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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 #2

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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 of themselves, particularly interesting, and comparison with previous calculations is important only in so far as one uncovers deeper understanding.

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.

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.

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.

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

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not acknowledging this on page 3783, line 26 seems disingenuous.

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).

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.

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?

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.

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