

This manuscript attempts to quantify the ENSO contribution to cloud feedbacks using a multiple linear regression approach. Using reanalysis, satellite data and GCM simulations, the authors identify substantial ENSO contributions to local cloud feedbacks over parts of low- and mid-latitude oceans, and smaller ENSO contributions to the global-long term cloud feedback even after several decades.

The manuscript addresses an interesting problem that would be of interest to the readers of ACP. However, I believe there are some conceptual issues with the multiple regression model and therefore recommend major revisions before this work is suitable for publication. I also have a number of smaller comments on the methods.

Major Comment:

The authors use a simple multiple regression model to account for the effects of ENSO. For any variable X , they write:

$$X(t) = a t + b \text{ONI}(t) + c,$$

where t is time, ONI is an ENSO index, c is the residual and a and b are constants. While this is an intuitive model, it has several issues:

1. The relationships between ENSO and the variables of interest here (GMST, CRE) are lag-dependent and tend to peak at lags other than zero (note that this is still an issue even with the band-pass filter). This has been noted in a number of studies, including Xie et al. (2016), Lutsko (2018) and Ceppi and Fueglistaler (2021).
2. The regression model assumes trends that are linear in time. This is clearly not the case in the 4XCO₂ simulations, where GMST is known to respond on two or more time-scales, and may not even be the case over the historical period. The linear time-dependence may weaken the model performance, and departures from linearity could be aliased into the ENSO term.
3. For CRE in particular, the ENSO effect is likely to be asymmetric/nonlinear between El Niño and La Niña, and this effect could be retained in the “corrected” variables.

These assumptions directly affect the magnitude and persistence of the estimated ENSO contributions and, while the authors seem to be aware of some of these issues, they haven't attempted to address them. With a little bit of work, the robustness of their model to these assumptions could be tested, which would give the reader more confidence in their results. At minimum I suggest the authors try:

1. Determining the optimal lag between the ONI and each variable, then using this in their regression model.
2. Replacing t in the regression model with low-pass filtered GMST (say, frequencies of (15 years)⁻¹ and lower).
3. Developing separate models for strong El Niño and La Niña years, to see how the coefficient b differs.

It would greatly strengthen the paper to either make these changes to their model, or show that they give similar results to the simpler version.

References:

Ceppi, P., & Fueglistaler, S. (2021). The El Niño–Southern Oscillation pattern effect. *Geophysical Research Letters*, 48, e2021GL095261. <https://doi.org/10.1029/2021GL095261>

Lutsko, N. J. (2018). The relationship between cloud radiative effect and surface temperature variability at El Niño–Southern Oscillation frequencies in CMIP5 models. *Geophysical Research Letters*, 45(19), 10599–10608. <https://doi.org/10.1029/2018GL079236>

Xie, SP., Kosaka, Y. & Okumura, Y. Distinct energy budgets for anthropogenic and natural changes during global warming hiatus. *Nature Geosci* 9, 29–33 (2016). <https://doi.org/10.1038/ngeo2581>

Minor Comments, Typos, etc.:

L22: The Sherwood et al climate sensitivity review estimates a positive cloud feedback, even if it is still quite uncertain.

L50: “data” is plural

L78: Please say more about how the ONI is calculated for the different datasets. What is the reference period in the 4xCO₂ simulations? It would also be good to clarify that it has units of K (I usually assume an “index” is non-dimensional).

L105: It would be helpful to point out here that using the bandpass filtered ONI mostly eliminates linear trends.

L110/13: There is a circumflex on Y in Eq 1 but not Eq 2.

L123: Strictly speaking the CRE-GMST regression slope is not the same as the cloud feedback parameter, as it neglects things like cloud masking. Please adjust terminology throughout and add a brief sentence acknowledging the limitations of using the regression slope (ignores cloud masking, etc.). This doesn't affect the analysis presented here, but it does affect the interpretation.

Figure 2: Why do you use K for some variables and C for others?

L155: Please explain why you aren't using the bandpass filtered ONI here.

L171: Please list the indices and show the results in the supplement.

Figure 3 and associated discussion: I think it would be worth explicitly pointing out that these maps show where there's a forced CRE signal and where the CRE is still dominated by ENSO-induced variability. It's interesting that outside the Pacific the CRE is generally not related to either of these.

Figure 5: I agree with the previous reviewer that the "ENSO effect minimal time" is kind of odd. Maybe a better definition would be to repeat the ENSO relative contribution calculation (panels j-l of Fig 4) for all times ≥ 10 years and find when the relative contribution first goes above 50%?

L272: Can you say more about why these models feature strong ENSO contributions? There are several possibilities: (1) weak forced CRE trends (e.g., MRI), (2) large ENSO amplitudes, (3) large responses to ENSO (large b in equation 1). In fact, it would be interesting to dig into these a little more. E.g., you could add a scatter plot across of ENSO contribution vs (1) ONI standard deviation, (2) regression coefficient b , and (3) forced CRE trend magnitude.