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Interactive Comment

Interactive comment on "Cluster analysis of midlatitude oceanic cloud regimes – Part 2: Temperature sensitivity of cloud properties" *by* N. D. Gordon and J. R. Norris

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The tropospheric mean temperature anomaly is a pressure-weighted average from 1000 mb to 200mb. I have included a line of text to clarify this.

We had tried previous iterations with different warm/cold restrictions, but our choice of the middle quartiles provided a good balance of eliminating confounding influences and still retaining enough observations to make some conclusions.

For the application of the constraints in moisture advection, we wanted the constraints to apply to the locations that clouds occurred. So, for low clouds, we wanted to make sure that the warm and cold subgroups both had similar moisture advection over the

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lowest layer that the cloud was predominantly observed in. We thus made a consistent restriction for moisture advection at all three layers the cloud observations are on. The vertical profiles of moisture advection displayed in Fig. 5 of Part I suggest a decorrelation length scale somewhat smaller than half of the troposphere, motivating the use of three rather than two layers. Regarding the exclusion of data from the stability and tropopause constraints, we don't have the exact numbers for the number of cases excluded for each reason. We did try several iterations of the temperature restriction with different selection criteria. Initially, we did not restrict LTS and tropopause height and found those factors clearly confounded the interpretation that cloud changes were solely due to temperature (e.g., LTS and tropopause change in the composites were much larger than that expected for global warming, thus raising the question of whether the observed cloud changes were due to tropopause height or LTS rather than temperature influence). Our view was to make as restrictive a selection as possible to ensure that dynamical effects were not confounding our results. There could be more tinkering with the exact controls of what points are excluded and included until interesting and statistically significant results are found. We feel that a conservative, consistent metric, even if it throws out 99% of the points, is a more appropriate method.

The reviewer is correct that lapse rate will most likely decrease in a warmer world, although we note that the change in lapse rate is much smaller at middle latitudes than in the tropics (Fig. 10.7 from the IPCC AR4 suggests that the midlatitude upper troposphere only warms about 0.5 °C more than the lower troposphere in the context of an overall 3°C change. It would require complicated additional compositing restrictions to ensure that this relatively small warm-cold lapse rate change was consistent (in a global warming sense) with the warm-cold temperature difference. We have included some additional text explaining this point.

Alpha in Eq 1 is the difference between two albedos because changes in both cloudy overcast albedo and clear sky (mostly surface) albedo can affect SWCRF (e.g., assuming no change in cloud fraction, SWCRF increases if clouds get brighter OR the

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surface gets darker). Nonetheless, clouds dominate SWCRF changes, so our alpha operates very much like albedo, and we continue to use the symbol. We choose the definition of SWCRF in equation 1 and 2 to allow us to look at changes separately in albedo and cloud fraction. We think the derivation of the equations is sufficiently simple that additional explanation is not worthwhile. The lack of correlation between changes in cloud fraction and cloud albedo was not expected, but it does aid in the interpretation of the results.

The reason the climate change analogy breaks down is that daily temperature variability is much greater for the troposphere than for the ocean surface (now explicitly mentioned in the text), unlike the case for climate change time scales. A different set of criteria would be to composite according to sea surface temperature rather than ocean temperature. This would cause the loss of many more data points due to requiring the tropospheric temperature to be commensurate with the surface temperature, and the smaller range of temperature variability would result in a similarly smaller range of cloud variability, thus resulting in less statistical significance. It is our opinion that it is more important to ensure that dynamical influences are not confounding the results than to increase the statistical significance of our results. In fact, the lack of statistical significance is also an important result in that it (through the uncertainty range) provides an upper limit on how much temperature influences cloud properties.

You are correct in our definition of greenhouse parameter, but it is not identical to that used by Cess and Udelhofen (2003). It can be obtained by rearranging Eq. 6. You are correct in your interpretation of our application of greenhouse parameter to clouds with non-unity emissivity. We have rewritten the text.

Since we cannot use the direct difference between LW fluxes, as we did for the SW fluxes, we can't verify that the changes in flux from the three components are uncorrelated. The warm-cold differences between cloud fraction, cloud-top pressure and optical thickness, however, are not well correlated (< 0.4).

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The net radiative cooling for the low-level cloud regimes results from a reduction in greenhouse effect due to lower mean cloud top height and les cloud cover that is larger than the reduction in solar reflection due to less cloud cover. We have added this to the text.

There is a utility in applying the same methodology to tropical clouds, but it is important to do clustering independently for the tropics, as was done by Williams and Tselioudis (2007), since clouds are driven by different dynamical processes. The weakness of applying our approach to the tropics is that vertical motion produced by reanalyses for the tropics is less certain because it is less closely related to large-scale temperature and wind and more closely related to poorly observed diabatic processes like precipitation. The possibility of cloud feedbacks related to changed dynamics was already mentioned in the final paragraph of the paper, and we have now added more discussion and the specific example of shifts in storm tracks.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 1595, 2010.

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