

Interactive comment on “El Niño, La Niña, and the global sea level budget” by Christopher G. Piecuch and Katherine J. Quinn

Anonymous Referee #1

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This paper quantifies the relative importance of steric and barystatic contributions to global mean sea level change associated with ENSO. It is logically arranged, well-presented, concise and careful, and I hope it will be published.

I don't have any detailed comments on the text, which is very well-written. I have a few comments on aspects of the method and conclusions.

(1) Is there a possible thermosteric contribution from depths greater than 2000 m, which are not sampled by Argo? Previous studies suggest that this is non-negligible for the GMSL trend e.g. Church et al. (2011) 10.1029/2011GL048794.

(2) The method assumes the form of the predictors: MEI, constant linear trend, sinusoidal annual cycle. If the long-term variation is not a constant rate of change, the annual cycle is not sinusoidal, or the MEI is not the right measure of ENSO variation, I

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suppose that the results will have a systematic error, and the conclusion might not be accurate. How well-justified are these assumptions?

(3) Did the authors consider regressing GMSL (from altimetry) against the barystatic and thermosteric contributions as predictors? In that case OLS would be inaccurate because it assumes there is no error in the independent variable, but total least squares (orthogonal regression) could be used.

(4) Having reached their conclusion that barystatic and thermosteric contributions are of comparable importance, could the authors comment on why previous authors reach different conclusions - the situation they described as "confusing" in the introduction?

(5) A minor point: it would be useful to note in Table 1 caption that n^* is evaluated following Eq A3.

(6) Could Fig 5 be put as a panel in Fig 1? It would be helpful to draw attention to the difference between Figs 1a and 5. The most relevant one is that Fig 5 is the whole altimeter period. Are they different otherwise?

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