Atmos. Chem. Phys. Discuss., 10, C186–C191, 2010
www.atmos-chem-phys-discuss.net/10/C186/2010/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

Received and published: 19 February 2010

Cluster analysis of midlatitude oceanic cloud regimes – Part 2: Temperature sensitivity of cloud properties

General Comments

The authors present an analysis of the sensitivity of midlatitude oceanic clouds to changes in temperature. The basis of the analysis is a clustering method to separate cloud regimes based on ISCCP CTP-tau histograms, and then dividing those regimes into "cold" and "warm" subsets following a set of criteria to try to keep dynamical effects out of the analysis. This analysis is a good contribution to understanding the response of clouds to temperature change. There are limits, some of which the authors note, but

this work represents a promising path toward better understanding this aspect of cloud response. One suggestion to improve this paper is to rely more heavily on the companion paper (Part 1) to deal with the data and methods and significantly cut down the introductory, data, and methods material here to focus more deeply on this analysis. This would allow more care to be given to some of the details (see below) and improve the overall clarity, which is the main weakness of the paper.

Specific Comments

This paper really starts to get separated from Part 1 in Section 3. This is where the relevant methods for this analysis are presented. One suggestion is to make the overlap between Part 1 and Part 2 minimal, and expand the details that are more relevant here. Some aspects of the methods in Section 3 that could be elaborated are as follows: (1) how was the tropospheric mean temperature determined? Did it rely on a determination of the tropopause height, or were a set number of levels used, and if so which levels? (2) How was the choice to use the 2nd and 3rd quartiles decided? Is there enough variability in the clusters to automatically exclude 50% of the data? (3) How much does applying the T and q advection constraints in three layers differ from using two? This is basically a question about how much correlation there is in the T and q advective tendencies through the column. (4) How much more data is lost with the LTS and tropopause constraints? (5) Have the authors tried applying different sets/subsets of these constraints to determine the minimal number of decisions needed to reduce confounding dynamical effects? Is such a minimization possible? It is appreciated that this data reduction is intended to be conservative, so there is little chance of including strong differences in weather conditions rather than similar conditions that happen to be warmer/colder than median, but since this reduces the size of the dataset to less than 1% of it's original size, it might be worth exploring which constraint is most important (and which might not be necessary). This also seems to have bearing on the later results that are not statistically significant.

The main result seems to be that holding the dynamics nearly constant and increasing temperature reduces cloud amount, pushes low clouds lower and makes them optically thicker, and lets high clouds be higher (cirrus). There are not statistically significant changes in the "frontal" clouds, except a slight increase in optical thickness in the weak frontal cluster. The changes among the clouds are mostly presented as differences in the warm versus cold distributions, which makes sense, but doesn't say much about whether those differences have anything to do with the physics. One might ask, for example, whether it makes any sense that the rising cirrus cluster is rising because of a decrease in low level clouds? Or whether the shift in the cumulus clusters toward more optically thick cloud in warmer environments is consistent with our understanding of cumulus clouds?

It is noted on page 1605 that the temperature anomaly profiles have about a constant difference between them through the troposphere, so differences in clouds are associated with temperature changes and not lapse rate changes. This raises the question of whether this is the desired analogy to a climate change? Isn't there an expected lapse rate response to warming? The rationale for using the tropospheric temperature was that it better represents the variability of the environments in which clouds exist. Does a uniform warming of the tropospheric temperature capture the thermodynamic environment of clouds over the midlatitude oceans of a warmer Earth? Or is this lapse rate change somehow implicit in the clusters? Perhaps the authors could elaborate on this point to clarify the analogy to anthropogenic climate change.

Breaking the SW cloud radiative forcing change into the cloud fraction and albedo contributions is a nice inclusion. It would help the reader to be more clear why alpha in Eq 1 is not an albedo but a difference between albedos; maybe even use a different symbol. Equation 1 generally is a little tricky, since people tend to think only in terms of the definition, SW_up_clear minus SW_up_allsky. The authors note the lack of correlation between cloud fraction and albedo; is this expected? The significance test of the results is much appreciated, even if the values in Table 2 are mostly insignificant. It's nice to see that the difference in the most common cluster, "small cumulus," is

significant and due to the reduction in cloud fraction while the "Deep As" cooling effect is all due to brighter clouds. These conclusions (and those drawn from the less significant numbers) make sense alongside Table 1, since cloud fraction in cluster 1 decrease but the clouds get brighter while the cloud fraction doesn't really change in cluster 4 while the optical thickness does.

The discussion of the longwave effects is less clear. The authors note that the analogy to climate change breaks down because of the choice to divide warm and cold data based on tropospheric temperature. This brings up, again, the question of whether it would be possible to use a different set of criteria to divide the data, but now with two goals: (1) to increase the statistical significance by retaining more data, and (2) to better capture behavior that is expected in a warming scenario. It might not be possible, but more discussion is warranted. The "above-cloud greenhouse parameter" is not well explained. The authors should consider including the expression for it; is it just g = 1 - OLR / \epsilon \sigma Tˆ4_{CT} (analogous to Cess & Udelhofen)? The application of the parameter to the clusters with non-unity emissivity is also not clear. After some time, I think I understand: by taking the clusters with emissivity near unity, it provides an estimate of the LW emission just from cloud-top, and luckily this emission has a nearly linear relationship with cloud-top pressure, providing an estimate of below-cloud LW flux in the other clusters. This should be explained more simply. On page 1610, the authors assume that CF, emissivity, and CTP are uncorrelated within clusters; this should be shown and not just assumed. The LW cooling associated with the (low) cloud clearing is a point that deserves some attention. The usual low-cloud effect goes "less cloud leads to more SW reaching the surface, meaning more warming." In this analysis, the LW cooling is larger than the SW warming in clusters 1 and 3, but this doesn't seem to make sense in an energy budget sense. Is there an interpretation for this, or is it just a non-equilibrium response? Either way, some discussion is needed.

There are a couple of points that could be better addressed, perhaps in the discussion section. First, the analogy to anthropogenic climate change isn't perfect, and this should be more clearly discussed. There are (at least) two parts to this. First is that the strong constraint on the temperature through the troposphere means that the warm subset temperature doesn't have the structure expected from global warming (cf. IPCC AR5, Figure 10.7). Second, as the authors mention, the LW effect is not analogous to the kind of LW change in global warming. Next, and also addressed by the authors, is that the effects shown here are partial derivatives with respect to temperature. The discussion points out that these effects may not be representative of the cloud feedback in midlatitudes because of changes in dynamics. A point that I wondered throughout reading this paper is whether this analysis would be better suited to the tropics, where the dynamical effects might be expected to be weaker. Several of the clusters used here are essentially forced clouds that are totally dependent on dynamics to exist, so any dynamical shifts (poleward migration of the jetstream, e.g.) will be crucial to their climate response. Perhaps a counter to that is in Table 1, where change associated with the less forced cloud types (i.e., Small Cu, Large Cu, Sc/St, and maybe cirrus) seem more statistically significant than the strongly forced clouds of frontal systems. In any case, the paper would benefit from a little more discussion of these aspects of the analysis, not because they are deficiencies or caveats, but because it would allow the authors to make some statements about the context and relevance of the present work.

Technical Corrections

1. Section 4, 2nd paragraph: delete 'and' in "warm minus and cold"

2. Section 4, 2nd paragraph: "N warm +N cold" seems a little sloppy, and could be worded better.

3. Section 4, 2nd paragraph: the sentence "This procedure was repeated..." needs to be reworded to be clear. Consider replacing 'how likely' with 'the likelihood that'

4. Equations: I suggest adding a clarification somewhere to emphasize that the subscript 'overcast' is not the same as 'all sky.'

5. Section 6, 2nd paragraph: in 'as a result in the' replace with 'as a result of the'

6. Section 6, 2nd paragraph: in the next line, insert altitude or levels after the word 'high'

7. Section 6, 3rd paragraph: I wonder if the "presence of dynamics" should be changed to something like "the potential for dynamical changes"?

8. Section 7, 1st sentence: "unique suite of dynamics" is not a very descriptive phrase, and could be replaced by something like "distinct dynamical properties."

9. Figures: The various labels (axis, titles, etc) are a bit small and hard to read.

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