

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

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Reviewer's Comment (RC): 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.

RC: 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.

Authors' Response (AR): We appreciate the reviewer's positive evaluation of our paper. The manuscript will be revised accordingly, as described in the responses given below.

RC: 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

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for the GMSL trend e.g. Church et al. (2011) 10.1029/2011GL048794.

AR: The deep ocean's contribution to climate variability and change remains uncertain. The findings of Church et al. cited by the referee are taken from the Purkey and Johnson (2010) results, based on precise but spatiotemporally sparse hydrographic section data. Models disagree on the nature of deep ocean changes—some show warming (e.g., Song and Colberg 2011), others cooling (e.g., Wunsch and Heimbach 2014), and still others no significant thermal changes at all during recent decades (e.g., Piecuch et al. 2015). In our analysis, any deep ocean steric contributions would appear in the budget residual term, which is indistinguishable from zero (Table 1). This result is in some senses analogous to findings in Llovel et al. (2014) with regard to the deep ocean temperature trend over the 2005-2013 interval.

We will mention these topics in the paper revision, explaining that, based on our results, any contributions from un-sampled regions are indistinguishable from zero.

RC: The method assumes the form of the predictors: MEI, constant linear trend, and 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 suppose that the results will have a systematic error, and the conclusion might not be accurate. How well justified are these assumptions?

AR: A degree of subjectivity in model selection is inevitable and unavoidable. We believe that the form of the predictors assumed here is reasonable judging from previous works (cited in the introduction). Regression onto these parameters explains 96% (99%) of the monthly variance in the altimetric sea level record over 2005-2015 (1993-2015), and the regression coefficients are all significant, suggesting that our assumptions are justified. Using indices other than the MEI, or allowing lags between MEI and GMSL, yields similar results. Variations in the GMSL annual cycle or its long-term rate of change are of course possible but are not obvious from the altimetry data, and addressing these issues would require a more detailed and dedicated study beyond

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the scope of our analysis.

In the revision, we will argue more clearly that our assumptions are justified.

RC: 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.

AR: We appreciate being made aware of the method of total least squares for the case that the predictors have errors. For various reasons, we hesitate to regress GMSL onto barystatic and thermosteric terms, as suggested by the reviewer. From a mathematical perspective, such a regression would be problematic, because, as we show in the paper (Fig. 1), barystatic and thermosteric terms are correlated. Thus, the regressors would not be linearly independent, as required by least squares. Further, and notwithstanding correlation between the regressors, such regression would be physically unenlightening; from the hydrostatic relation (cf. Eqn. 2.11 in Gill and Niiler 1973), it must be that the coefficients of such a regression equal one, and hence there is insufficient motivation to perform the additional analyses suggested by the reviewer.

For these reasons, we have not made any changes to the paper on these points.

RC: 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

AR: There are a few potential reasons for this confusion, some of which are given below:

The nature of GMSL changes linked to ENSO has been inferred from observations of isolated events, such as the 2010/2011 La Niña. These particular events might not be representative of the general GMSL response to ENSO. As revealed by Fasullo et al. (2013), isolated GMSL events can be related not only to ENSO but also, for example,

IOD and SAM. These considerations complicate interpretation of GMSL, barystatic, and thermosteric data for isolated events in terms of GMSL response during ENSO events more generally.

Some studies base conclusions regarding barystatic contributions to changes in GMSL on strong correlation between the two signals. But, correlations only take into account the relative phase of the signals, and not their relative magnitudes. This can paint a deceptive picture in the present context. To give a toy example, consider two time series,  $x(t) = \sin(t)$ , and  $y(t) = 2\sin(t)$ . Obviously,  $x$  and  $y$  are perfectly correlated, but the changes in  $x$  do not entirely account for changes in  $y$ , as the former only has half the magnitude of the latter. Thus, in cases such as the present, where all terms (i.e., GMSL, steric, barystatic) exhibit similar phase behavior, but varying magnitude, a more thorough analysis must consider both phasing and magnitude of the signals.

Observational and modeling products used to evaluate the steric and barystatic effects on sea level changes are of course characterized by errors. As we reveal in our accompanying response to Referee #2, Argo grids processed by different centers can show important differences regionally and globally. These errors in models and data could lead to links between GMSL and its components that are too strong or weak.

Language will be added to the revised introduction and conclusion that speaks to these points without unduly criticizing previous works.

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

AR: We will certainly make this note in the revised manuscript.

RC: 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?

AR: While it's admittedly a matter of subjective, aesthetic preference, we are hesitant

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to add Figure 5a as a panel in Figure 1, since the former covers a time period different from that in the other Figure 1 panels. Nevertheless, we will add text into the manuscript that points out the differences between the two figures. These differences include the period of display, as pointed out by the reviewer, and also the facts that the (removed) annual cycle and linear trend are estimated for the 2005-2015 period in Figure 1 while they are estimated for the 1993-2015 period in Figure 5a.

## References

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