

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 #3

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This paper pursues a promising approach to study the sensitivity of marine liquid-water cloud properties on a set of meteorological and aerosol predictors, using an artificial neural network approach. It steers clear of correlative approaches for studying aerosol-cloud interactions and instead considers the meteorological context, segregated by region / meteorological regime. In essence, this amounts to a multi-variate analysis based on an optimal combination of satellite and re-analysis data. The paper is very well written, clearly represents new ideas, and has the potential to lead to major improvements in our assessment of ACI, regionally and globally. It is rare to see such a high-quality paper. I only have minor comments, which don't necessarily have to be addressed in this manuscript, but could be considered in future work. The most impor-

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tant ones are probably #1 regarding scale, and regarding the quality (reliability) of the data. Also, follow-up papers might consider using the co-sensitivity of some predictors (details below).

In a separate comment to the editor, I recommended that the paper be highlighted because it seems highly innovative in its approach and deviates from the traditional correlative aerosol-cloud interaction studies. I believe that it has potential to change the direction of this field of research.

General comments:

p5,L18: In the spirit of the McComiskey and Feingold ACI papers, it would have been interesting to also consider the impact of scale on ACI relationships. Here, one specific scale has been used (dictated by the analysis grid) - but it may not be straightforward to generalize these relationships.

p6,L4: “skill of simple correlation between AOD & cloud properties”: It is a bit unclear, which “simple correlations” specifically have been used for this study. This statement calls for elaboration. The statement on p6,L6/7 shows the intent - the “simple correlations” are used as a baseline to show the improved predictive skill of ANN. The quantitative results would be more useful by including more information about that baseline.

p6,L11 (fig 4): How/where are the equal-area regions defined? Are those just pixel aggregated that meet the selection criteria for the sensitivity analysis?

p9, Fig 5. How is the CF and LWP sensitivity to AOD compatible? Is it a fair statement to say that we get more clouds with lower LWP for higher aerosol loading, while COD stays the same (perhaps because the “classical” indirect effect kicks in) - or can we not make such a blanket statement?

p10, L5: Would it make sense to plot co-sensitivity maps, considering that many predictands co-vary with predictors. In the inverse theory equivalent, one would consider

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the off-diagonal elements of the covariance matrices. After all, one of the attractive features of this analysis is that it allows multi-variate analysis of ACI, fully considering the meteorologic conditions - but then the plots / analysis do not reap the full benefits of this approach. The authors do explain some of the co-variabilities/co-sensitivities, but then again it would be even better to have some graphical representation for some of these connections.

p10,L28: Does the CDR - AOD relationship for the SE Atlantic region make sense? For the outflow from the Arabian peninsula and the Sahara, it does, and the manuscript explains this with dust - but on the West coast of Namibia and Angola the dust is confined to the coast. It is possible that the identified relationships here points to limitations of the data set(s) that serve as the basis. Perhaps dust is overrepresented in the data? Overall, it would be good to see a discussion in which regions we would trust the correlations (given the uncertainties in the data).

p12, L15: So, cloud radiative effect sensitivities are actually not (yet) addressed in the manuscript. Instead, cloud properties are analyzed. Earlier in the manuscript (p4,L24), it is stated that cloud radiative effects are analyzed. This should be fixed (minor comment).

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