

Review of “Satellite Climatology of Cloud Liquid Water Path Over the Southeast Pacific between 2002 and 2009” by L.W. O’Neill et al.

Overall, this paper provides some interesting new details on water clouds in the the Southeast Pacific. However, I find that some of this ground was covered in O’Dell et al. (2008), and the authors should clearly distinguish what they have done differently from that work. Also, they

Comment on the the diurnal & semi-diurnal model & fitting formalism (sect 2.2
The authors state that the time mean of liquid water path, L_{bar} , is not formally identical to β_0 . How so? The last time I checked, the integral over a complete cycle of sine or cosine is zero; so in fact β_0 IS formally identical to L_{bar} . “Different methods of computation” is not a reason – they are formally identical quantities. One could have been calculated incorrectly, but assuming that this model is correct, they are identical.

This point relates to the fitting of the semi-diurnal cycle as well. The authors appear NOT to fit the semi-diurnal cycle simultaneously with the diurnal cycle fit. They WILL affect one another – you cannot do them sequentially. You must do them simultaneously. The authors should state more clearly how they do this, and if they do them sequentially, they need to re-do the calculation with a simultaneous 5-parameter fit.

Finally, on the topic of the model they assume, they fit for the diurnal (and semi-diurnal cycle) for each quarter-degree grid box, for each month, and performed the fit *independently for each year*. This allows the diurnal and semi-diurnal cycles to change over time in a given grid box and month. The authors should point out to the reader that O’Dell et al (2008) held the diurnal and semi-diurnal cycles fixed for all years, as they found that it didn’t vary much, and they felt that the data did not have the fidelity to accurately fit for the diurnal and semi-diurnal cycles independently for each year. The authors should comment on this. Do they find that they diurnal and semi-diurnal cycles do not change significantly from one year to the next (for a given grid box and month), or do they change in a statistically significant way? Validating or falsifying the assumptions of O’Dell et al. would be a valuable contribution this paper could make, even though it would only apply in the SE Pacific. This is a clear difference between the two approaches and is worth discussing in greater detail.

Also, in section 6, it is not clear (to me at least) that the authors fit the semi-diurnal cycle in a given grid box and month independently for each year. Please clarify this as well.

Comment on synthetic tests (section 3.3)

The authors synthetic test results imply very low fitting errors on the three parameters of equation (1), but they exclude the semi-diurnal cycle explicitly. Fitting for 5 parameters instead of 3 will make the fit of the 3 parameters less accurate (because there is some bleed-through between the diurnal and semi-diurnal cycles, given the incomplete sampling). We recommend the authors choose a single model and fit for it. Could this section be redone with the relaxed assumption that semi-diurnal cycles CAN exist (whether or not you choose to fit for them)? I suspect the recovered errors on the fit coefficients will become somewhat larger when this is done.

Sect 6: I strongly suggest that the authors put their semi-diurnal cycle results in the SE Pacific in the context of the findings of Wood, Kohler, Bennartz, and O'Dell (QJRMS, 2009), specifically the evidence of a strong subsidence wave that can propagate away from the Andes at ~ 25 m/s and induces a cloud response along its track.