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Interactive comment on "Linking large-scale circulation patterns to low-cloud properties" *by* Timothy W. Juliano and Zachary J. Lebo

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I have reviewed the manuscript "Linking large-scale circulation patterns to low-cloud properties" by Juliano and Lebo. The manuscript describes a study to classify the meteorology in the northeastern Pacific Ocean using self-organizing maps, an unsupervised machine learning algorithm. Based on this classification, the authors infer influences of continental aerosols on marine clouds in their study region.

As the authors conclude, this study could provide valuable knowledge about ACI in this region, and it could provide a test for model representation of ACI. However, before the study can do so, I believe the authors would first need to address two major concerns:

1. Now that even the most complex (opaque?) machine learning algorithms are avail-

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able as off-the-shelf black boxes, the temptation is great to use them on any problem that comes along. To be a bit blunt, I think this work illustrates the dangers of doing so without carefully considering potential pitfalls. I am worried that the 20 meteorological regimes (really, 20? I am struggling to see the differences between many of them) are simply not robust. If the training dataset were slightly different (included an extra year at either end, an extra degree of latitude or longitude, ...), would the regimes look the same? Given that the manuscript's conclusions recommend these regimes be used for model evaluation, I think this is an important question to address; otherwise, if the models do not reproduce the regimes, we might end up falsely blaming the models for not including some non-robust idiosyncrasies of the training dataset that the machine learning happened to pick up on. In light of this (and in general), the authors' statement that unsupervised learning does not require a validation dataset is simply wrong.

2. Independently of the methods, I am suspicious of the authors' conclusions about the influence of continental aerosols. The conclusion that "we attribute the variability in the satellite-retrieved cloud microphysical and radiative properties to aerosol forcing (first order effect) as opposed to meteorological factors (second order effect)" (l. 214 ff) would raise all kinds of red flags even if it were the result of careful quantitative analysis, as I struggle to think of any situation where aerosols have a first-order effect on cloud radiative properties on a regional scale. Here, it is presented on the basis of a number of "appears to" and "does not appear to" statements that leave me unconvinced.

More detailed comments and suggestions on how to improve the robustness of both the methods and the conclusions are in the attached annotated manuscript.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2019-836/acp-2019-836-RC1-supplement.pdf

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