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Interactive comment on "A Mechanistic Analysis of Tropical Pacific Dynamic Sea Level in GFDL-OM4 under OMIP-I and OMIP-II Forcings" *by* Chia-Wei Hsu et al.

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We thank the reviewer's useful comments and suggestions. To address all reviewer's questions, we listed out all questions and the corresponding answers from us below.

–General Comments

-1. My major comment/concern after reading through this quite lengthy manuscript is that I feel it may be more appropriate for other journals than GMD – a journal focusing on model development. But I have to say it's my personal feeling based on my understanding of GMD versus other journals, and it's really up to the editor to decide. If the

authors agree on this, I suggest the authors can reduce some parts (which don't really show new results), and tighten up the storyline. I guess it would be a better paper.

A: In our opinion, the Geoscience Model Development journal fits well with the purpose of this paper which is focusing on the model evaluation. This is in-line with the "Model evaluation papers" listed in the GMD manuscript types. Our study uses the state-of-the-art ocean model comparing with observations across different time scales which can help us systematically understand the current model biases and further improve the model simulation and development in the future.

-2. Wind stress and wind stress curl are heavily discussed throughout this paper, which is fine. However, the coarse-resolution (4x4 deg) WASwind product is used, and differences between JRA55-do and CORE are defined as "biases". I feel it could be problematic. For example, some features in the wind stress curl map (Fig. 11b) may be "artificial", reflecting the 4x4 grid. I think there should be some better-quality wind products, e.g., based on satellite Scatterometer observations?

A: Yes, we have tried CCMP a multi-mission surface wind analysis that backdates to 1989 and has a higher resolution (0.25x0.25 degree). However, in our study, we can only find significant changes of wind stress trend bias over a longer period (1950-2011) which WASwind provides but not available in CCMP (1989-2011). As an alternative way to verify the WASwind result, we compare the WASwind and CCMP data in the mean, long-term trend, and seasonal variability over the overlapping period. The results are comparable. We did not see significant differences across time scales. Like WASwind, CCMP also shows no statistical significance in long-term trend analysis during the overlapping period. The other important reason we think the coarse resolution does not affect our result is due to the biases in this study are all more than 4 degrees (Figure 4, 4N - 9N, figure 12, figure 16,17 2N-10N). Therefore, we will keep the WASwind analyses in our study. However, we do agree figure 11b,d,f might be somewhat misleading since we are not focusing on all bias that shows on the map that has a smaller spatial scale. The original purpose of the figure is to show the spatial pattern of the bias that we want to focus on in figure 12. In the revision, we will remove figure 11b,d,f to avoid this confusion. As for the JRA55-do and CORE difference, we do carefully avoid the word “biases” since it is not a comparison with observation. In this part of the analysis, since the observation is limited, we use JRA55-do as a relatively good estimate to evaluate improvement from CORE forced result. As for why JRA55-do can be assumed as a relatively good estimate, it is due to the significant reduction of sea level bias during the period when the observation is available (1993-2007). These reasons are mentioned in our manuscript line 232-237.

–detailed comments

-Line 23 (L23), this sentence is confusing and thus needs to be changed. Sea level variability is not only associated with ocean temperature (heat content), but can also due to halosteric component (ocean salinity) and mass component.

A: We will change to “Sea level in the tropical Pacific is dominated by the ocean heat content variability and long-term trends.”

-L31, OMIP-I and II are not defined before. I feel they should be introduced here with a couple of sentences. What are main differences and similarities between them? It may help to state your motivation more clearly. Some material from the 2nd paragraph of Section 2.1 (L63-69) can be moved here.

A: We will move part of Section 2.1 here to strengthen the motivation. “OMIP-I is forced by the CORE dataset of Large and Yeager (2009) and it extends over years 1948-2007 (hereafter, CORE), whereas OMIP-II uses the JRA55-do dataset of Tsujino et al. (2018), which extends over years 1958-2018. ” will be moved up.

-L96-97, steric sea level rise is not only due to ocean warming, but also ocean freshening (decreasing salinity).

A: We will change the sentence to “The minus sign on the steric term arises since decreases in density, as from ocean warming or freshening, lead to increases in sea

level.”

-L105, change “which relates” to “which mainly relates”, since salinity can also play a role, in addition to ocean temperature (or equivalently thermocline depth change).

A: We will change the sentence accordingly.

-L115 “sea level variations” should be more appropriate here than “sea level changes”

A: Agreed. We will change the sentence to “sea level variations”.

-L152-153, This statement is reasonable, but you used a coarse-resolution (4x4) wind product (WASwind) as your observational reference, could it affect your derivation of “biases” as shown in Fig. 4 and discussions about wind stress curl in the following sections?

A: To show the coarse resolution is not affecting the analyses we mentioned here, we calculate the coarse resolution WASwind and the fine resolution observation from CCMP (0.25*0.25 degree) over the overlapping period. For the mean-field, it shows a maximum difference between the two products of 40% to WASwind value in the tropical region. This difference can come from the observational technique difference, the resolution difference, or a combined effect. However, the bias we want to show here between 4N to 9N is more than 200% of the observed value in WASwind. This means that even we assume the 40% difference between the WASwind and CCMP is all coming from resolution difference, it is still smaller than the bias we want to show here.

-L225-229, one important aspect from Fig. 8, not discussed here, is that this sea level trend map (east-west contrast) mainly results from decadal variability rather than represents the long-term trend, as discussed by Bromirski et al. (2011), Zhang and Church (2012), and Hamlington et al. (2014).

A: Agreed. We will add in the decadal variability studies since this would also affect the trend analysis over a short time period.

-L247, by calculating the degrees of freedom in this way, you treat each monthly data point independent from each other, which is not true. You need to calculate "effective" degrees of freedom by considering the autocorrelation of the time series.

A: Agreed. We will update the description here related to the degree of freedom. The results from the analyses have little to no change after updating the degree of freedom.

-Fig. 11, for the left three panels (i.e., a, c, e), it would be ideally to plot the corresponding vector (e.g., JRA55-do bias wind stress vector), rather than the same mean wind stress vector. By doing so, you would also show the meridional wind stress information, which help to understand wind stress curl plots on the right.

A: Yes, we will change panel c,e to vector showing the bias.

-L261: you may want to give information of the "five-year mean", over which period?

A: It is 2000-2004 [Large and Yeager, 2009]. We will add this info in the revision.

-L288-291, as commented above (L225-229), deriving trends over short periods can be influenced by interannual to decadal variability.

A: Yes, we agree. We will add the citation and mentioned the possible contribution from decadal variability based on past studies.

-L307, this 0.3 m/s doesn't make sense to me, shouldn't it be around 0.9 m/s (it takes about 6 months for 1st baroclinic Rossby waves to travel across the tropical Pacific basin, which gives a speed of about 0.9 m/s). A simple check of Fig. 16 doesn't support 0.3 m/s.

A: The calculation is done by looking at the contour (dashed) in figure 16a,c,e (or figure 18f which shows the 20C isotherm depth) starting from 110W to 150E (total 110 degree longitude) from January to December. The number we used to calculate the speed is as follows. 110 [degree longitude] * 100000 [m/degree] / (12*30*24*60*60) [s] $\sim$ 0.35 m/s This baroclinic Rossby wave and its non-dispersive phase speed are also mentioned

in Meyers which matches with our number [1979]. We will add the above information in the revised manuscript for clarification. Meyers, G. (1979). On the Annual Rossby Wave in the Tropical North Pacific Ocean. Journal of Physical Oceanography, 9(4), 663–674. https://doi.org/10.1175/1520-0485(1979)009<0663:OTARWI>2.0.CO;2

-L376, is it possible to use Johnson et al. (2002) as an observational reference and overplot it in Fig. 19c?

A: Johnson et al. [2002] only mentioned the maximum (peak) month in their study. We could not find a whole seasonal cycle (monthly values) available.

-L423, the range for the third stage (Jan-June Year 2) overlaps with the 2nd Stage (Aug Year 1 to Jan Year 2).

A: Right, this is an intentional overlap since Jan Year2 is when the maximum ONI happens. The main purpose of this analysis here is to show that the difference of 6 months mean before and after the maximum ONI is very different. We also tried to shift one month for both stage3 and stage4. It shows little change to result.

-L435-437, there are already some studies on the meridional asymmetry, e.g., by McGregor et al. (2012).

A: Thank you for letting us know about this study. We will cite this study in this part of the analysis.

-L467-468, by designing this regression between DSL and wind stress curl at each grid point, are you implying that sea level responds to mainly local Ekman pumping and wave propagation can be neglected (using the simplified 1st baroclinic Rossby wave model as an example).

A: No. The regression map is trying to emphasize the strong correlation in the region of interest (black dashed box in figure 22 23 24 25) mentioned in the analyses when comparing with other regions. It does not imply the wave propagation during ENSO can be neglected. Wave propagation still plays a central role in the ENSO dynamics.

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