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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the relationship between low cloud variability and lower tropospheric stability in the Southeast Pacific" *by* F. Sun et al.

Anonymous Referee #1

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Review of "On the relationship between low cloud variability and lower tropospheric stability in the Southeast Pacific" by F. Sun, A. Hall and X. Qu, ACP-11-3777-2011.

The authors study how low-cloud fraction in the Southeast Pacific relates to lower tropospheric stability on the daily, seasonal and interannual time-scale. From all time-scales, it is found that the relationship is strongest when the stability is less than ~20K but weakens when it is above this value. This appears to be generally true – but I will note that the weakest relationships occur in JJA, and not the season of maximum LTS which is SON – so this simple observation cannot be the whole story. This result is interesting and with the work generally well done, the paper deserves publication,

subject to the comments below.

I have five major comments.

1. I think it is important to at least postulate a physical reason why the relationship weakens when LTS > 20K. None is given. The simplest potential explanation is that cloud fraction won't be sensitive to LTS when the cloud fraction reaches unity. Although this may seem to be happening here, it is worthy of mention and further investigation. Even if you have to speculate, what might be the reason for this behavior?

2. In my mind, the Estimated Inversion Strength (Wood & Bretherton) is an equally valid measure and I strongly ask that you repeat the analysis using this measure. Do you find similar results, particularly in the sense that there is a value of EIS above which the sensitivity to EIS is reduced? Also in the conclusions, you must discuss the climate change implications if low cloud amount is predicted with EIS. The very important result is that you get two different predictions for the climate change response of low clouds depending on whether you base your prediction of EIS or LTS – two measures that appear to equally well explain current climate variability in low clouds.

3. I think that your box for which you do calculations is too big. In particular, I can imagine somewhat different behaviors for the region to the west of 90W (a transition region) as compared to the region east of 90W (a solid stratocu region). I think you may want to repeat the analysis separately for each region. I expect that you may find that the limited sensitivity to LTS may be most obvious in the eastern half and that the explanation may be that that is when overcast conditions occur. Another possibility (which is found in other stratocumulus regions) is that occasional offshore flow brings very warm and dry air over the near coastal ocean leading to very high LTS but too dry conditions for a cloud to form.

4. I think it is also essential that you mention in the paper that other stratocumulus regions may behave differently. Obviously, they should be studied too although probably you'll conclude that this should be done in a future study.

5. Finally I have some concern about how you use the ISCCP data. It is well known that ISCCP has trouble locating cloud-top pressure under inversions. Most often the problem results in a cloud-top pressure too low and in the terminology of ISCCP bins, clouds that should be in the lowest bin (800-1000 hPa) end up in the next bin up (680-800 hPa). That next bin up is still a low cloud bin so you might be ok in a lot of circumstances. However, when the inversion (i.e. LTS) is strongest, the error sometimes results in ISCCP erroneously placing a low cloud into the 560-680 hPa bin and thus mistakenly calling a low cloud a middle level cloud when it should be a low cloud. Thus most ISCCP middle level clouds in the stratocumulus regions are really low clouds. See the article by Garay et al. 2008 for some information about this – but there are other references. Also see the map of ISCCP middle level cloud for SON (available from the data browser at isccp.giss.nasa.gov) – the detached maximum in the Southeast pacific is clearly low cloud – and not frontal cloud systems (if it were, it should be connected to the southern ocean middle level cloud peak).

Thus I think you should be using L" = (L+M)/(1-H) and not L'=L/(1-M-H). A possible refinement would be to just use the middle level cloudiness between 560-680 hPa but not the 440-560 hPa bin.

This error I think may cause you to reach incorrect conclusions. For example, the seasonal cycle of clouds peaks in SON (which your data shows is true if you consider L"). Also the change in the slope of the cloud vs. LTS line around LTS of 20K may partly be the result of the fact that ISCCP creates these erroneous middle level clouds only when the LTS is very high. Finally the poor interannual correlations may also be impacted by not including what ISCCP calls middle level cloud into your low cloud diagnostic.

Other comments

1. I prefer that you change the word “disintegrates” to “weakens” or “substantially weakens”. Disintegrates implies no relationship or a relationship opposite to that found

before. In general, it appears that the relationship weakens but positive (albeit not significant) relationships are present. Longer data records might render significant these weaker correlations.

2. Page 3779 main paragraph. You should mention that a poor stratocumulus simulation in a coupled ocean-atmosphere model may also be due to the inability of low horizontal resolution climate models to well simulate the intense coastal jets that drive the upwelling to keep the ocean cold.

3. On the datasets, why do you stop with ISCCP data in 2002? It is worth extending your dataset to 2008. You might choose to get ERA Interim to cover the later period. Also, can you confirm that you're using the low clouds from the VIS+IR analysis and not the IR-only? Also, you might repeat the analysis using subset of low clouds that are called stratocumulus (stratocumulus+stratus), instead of all low clouds. Is there anything interesting that results from looking at this subset?

4. Page 3783. How was the data detrended? That is, the variability with what periods (> 10 days, > 30 days, > 90 days, etc.) were removed. Also how does the detrending impact the daily analysis? It looks like your plotting the full values of cloud amount, not anomalies, so I am not clear what data is correlated for the daily time scale.

5. Page 3783. I hope that your box does not include land – the figure makes me think so.

6. ENSO Modulation section: Are differences in the slopes for cloud amount on LTS between years statistically significantly different? Please use statistical analysis theory to demonstrate this.

7. Also, I think you'll want to mention that the El Nino connection is not always the case. There is clearly at least one El Nino year with low LTS and reduced cloud amount. Have you examined that year to see what is happening?

8. Page 3792. The lack of correlation between upper air and near surface air tempera-

ture was also found by Klein et al. (1995) or Klein (1997) for the location of weathership N in the Northeast Pacific.

9. Could someone say that there is a non-linear relationship between LTS and cloud amount, one that might be described by a smooth curve that is more linear for LTS < 20K, but flattens out for LTS > 20, perhaps like a 1 – exp (- a LTS) curve?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 3777, 2011.