Reponses to review #1 (anonymous):

General comments:

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 timescales, 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.

[reply]: First of all, we would like to acknowledge the reviewer for the work and insightful and valuable comments that have improved the quality of our original manuscript. As indicated below, we have checked all the comments provided by the reviewer and all of them have been revised accordingly to the reviewer's suggestions. Please see below our detailed responses.

Specific 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?

[reply]: Suggestion taken. In the discussion section, we've added some potential explanations to explain the substantially weakened linear relationship between low cloud amount and LTS when LTS reaches high values. Besides the one that the reviewer suggested, we proposed the other one to be related to the landmass impacts. We also included discussions of previous studies, e.g., Zhang et al. (2009), who used mixed-layer model simulations to show that the large-scale divergence is a constraint for the low cloud and LTS relationship.

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.

[reply]: We've calculated the EIS using the equations in Wood and Bretherton (2006) and repeated all the analysis in our manuscript replacing LTS with EIS and we found qualitatively similar results. Here we present Figure 1A as one example, the analogue of Figure 5 in our original manuscript, but for EIS. It clearly shows that the relationship between EIS and low clouds is significant during DJF for the whole region while it is substantially weaker in JJA. This result is very consistent with the LTS-low cloud relationship that we presented in our manuscript. Meanwhile, we've added discussions of the EIS-low cloud relationship in the revised manuscript.

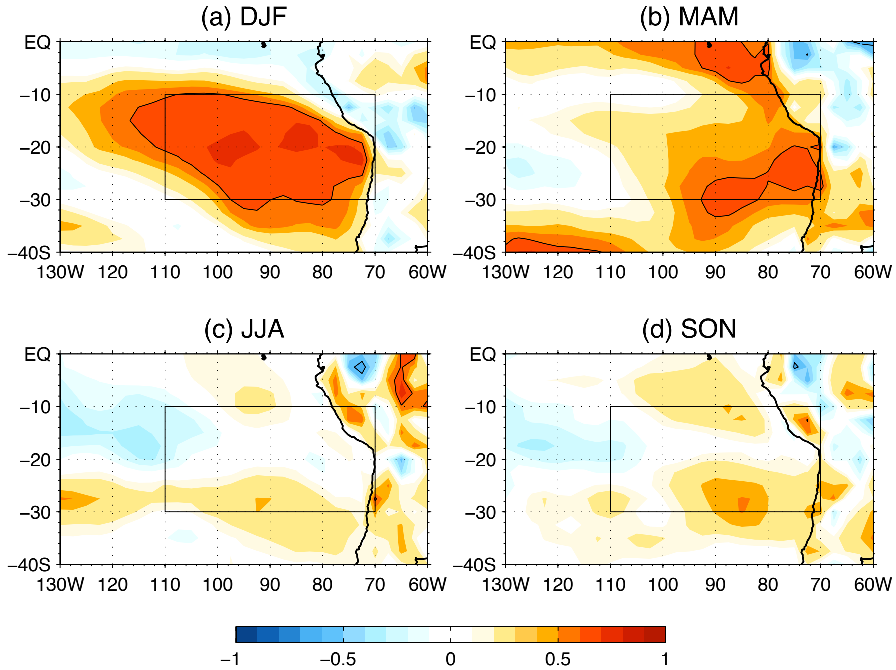

Figure 1A: Correlation of seasonal mean, area-averaged adjusted low cloud amount with EIS at each grid point for each season.

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.

[reply]: Thanks to the reviewer for bringing this to our attention. This is an excellent suggestion. We've chosen different boxes to do the area average and tested the sensitivity of our results to different chosen domains. In sum, we found the quantitative values of the slopes between area-averaged low cloud and LTS is area-dependent, but the qualitative results are the same. And that does not affect our main conclusions. Table A1 presents the slopes of linear regression between LTS and area-averaged low clouds on interannual timescales grouped by different seasons. We show three regions: 1) region (110°W-90°W, 10°S-30°S), the western half of the region in our manuscript; 2) region (90°W-70°W, 10°S-30°S), the eastern half of the region and 3) region (90°W-80°W, 10S°-20°S) used by Klein and Hartmann (1993). All the three domains present the linear regression slopes show the largest values in DJF. In JJA, the solid stratocumulus region shows very weak (even slightly negative) sensitivity of low cloud to LTS while the transition region shows somewhat weakened sensitivity to LTS. The two regions behave differently and the solid stratocumulus region accounts largely for the limited sensitivity of low cloud to LTS within the large box in our manuscript. We also used the domain, which Klein and Hartmann (1993) used to study the seasonal cycle of Southeast Pacific low cloud regime. Interestingly, we found the limited sensitivity in JJA also apparent in SON. The reviewer's comment has been very helpful and informative and we've included this discussion in the revised manuscript.

Table A1: Slopes of linear regression between area-averaged adjusted low cloud amount and LTS over three different regions in the Southeast Pacific on interannual timescales grouped by seasons.

Slope (% per K)	(110-90W, 10-30S)	(90-70W, 10-30S)	(90-80W, 10-20S)
DJF	3.60	3.76	5.57
MAM	2.77	2.73	3.03
JJA	2.02	-0.31	0.26
SON	2.42	2.45	0.34

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.

[reply]: Suggestion taken.

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.

[reply]: We thank the reviewer for this very helpful suggestion. We've followed the reviewer's suggestion to calculate the adjusted low cloud in ISCCP as L''=(L+M)/(1-H), and repeated all the analysis, including all the tables and figures and made corresponding changes in the revised manuscripts. In sum, the new adjusted low cloud calculation does not qualitatively affect our results and our interpretations in the original manuscript, taking into account that the observed "low" cloud amount in ISCCP is much larger than the "middle" and "high" cloud amount. This could be supported by the similarities between all the tables and figures in the original and revised manuscripts. However, we do found some small quantitative difference and revised the manuscript accordingly. We list the corresponding changes:

1) On seasonal cycle, by considering "most ISCCP middle level clouds in the stratocumulus regions are really low clouds", SON stands out as the peak season, which has slightly more low clouds than in JJA. This is shown in Figure 2 of the revised manuscript.

2) "the poor interannual correlations may also be impacted by not including what ISCCP calls

middle level cloud into your low cloud diagnostic" has been verified using the new adjusted low cloud calculation. Comparing Table 1 in our new manuscript to the one in the original one, the correlation coefficients increase for all seasons on interannual timescales. For example, the coefficient for DJF increases from 0.72 to 0.85 while for JJA, the correlation remains poor, which is consistent with our seasonally-dependent correlation argument.
3) As to the reviewer's remark "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", by using L", we found the same conclusion as shown in our original manuscript. However, the slope does increase a bit (changing from 0.68% to 1.63% per K) and so the correlation coefficient (r changing from 0.16 to 0.27) for the points to the right of LTS climatology. But compared to the counterpart to the left, this value is still relatively small. This possibly suggests that the misclassification of low to middle cloud of ISCCP data might be one of the causes for the change of the slope when LTS is very high, which is shown in our manuscript, but possibly not the only reason.
4) Usage of L'' does impact the interannual correlations. And we found a better relationship in LTS and low clouds on interannual timescales for all seasons. This is shown in Table 1 of the revised manuscript.

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.

[reply]: Suggestion taken.

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.

[reply]: Suggestion taken. We've added this point in the revised manuscript.

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?

[reply]: Following the reviewer's suggestion, we extended the dataset to 2008 using ERA-Interim to cover the later period, i.e., ERA-40 (1983/12~2002/08)+ERA-Interim (2002/09~2007/11). An alternative extension to fully use the ISCCP data is to use the ERA-Interim as main body but ERA-40 to cover the starting period, i.e., ERA40 (1983/12~1988/12)+ERA-Interim (1989/01~2007/11). The linear regression slopes on interannual timescales grouped by seasons using both calculations are shown in Table A2. Both calculations gave qualitatively similar results to Table 1 in our manuscript: stronger sensitivity to LTS in DJF while weaker in JJA, especially for the latter. We then verified the LTS values calculated from ERA-40 and ERA-Interim for the common period, i.e., 1989/01-2002/08 are highly correlated. But LTS calculated from ERA-Interim tends to be larger than LTS calculated from ERA-40 systematically. This is shown in Figure A2. To avoid data inconsistency, we believe it might be ideal to use one single

data set. We used low clouds from the VIS+IR bands. By using the strati-form clouds, we found similar results.

Table A1: Slopes of linear regression and correlation coefficients between area-averaged (70°W-110°W, 10°S-30°S) adjusted low cloud amount and LTS on interannual timescales grouped by seasons using the combination of ERA-Interim and ERA-40 data.

Slope (corr)	ERA40(1983/12~2002/08)+ ERAinterim(2002/09~2007/11)	ERA40(1983/12~1988/12)+ ERAinterim(1989/01~2007/11)
DJF	3.96 (0.85)	4.71 (0.84)
MAM	2.75 (0.61)	2.07 (0.43)
JJA	0.86 (0.32)	0.10 (0.04)
SON	2.86 (0.67)	2.59 (0.60)

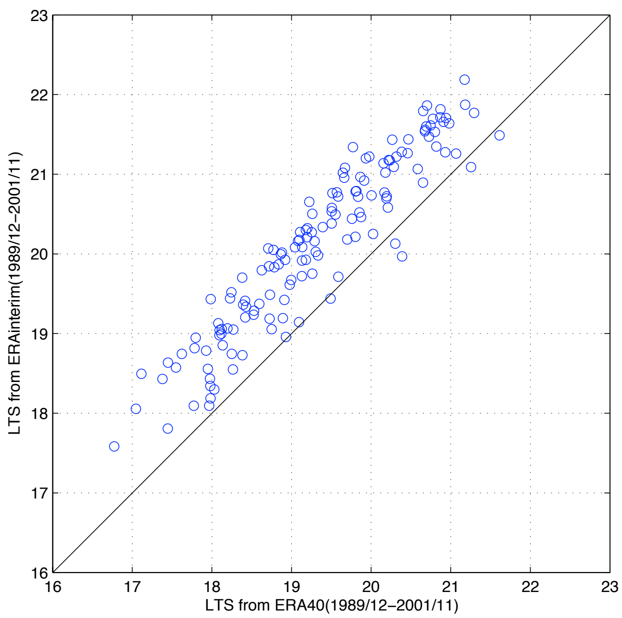

Figure A2: Scatterplot of LTS calculated from ERA-Interim against LTS calculated from ERA-40 for the common periods from 1989/12 to 2001/11.

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.

[reply]: For the correlation analysis on the daily timescales, we used the detrended data within each season. For example, we removed the trend within 90 days for DJF, MAM, JJA and SON separately. The detrending does impact the daily analysis but it is very minimal. However, for the scatterplots we use the full value data without detrending.

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

[reply]: The box does not include land grids. To clarify this, we've noted in the text the grids in

the box as the oceanic grids within the box.

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.

[reply]: We've used the statistical analysis theory in Fox (1997) and von Storch and Zwiers (1999) by first making a dummy variable and using a two-sided Student-t test to test the null hypothesis Ho: $Slope_{El\ Nino} = Slope_{La\ Nina}$, which are the low cloud-LTS slopes of the El Nino year and the La Nina year. We found the slope for the El Nino year is not significantly different from the slope for the La Nina year at the 95% confidence level. But the slope is significantly different at the 90% confidence level.

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?

[reply]: Suggestion taken. We've examined the vertical thermal pattern and low cloud amount for that El Nino event and we found in that year both the upper air and near surface are warmed up and that leads to relative low LTS values.

8. Page 3792. The lack of correlation between upper air and near surface air temperature was also found by Klein et al. (1995) or Klein (1997) for the location of weathership N in the Northeast Pacific.

[reply]: We've added the discussion into the revised manuscript.

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?

[reply]: Visually there is a linear relationship between low cloud amount and LTS for lower LTS values while a weaker relationship for higher LTS values. To further demonstrate this, we believe more samples numbers and model studies are needed.

Reference:

Fox, J., 1997: Applied Regression analysis, linear models, and related methods. Thousand Oaks, CA: Sage. ISBN: 080394540X.

Klein, S. A. and D. L. Hartmann, 1993: The seasonal cycle of low stratiform clouds. Journal of Climate, 6 (8), 1587–1606.

Wood, R. and C. S. Bretherton, 2006: On the relationship between stratiform low cloud cover and lower-tropospheric stability. Journal of Climate, 19 (24), 6425–6432.

von Storch, H. and F.W. Zwiers, 1999: Statistical analysis in climate research. Cambridge University Press, 484 pp, ISBN 0521 450713.

Zhang, Y., B. Stevens, B. Medeiros, and M. Ghil, 2009: Low-cloud fraction, lower-tropospheric stability, and large-scale divergence. Journal of Climate, 22 (18), 4827–4844.