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Geoscientific Model Development Discussions
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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.

Anonymous Referee #1

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General comments

In this paper, authors present dynamical features of tropical Pacific sea level simulated by GFDL-OM4 under OMIP protocols. Authors investigate biases of simulated dynamic sea level (DSL) forced by the distinct surface atmospheric datasets prepared for two phases of OMIP (CORE and JRA55-do for OMIP-1 and OMIP-2, respectively). They take up the following aspects of tropical Pacific DSL field that characterize its mean state and variability.

- Time mean (Section 3).

- Decadal and longer trend (Section 4).

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Discussion paper

- Seasonal variability (Section 5).

- Response to interannual ENSO variability (Section 6).

Above aspects of the tropical Pacific DSL is largely determined by imposed wind stress forcing. The long-term mean DSL has positive bias around the intertropical convergence zone in both OMIP-1 and OMIP-2 simulations. This is caused by the biases in wind stress forcing which have been introduced into the forcing dataset by the method to adjust wind vectors. The long-term trend over the northern tropical Pacific is improved in OMIP-2 relative to OMIP-1, but it is still suffered from a negative bias due to easterly wind trend along 20N. Seasonal variability of the North Equatorial Counter Current, which is caused by the seasonal variability of DSL, is also improved in OMIP-2. Both OMIP-1 (CORE) and OMIP-2 (JRA55-do) forcing datasets generate realistic DSL variation during El Niño/Southern Oscillation (ENSO), which includes a meridional asymmetry across the equator.

I think that results are presented well overall and I do not find any serious issues in analysis and reasoning. However, I am also a bit afraid that the paper may give some readers an impression that the presented contents are somewhat superficial. I thought that in several places descriptions are given without providing robust evidences and thus not easy to follow. Specifically, the discussion about the cause of the absence of the observed DSL trough around 9N in simulations is made using zonally averaged zonal wind (e.g., Fig.4b). Considering the slanting distribution of the wind convergence zone in the eastern tropical north Pacific, two-dimensional distribution of wind stress curl anomaly might be more illustrative. The seasonal variation of NECC might be more clearly explained by using seasonal evolution of horizontal distribution of DSL in the tropical north Pacific. Also, I thought it would be helpful for the reader if authors add some paragraphs that give quantitative guidance about how the simulated biases may compromise practical assessments of sea level variability based on the OMIP simulations. For example, are the biases of DSL trend comparable to global mean sea level rise observed in recent years? I would like to ask the authors to add or revise materials used to explain important features. This would attract a wider range of readers.

Specific comments

L129: "DSL correlates well with the surface wind stress in the mean state". I think that this expression is somewhat inappropriate. I would suggest something like, "distribution of DSL can be explained well by the surface wind stress in the mean state", if I understand the authors' intention correctly.

Figures 3 and 4: Given that the locations of wind stress convergence are slanting from southwest to northeast in the eastern north Pacific as shown in Figure 3a, contribution of zonal derivative of the meridional component to wind stress curl might not be neglected and the horizontal distribution of wind stress curl bias would be of interest. Is the bias pattern of either wind stress curl or Sverdrup stream function comparable with the DSL bias shown in Figures 3b and 3c? A bit more detail would clarify the point that should be improved in the forcing dataset.

L.213-215: "We hypothesize that the key reason for the weak NECC is due to both the underestimated zonal wind stress in JRA55-do and a flattening of the DSL trough due to the wind stress curl bias in the northern tropical Pacific found in both CORE and JRA55-do [figure 4b,c]." I think that a bit more detailed explanation is required about how the underestimated zonal wind stress in JRA55-do is related to the weak NECC.

L.251: Does the excessive westerly wind trend in CORE and JRA55-do affect simulated features of the global warming hiatus?

L.374-375: How does the improved seasonal variation of DSL in JRA55-do result in the better seasonal variation of NECC?

L.493-495: "the bias in the wind stress forcing causes biases in the geostrophic current that leads to the flattening of the DSL gradient in the meridional direction". I think that the flattening of the DSL leads to the biases in the geostrophic current. For example, "the bias in the wind stress forcing causes flattening of the DSL gradient in the meridional direction that leads to biases in the geostrophic current".

Technical comments

L58-59: It would be worth noting here that baroclinic deformation radius in the tropics can be resolved by the 0.25-degree resolution used by GFDL-OM4 as also noted in L.122-123.

L180-181: "the heat is not stored in the eastern tropical Pacific but flushed to the western tropical Pacific". I think this is worth noting here that "details are discussed in the next paragraph".

L.218-220: I think discussion about trend can be moved to somewhere around the paragraphs that discuss DSL trend using Figure 8 in section 4.

L.252: "extends" should read "extending".

L.266: "(called offsetting factor)". That is actually used as an offsetting factor to correct JRA-55 wind.

L.314: "off" should perhaps read "offset" or "delayed".

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