Atmos. Chem. Phys. Discuss., 9, C10258–C10267, 2010
www.atmos-chem-phys-discuss.net/9/C10258/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Subseasonal variability of low cloud radiative properties over the southeast Pacific Ocean" *by* R. C. George and R. Wood

R. George

rheag@atmos.washington.edu

Received and published: 28 January 2010

We thank the referee #1 for the incredibly constructive and thorough review. Their comments and corrections will greatly improve this paper.

Response to general comment:

This is a valid point, though there are several reasons why we think we can trust the results using satellite data:

– The region we most care about understanding is east of 90W and north of 30S– where the stratocumulus deck is, where cloud cover tends to be less broken on the

sub 1-degree scale and more persistent (of course exceptions occur). In our figures we show a larger domain, including regions of less consistent cloud cover where more serious questions of retrieval accuracy occur. We do this primarily so that we can look at the large scale patterns/changes upwind of and preceding changes in the main region of interest.

– One of our hypotheses does rely on patterns noted south of 30S, however. We've included a figure- a histogram of low cloud fraction (MODIS data) for data within 33-40 S and 79-86W, where in the mean field (Fig. 1 of paper) we see an average cloud coverage of near 0.42-0.46. While the retrieval of broken cloud is certainly not negligible, the attached figure shows that the mean cloud cover values result as an average mostly over days of either no cloud or cloud cover near unity. While there are days with broken cloud on the sub 1-degree scale, it is common, even in this region outside of the main stratocumulus deck for there to be no cloud or 100% cloud cover in the given pixels. Only 37% of the CF data in this region is in the middle 60% of possible values– between 0.2 and 0.8 (for a box of equivalent size in the stratocumulus deck, 19-26S, 73-80W, 24% of the CF data is between 0.2 and 0.8). Only about 53% of the CF data is in the middle 80% of possible values (between 0.1 and 0.9). This means there is less broken cloud situations than one might think given a mean value of approximately 0.45. The histogram looks similar no matter the size or location of the domain in the regions with the lowest mean cloud cover in the domain of study. This increases our confidence in the results.

– We also applied sensitivity tests to the results, restricting the data used (Tau, Re, CF, etc.) to days with low cloud fraction larger than 0.9,0.7, etc. While the quantitative results change slightly, the qualitative results (i.e, patterns of composites on SLP PC1 and PC2) evidence the same processes since the patterns look very similar. This gives us confidence that our hypotheses are based on trustworthy retrievals rather than being an artifact of poor satellite retrievals. One should still be careful of course about believing cloud retrievals in regions of broken cloud, but in the mean, or averaged over enough samples in a composite plot, we are confident that the histogram allows enough accuracy to at least form a qualitative picture.

We did not specifically explain this reasoning in the paper, but did mention that concerns about retrieval accuracy have been a barrier to diagnostic studies of cloud variability in the SEP (page 25278, line 8). The underestimate of fc shouldn't impact our general results severely, as we are investigating differences rather than absolute values. We attempt to address these issues briefly in the revised version. "Although in regions of shallow cumulus MODIS data may underestimate fc, this shouldn't change the conclusions we will make about the system." was added to discussion of computation of albedo proxy. We haven't mentioned the surface contamination problem of cloud top temperature, because as described above, fc is often larger than 0.9 or close to 0, and the composites we make average enough days that this shouldn't impact our results heavily.

Specific comments

1. Thank you for noticing this, we agree these paragraphs need to be better linked. The references to past studies do seem like they'd fit well after the first phrase in the 2nd paragraph on page 25278, but if we move this from its current location it takes away from the first 3 paragraphs of the introduction where we: (1). explain aerosol indirect effects; (2). explain the observational approach to understanding AIEs and its limitations; (3). explain the modeling approach and its limitations. The flow from aerosol indirect effects to studies of such effects would, with suggested changes, be broken. The observational approach paragraph transitions to modeling studies paragraph via the problem of meteorological influence, and without it the paragraphs don't connect well.

The discussion of stratocumulus does seem out of place and does interrupt the flow, but we feel it is necessary, because it is the properties of stratocumulus that allow us to make the assumptions that we do to calculate albedo (page 25281). The previous

studies of variability on different timescales (paragraph starting line 25, page 25278 are about stratocumulus clouds, and it seems that discussing these results requires us to have already motivated why stratocumulus in particular is useful to study. Also, the discussion of the important variables controlling albedo requires mention of the fact that we are looking over ocean (so that clear sky albedo variability is practically negligible). We have moved some of these concepts around to attempt to address your valid point, and we hope that the resulting argument flows better.

2. We replaced "The results allow us to hypothesize physical mechanisms to be tested with such a model that explain how meteorology impacts cloud variability. " with "We then use the results to hypothesize physical mechanisms that explain how meteorology impacts cloud variability, which could be tested with a model in future studies."

3. The inputs to the radiative model are Tau, insolar zenith angle, the single scattering albedo (w0), and the asymmetry factor (g). In our calculations we assume the latter two inputs are constant. We have incorporated this information into the paragraph describing the albedo proxy in the revised version.

4. We replaced "Dry subsiding air associated with the high warms the air above the MBL, which can be entrained into the MBL, leading to strong latent cooling of the ocean surface" with "Dry subsiding air associated with the high warms the air above the MBL, which can be entrained into the MBL, leading to cooling of the ocean surface by evaporation."

5. This is a good point, that it is confusing for the reader to flip back and forth between figures. However, Figure 2 demonstrates the potential for droplet concentration being an important field for the mean albedo, and the paragraph that follows refutes that idea, showing that correlations complicate interpretation of Figure 2, that the mean state isn't enough to quantify the aerosol indirect effects. If the second paragraph is read first and makes this point, it seems counterintuitive to then refer to Figure 2 as a point of interest. Figure 2 is intended to motivate the idea that droplet concentration

has a significant impact on albedo, and then the following paragraph referring back to Figure 1 demonstrates that it is more complicated. When we talked about Figure 1 before Figure 2, it was to point out the basic mean state features of the region.

6. Thank you, this has been added to the revised version.

7. North of 30S within 5-10 degrees of the coast, there is some correlation of Nd with LWP and of Nd with CF, but you are right it is not obvious with the image of the entire domain.

8. In line 17 we changed "fraction of variance methodology" to "an equation for the variance of albedo dependent on the variances of the controlling variables". We changed the section 3.1 title to "Albedo variance"

9. Not showing the 4th order terms in the figure does force the reader to blindly trust the paper's statement they are actually smaller than the other contributions. We will specify the magnitudes more clearly. Over most of the domain 4th order terms explain much less than cloud albedo. Values up to 21% occur, but in tiny regions south of 30S near the coast, where zonal flow interacts with the coastal jet and clouds are present less often. To better describe the 4th order terms and remove this confusion we've changed: In text: "contribute 6–10{\%} over most of the domain"

In figure caption: "their relative contribution is smaller (6–10%) over most of the domain (up to 21% in small localized regions south of 30S)"

10. No we meant the variability of all cloud properties considered, we should make this more clear. We changed the phrase "cloud variability" to "variability of cloud properties."

11. The compositing technique was defined because in the paragraphs describing what the SLP EOFs mean, we mention the composites of SLP on the PCs. However, it doesn't need to be in the first paragraph. We consider many variables composited, and could consider any, so we don't specify which variables we will composite, just the method by which we can do so with any variable on hand. We have reworked this

section (4.1 and beginning of 4.2.1).

4.1 "In this section we describe the dominant modes of SLP variability, which essentially represent changes to the subtropical high. We apply Empirical Orthogonal Function Analysis (EOF) ..."

4.2.1 "To understand how other cloud and meteorological variables covary with the meteorological changes associated with the subtropical high, we will 'composite' them on SLP PC1 and PC2 time series."

12. We were trying to show the anomalies represented by PC1 and PC2 may be influenced by barotropic Rossby waves – what was written originally was slightly incorrect. The SLP anomaly composited on PC2 moves faster than that composited on PC1. Since Rossby waves propagate westward, if representing a Rossby wave this would mean PC2 would have a slower zonal phase speed. By Holton, 1992, this means a larger wavenumber and thus a shorter (not longer) wavelength wave. The SLP composite anomalies are narrower on PC2 than PC1, which would be consistent with a shorter wavelength. This is difficult to describe concisely in the text given that we moved the discussion of composites to after this section. We will change "in keeping with a slower zonal phase speed for longer wavelength barotropic Rossby waves " to "consistent with a slower zonal phase speed for shorter wavelength barotropic Rossby waves (Holton, 1992)." We may modify this to make our meaning more clear.

13. We changed "Despite this significant modulation of Nd it still does not contribute strongly to the albedo variance" to "Droplet concentration variance still does not contribute strongly to the albedo variance (Sect.∼3) despite this significant modulation"

14. Good catch – the 'negative correlation' phrase made the meaning of the second half of the sentence ambiguous. We meant that the existence of a plausible dynamical mechanism that causes a correlation might mitigate the need for an attribution of the correlation to aerosol indirect effects, not that aerosol indirect effects cause a positive correlation. Thank you, we have rephrased this section. We changed "This

mechanism would tend to induce a negative correlation between Lp and Nd in the near coastal zone, demonstrating that their covariability is not necessarily indicative of aerosol indirect effects."

to

"This mechanism would tend to induce a negative correlation between Lp and Nd in the near coastal zone, demonstrating a specific way meteorology could complicate the interpretation of such a correlation as indicative of aerosol indirect effects alone."

15. You are right, the second part of the paragraph is meant to tie the section together. Making a new paragraph causes 1 sentence paragraph, so we added: " Although we've identified two possible mechanisms for cloud properties to change due to meteorological impacts, they aren't independent of each other." to clarify the purpose of the paragraph.

16. We've now included a definition for the coefficient of variation (standard deviation divided by the mean, a measure of dispersion normalized). We refer to the squared coefficient of variation, which is just the variance divided by the squared mean. This is important for the interpretation of the terms in equation 5. For example, the first term is the mean of cloud albedo squared multiplied by squared perturbations in fc. Because the squared coefficient of variation in cloud albedo is smaller than 1, the variations in cloud albedo compared with the mean won't change the whole term much, while the fc perturbation term will have more control over the value of this term, allowing us to interpret the term as the contribution from the variance of fc. For efficiency in the text we did not explain this in the caption, but is described in the Appendix. We have added "see Appendix" to the caption.

17. Fig 3 is computed the same way as Fig. 4, it wasn't mentioned in Fig. 3 because the covariance term isn't negative anywhere like in Fig. 4, but good point, this should still be specified.

18.We added: "The "red noise" spectrum is the null hypothesis for the significance of each point, computed using the autocorrelation of the PC at a lag of 1 day."

Technical corrections: Thank you for these valuable catches!

Correction 3. Good point, but larger cloud fraction also means higher albedo. The suggested phrase implies the increase in albedo is due to the increase in Lp when it is both fc and Lp increasing that increases albedo. The covariance term is about covariance between fc and Lp contributing to albedo variance, not albedo covariance with either variable.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 25275, 2009.

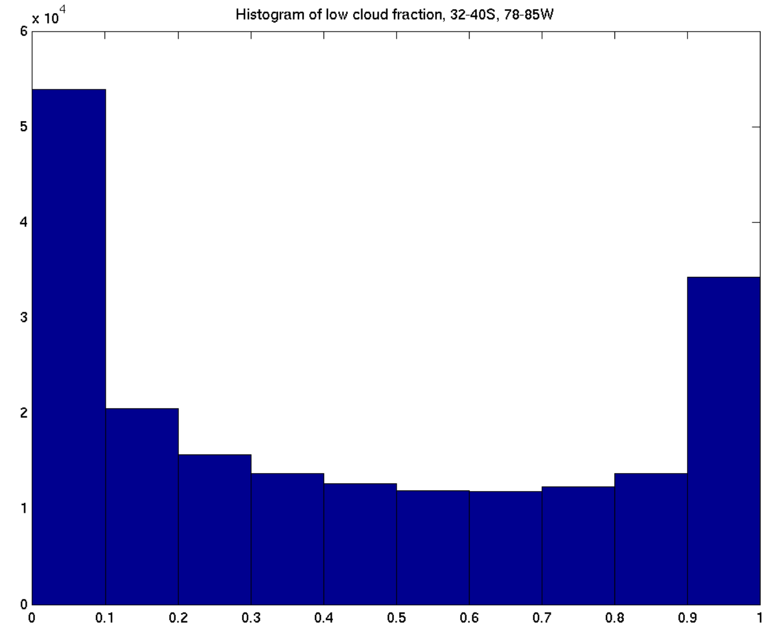

Fig. 1. Histogram of CF in domain 33-40 S and 79-86W

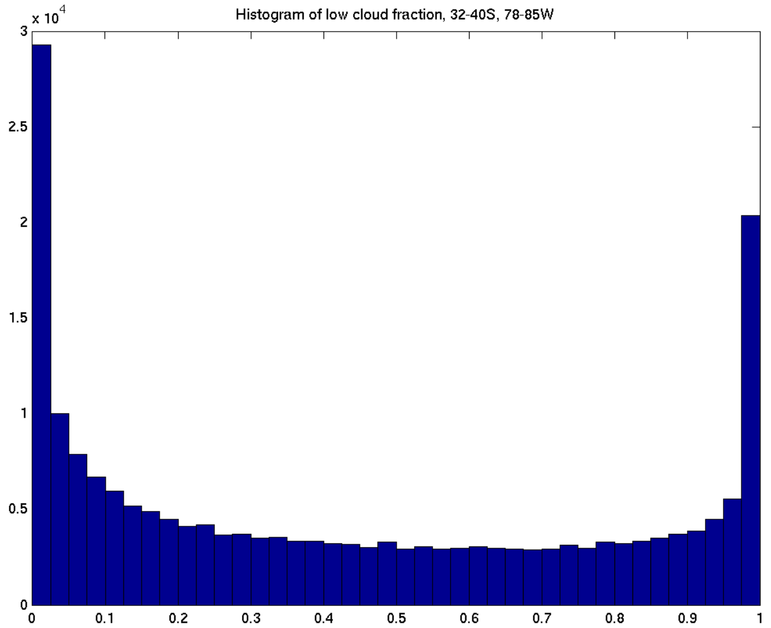

Fig. 2. Histogram of CF in domain 33-40 S and 79-86W