Reponses to review #2 (anonymous):

This manuscript examines the correlation between lower tropospheric thermodynamic stability and the amount of low cloudiness in the southeast Pacific. It's been known for about 20 years that increased stability tends to increase cloudiness on seasonal time scales; the relationship is less robust on shorter time scales. This manuscript examines the correlations between the two at daily, seasonal, and inter-annual time scales.
The work appears technically sound so, on the one hand, there's no reason not to publish this paper. But the work, as it stands, is undigested - the results of a great many calculations without an explanation for why these calculations are relevant or what the results mean. One might therefore be hard-pressed to argue that it's important or relevant enough to be worth publishing. I expect that this shortcoming can be addressed with changes to the writing alone, but such changes will help ensure that the paper is worth both the authors' and their readers' time.
What I want from a scientific paper is context, an interesting question, a plausible response to that question, and some sense as to what the answer means or implies. This paper reads as if the calculations themselves were the point, and that makes the paper hard to engage with. If there is a scientific question or a hypothesis here it is not articulated clearly. Correlations are not, in an(y) of themselves, particularly interesting, and comparison with previous calculations is important only in so far as one uncovers deeper understanding.

[reply]: First we would like to thank the reviewer for the very constructive comments and suggestions. The time spent in critiquing the manuscript is appreciated. We believe that addressing the reviewer's concerns has led to improvements in the revised manuscript. In the revised manuscript, we articulated clearly our hypotheses in the introduction section. Our hypotheses include the seasonal and timescale dependence of the LTS-low cloud relationship. We found the LTS-low cloud relationship to be linear when LTS is relatively low while nonlinear when LTS reaches relatively high values. We then provided potential explanations in the discussion section based on our analysis and previous modeling studies and we pointed out the implications for future climate predictions related to low cloud feedback, especially for those models using LTS and observed LTS-low cloud relationship as a predictor for low cloud. Please find our detailed responses.

General comments:
The introduction is more general and longer than is appropriate for a journal paper. One useful test may be for the first author to ask themselves if, having now read much of the relevant literature, how much of the introduction they would read in a paper they picked up.

[reply]: Suggestions taken. We've condensed the introduction section and made it concise.

Many figures are nearly illegible. The type is small - 9 point type is about the limit for readers over 40. There's a lot of wasted space as well - the panels in figure 3, 5, 7, 9, 10, and 11 all share axes, which only need labeling once. Removing extra labels would let the authors increase the size of the active part of the figure.

[reply]: We thank the reviewer for the constructive comments. We prepared our manuscript using letter size pages but ACP rendered the figures with captions in smaller-size pages. In the revised manuscript, we have attempted to increase the font size in figures where this may pose a problem with readability.

Technical points:

Why do the authors choose to use ERA-40 instead of the more modern ERA-Interim reanalysis? The assimilating model is demonstrably better in the latter, and one might expect more accurate estimates of 700 hPa temperature.

[reply]: Following the reviewer's suggestion, we've repeated our analysis by using ERA-Interim data. In terms of time span, both ERA-40 (1957/09-2002/08) and ERA-interim (1989/01-2011/03) data only partly overlap with the available ISCCP (1983/07-2008/06) cloud data. For both reanalysis data, only part of the ISCCP data could be used for the analysis. Using the ERA-Interim data does not affect the qualitative results of this study while it shows quantitatively different results. Example results are shown and compared with the results in our original manuscript in Table A1. For example, as shown in Table A1, LTS and low clouds are highly linear correlated in DJF while in JJA this linear relationship is substantially weak for ERA-Interim data. This is consistent with the results using ERA-40 data though different period of data are used while there are quantitatively differences in the magnitudes of regression slopes and correlation coefficients. ERA-Interim generally presents larger sensitivity of low cloud amount to LTS changes with stronger correlation than the ERA-40 for all seasons. We've also analyzed the NCEP-NCAR reanalysis data and found the results are in excellent agreement. We've addressed this issue in the revised manuscript.

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 ERA-Interim and ERA-40 data (shown in our original manuscript).

Slope (% per K); (corr)	DJF	MAM	JJA	SON
ERAinterim (1989/12~2007/11)	5.07 (0.81)	3.92 (0.77)	0.60 (0.15)	3.88 (0.52)
ERA40 (1983/12~2001/11)	3.72 (0.72)	2.75 (0.54)	0.29 (0.06)	2.54 (0.28)

It has been understood since at least Wayne Schubert's 1979 papers on mixed-layer model that boundary layer clouds are not in equilibrium with their local environment, so not acknowledging this on page 3783, line 26 seems disingenuous.

[reply]: We've acknowledged Schubert et al. (1979) in the revised manuscript.

The authors may want to at least acknowledge that much of what ISCCP reports as mid-level cloud is in fact thin, high clouds over low clouds, at least in some regimes (e.g. doi:10.1029/2005JD005921).

[reply]: We carefully studied the reference Mace et al. (2006). Their Figure 16 does show the ISCCP data bias cloud tops into the middle troposphere as the reviewer pointed out. However, the site in this study is over the land at the Southern Great Plains. Our region of interest is the Southeast Pacific, one of the most dominant stratocumulus decks, which is a different regime from the site studied in Mace et al. (2006). There have been studies, e.g., Minnis et al., 1992; Rozendaal et al. 1995; Garay et al. 2008; Ghate et al., 2009 to show that for the marine low cloud regime, the ISCCP tends to misclassify low clouds into middle and high clouds. Nevertheless, in the revised manuscript, we've acknowledged Mace et al. (2006) and indicated this bias of ISCCP data, which is especially true for certain regions.

The form of Figure 2 is needlessly confusing. Why are any of the data shown as bars? I suggest line plots here to stress that these are all cloud amounts. Plotting ISCCP low, low + mid, and total is one possibility.

[reply]: We used the stacked bars with different colors to represent the ISCCP observed low, middle and high cloud to show that the region is predominated by low clouds though there is misclassification of certain type of cloud into the other type. All the bars share the same axis on the left side with the adjusted low cloud. In order to avoid confusion, we used different colored axis on the right side to show the seasonal cycle of LTS. We thought the figure is clearly explained in our manuscript but if the reviewer find we need to clarify more, please let us know.

The division of the observations in Figure 4 at an LTS of 19.5 K seems arbitrary. Can it be justified more rigorously? What does it mean that the two regression lines are discontinuous?

[reply]: The LTS of 19.5 K is the long-term climatology. It might be arbitrary shown in the figure. We simply use this value to shown when LTS values are relatively small there is a strong linear relationship between LTS and low clouds while when LTS values are relatively big the linear relationship becomes weaker. The two regression lines are not necessarily to be continuous.

Figure 6 is right at the limits of plausibility. It's true that the correlation coefficient is technically significant at some levels, but only just.

[reply]: This might be true but we've used the two-sided Student-t test to do the significance test. We thought part of reason is related to the coarser vertical resolution of reanalysis data and a larger number of sample size will help.

Reference:

Garay, M. J., S. P. de Szoeke, and C. M. Moroney, 2008: Comparison of marine stratocumulus cloud top heights in the southeastern pacific retrieved from satellites with coincident ship-based observations. J. Geophys. Res., 113 (D18).

Ghate, V. P., B. A. Albrecht, C. W. Fairall, and R. A. Weller, 2009: Climatology of surface meteorology, surface fluxes, cloud fraction, and radiative forcing over the southeast pacific from buoy observations. Journal of Climate, 22 (20), 5527–5540.

Mace, G. G., et al., 2006: Cloud radiative forcing at the atmospheric radiation measurement program climate research facility: 1. technique, validation, and comparison to satellite-derived diagnostic quantities. J. Geophys. Res., 111 (D11).

Minnis, P., P. W. Heck, D. F. Young, C. W. Fairall, and J. B. Snider, 1992: Stratocumulus cloud properties derived from simultaneous satellite and island-based instrumentation during fire. Journal of Applied Meteorology, 31 (4), 317–339.

Rozendaal, M. A., C. B. Leovy, and S. A. Klein, 1995: An observational study of diurnal variations of marine stratiform cloud. Journal of Climate, 8 (7), 1795–1809.

Schubert, W. H., J. S. Wakefield, E. J. Steiner, and S. K. Cox, 1979b: Marine stratocumulus convection. part ii: Horizontally inhomogeneous solutions. Journal of the Atmospheric Sciences, 36 (7), 1308–1324.