Referee #1:

The authors have done a lot of work going through my comments, which I appreciate.

One remaining issue is reconciling point (3) on page 5 of the author response document and (4) on page 6. I agree that it is interesting to look at these relationships with an NNA, but I don't understand how the complexity of the NNA is justified in this case. It seems that the system is relatively linear based on the new version of Figure 2. As the authors point out in (4) they are clarifying previous work, which I also think is a good thing to do, but if the NNA doesn't greatly alter the model skill beyond a multiple linear regression then it is more difficult to say that this is an independent test of the original results. To be publishable the paper needs to clarify what the NNA can do that the multiple linear regression cannot. I will admit that I am not very familiar with the literature on NNA, so I appreciate the authors taking time to explain what they are doing. Is there some criterion from the literature for when an NNA is an appropriate choice? Eg. something like Bayesian information criterion that would allow an objective statement of 'NNA is better than multiple linear regression'. If some criterion for NNA use from previous investigations can be met it seems like a reasonable analysis.

In regards to point 2 in the summary on page 12. Do the regional dependencies decrease if SST is added as a predictor? Between LTS, w, RH, BLH and SST that should encompass most of the factors that constitute a regime. However, I acknowledge that different factors should drive different regimes so the point that the authors are making seems reasonable.

Referee #2

I thank the authors for their work in responding to the comments. The majority of my comments have been addressed, only two things remain. I would suggest the authors include these at their own discretion. However, some mention of them might improve the paper still further.

The authors have been clear that they would prefer not to include results from a single global ANN. While creating a single relationship for predicting oceanic CLF globally may be difficult, this is not as far-fetched as they make it sound, as this is the central aim of a cloud parametrisation. By making their ANN regionally dependent, this is similar to including the latitude and longitude in a parametrisation. I understand that it is difficult to include all of the necessary parameters in a single ANN to predict the CLF (or other parameters), but as with a cloud parametrisation, the regions or conditions where the global ANN is deficient would indicate locations for future research.

Secondly, I think that my previous point about overlying ice cloud was misunderstood. The CLF is the fraction of retrievals where clouds with liquid tops are detected, so any situation where there is overlying ice cloud has the potential to reduce the CLF without changing the properties of the underlying liquid cloud. If there are not suitable variables in the ANN to predict the ice cloud fraction, it essentially becomes random noise and would thus limit the predictive ability of the ANN. Using only single-layer cloud retrievals does not address this issue, as they provide only the fraction of retrievals where a single layer liquid water cloud is detected, a subset of CLF. The only way I can see to easily address this issue is to restrict the study to gridboxes where only liquid cloud is detected, ensuring that there is no overlying ice cloud to artificially reduce the CLF. While excluding pixels with ice clouds is not essential (as the ANN clearly already has significant predictive ability), I would recommend that the authors consider it as a method of improving the statistics generated by the ANN.