative to the seasonal mean, or simply using annual means. Either of these options would be better than the approach taken in this paper. Admittedly the authors talk about this on page 2 line 25, but they don't provide any convincing proof that they haven't just created a regional seasonal cycle simulator.

On page 4/line 10 the authors note that they throw out models that have a low R2. I'm not sure why this is ok to do.

On page 6 I find this something of a straw man. A better test would be to compare a multiple linear regression of all the predictors to the ANN, as opposed to a regression on AOD alone. Or to compare the ANN trained using only AOD. I think that the paper would actually be vastly improved by just repeating the analysis with a multiple linear regression to demonstrate to skeptical readers why their paper brings anything new to the table as compared to the numerous previous papers that have looked at ACI and low cloud variability in the past.

Figure 5- If the error bars give the range in sensitivity does that mean that nothing except LTS and AOD have a robust relationship with cloud properties that holds outside of a few regions? Didn't we already know this very well from simple regression models that were easy to interpret (Klein & Hartmann, 1993; Nakajima, Higurashi, Kawamoto, & Penner, 2001)?

Choice of predictor/predictands: The choice of predictors by the authors is not appropriate for a paper in the last decade. Why have the authors chosen AOD to be a CCN proxy? AOD is not equivalent to CCN since it has a large contribution from larger, non-CCN relevant aerosols. Why don't the authors use AI, which is far more relevant and typical of more recent studies (Patel, Quaas, & Kumar, 2017)? The authors acknowledge this, but then shrug this off because papers from almost a decade ago do it. In a similar vein, why do the authors use effective radius instead of CDNC? Effective radius for a fixed CCN increases with increasing LWC, making it sensitive to meteorological drivers. The authors do acknowledge this in page 10, section 25 noting that the interaction between inversion strength and effective radius is most likely driven by variations in LWC. This makes the interpretation of the CDR as a proxy for aerosol-cloud effects muddied. Further, the authors use LTS. Why not use EIS, which is used by every study investigating low cloud in the last decade (Myers & Norris, 2015; Qu, Hall, Klein, & Caldwell, 2014; Seethala, Norris, & Myers, 2015; Webb, Lambert, & Gregory, 2013)? Finally, I am concerned with the use of RH. Clouds and RH are a semi-equivalent quantity, which may just mean that they are comparing ECMWF-interim's cloud cover to MODIS, further aliasing in the seasonal cycle to their prediction model.

Writing: The writing is rushed and hard to follow. Clearly expressing why the methodology is valid is crucial for this study and as such the writing needs to be tightened up substantially to clarify their ideas.

Summary The authors articulate their guiding hypotheses, which I think is a good thing to do. I am not sure why (1) is a hypothesis. It seems to be more of a statement about neural networks and is worrisome since I am still concerned that the neural network is just looking at the seasonal cycle and is guaranteed to get a high R2. (2) is odd. Why would we have regional patterns? I could see it if this was a regime-dependent analysis (eg stratus vs convection), but the use of w and LTS as predictors in the neural network should mean that the authors can create a single neural network that effectively does this for them. Why is this not the case? What makes a specific lat-lon box a natural choice. (3) seems to imply that meteorology plays a secondary role to aerosols, which is not true. We don't expect aerosol to tell us where convection and stratus are, for instance.

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