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Interactive comment on "Deep Circulation in the South China Sea Simulated in a Regional Model" by Xiaolong Zhao et al.

Anonymous Referee #2

Received and published: 29 May 2019

Review of "Deep Circulation in the South China Sea Simulated in a Regional Model"

Zhao et al. presented a numerical study on the spatial characteristics of the deep western boundary currents (WBCs) in the South China Sea (SCS), using an eddy-resolving configuration of HYCOM. They addressed the role of Pacific overflow water and enhanced diapycnal mixing in studying the dynamics of the WBCs. The study provides some quantitative model diagnosis which could be useful to the relevant research community, however, I am not convinced that this work brings further insight into understanding the dynamics of WBCs in general or specifically in the SCS, and therefore has limited contribution to advancing the science for this topic. The simulations are more like a model exercise on idealized vertical mixing, although the authors have been trying to compare their results to observations and previous model stud-

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Discussion paper

ies. Many numbers quantified and conclusions drawn seem to merely confirm what has been found before in the literature. In my opinion, this work to some extent also suffers from flawed methodology, inadequate discussion/analysis, missing information, and poor presentation of figures.

I acknowledge the efforts that the authors have put and the potential value of the topic, but I have to recommend rejection of the manuscript with its current status. Some overall and more specific comments are as follows.

1) model configuration (method): I am not convinced that the authors can make any meaningful quantification of the WBCs, nor any one-to-one comparison with observations, without surface forcing and with closed open boundaries. "... evidence of surface forcing to the deep layer dynamics has not yet been found" is not a good argument, and does not necessarily mean that surface forcing is not important. It is possible that the role of the surface forcing is not as important as the overflow inflow at Luzon Strait (LS), but it remains unknown unless some sensitivity experiments are carried out to test it. Similarly, ocean circulations in the LS and SCS are highly interactive with circulations in the western Pacific. Although T/S are relaxed at the open boundaries, but without inflow from the Equatorial Pacific and outflow leaving the model domain in the north (e.g. Kuroshio), how would this affect the analysis and quantified properties of the deep WBCs in this work?

If too much work for implementing or turning on the surface forcing and open boundaries, the authors should at least make clear of the limitations in the manuscript, and/or discuss how this could potentially affect their simulation results.

Furthermore, I am also not convinced by the approach the authors did with enhanced mixing. a) The configuration of enhanced mixing is still highly idealized compared to the observations compiled by Yang et al. (2016), which showed an inhomogeneous distribution of diapycnal mixing in the zonal, meridional, and vertical directions. For example, the vertical mixing in the northern SCS and near the bottom is much larger

(up to 10ˆ-2). I am not saying the model setup is wrong, but I think the authors should add more discussions on this point, and make clear of the caveat and idealized nature of their model setup. b) the two hot spots with enhanced mixing, separated by a narrow band of low mixing in the model seems bizarre to me; would it make any difference if this narrow band is also filled with high mixing? There actually seems a lack of observations within this band in the observations of Yang et al.

2) Model spin-up and validation I do not see sufficient validation and assessment of the simulation. A comparison of velocity cross-section with observations (e.g. Fig. 2 only) provides very limited support on the robustness of the simulation results. For instance, how does the model perform in simulating the T/S properties in general for the SCS (at least for the mid- and deep ocean, if not for the whole depth)? Importantly, as the authors deem the dense overflow at the LS to be a crucial factor in determining the dynamics of the WBCs, how is the model behaving in simulating the overflow at the strait? e.g. are the T/S/rho properties of the overflow captured? Are they descending to the bottom basin without being (numerically) diffused? The authors mentioned some numbers on the volume transport of the overflow, but how is the overflow defined?

Besides, regarding the model-data comparison (Fig. 2), "As expected, the control run shows reasonable agreement with the cross-section observations" (L171) seems like an overstatement to me. The WBCs and the return flow are clearly stronger with a broader core compared to the observations. This inconsistency and the possible reasons are not adequately addressed.

As for the model spin-up, the authors need to justify that 20 years are long enough for the model to reach equilibrium or quasi-equilibrium. Evidence should be shown; examples are timeseries of volume transport of the Pacific overflow and the deep WBCs, timeseries of deep basin T/S, and so on.

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Some more specific (not necessarily minor) comments are listed below.

> L9: the first sentence does not need to and should not appear in the abstract;

> L35-37: just to clarify, the 'three-dimensional circulation' is one component of the SCS throughflow, and it is the latter that serves as a heat/freshwater conveyor, right?

> L38-52: while the topic of this work is on the SCS deep WBCs, the authors use lots of space introducing the overflow at the LS (I do understand that the WBCs are strongly linked with the overflow). The authors could consider reducing this part.

On the other hand, having given so much background information on the overflow, the authors are expected to describe a bit more on the model representation of overflows, other than merely giving a number of volume transport. See also my earlier comment 2).

> L41-44: the geographical locations are challenging to the readers. The authors could consider avoiding this, or provide a map showing their locations.

> L46: rms – full name should be given here.

> L68-69: '... and higher resolution' – how much higher? e.g. slightly higher than 0.4/0.5 degree, or as high as the resolution in this study?

> L71: what is a/the 'north deep circulation'?

> L76: "Since the DWBC is due to the LS overflow and the beta effect" – this assertion appears to come out of the blue; is this very well known already? Any reference?

> L77: To me the resolution is a secondary consideration. Whether the model produces sufficient overflow water that spills over the sill and whether the water can descend down to the bottom with proper entrainment en route are more important. This could well be my personal biased view though.

> L81: I don't see the logical connection between "Due to the lack of field observations" and the rest of the sentence.

> L85: It should be made more clear that the measurements of Tian et al. and Alford et al. covers the LS and only a small part of the SCS.

> L100: what do you mean by saying "... would have a negative effect on driving the cyclonic SCS deep circulation"?

> L137: please be more specific over here in terms of what is in good agreement with observations. Also, if the model by Zhao et al. (2014) is already performing well in the LS, how about the behavior of the control simulation with enhance mixing at LS?

> L146: larger not lager

> L153: "simulated"

> L154: "mechanism" – I don't see much analysis or discussion on the mechanism of the deep WBCs throughout the main text.

> L158-160: I don't really understand what the authors mean by "re-coordination" and "projection" here.

> L164-166: to me this seems too speculative.

> L170-171: what do the authors mean by "is the same status"? do they mean that the model reproduces the observed volume transport of the overflow, but overestimates the strength of the deep WBCs?

> Figure. 2: a comparison of cross-sectional T/S/rho would also be beneficial.

> L174: across, not along?

> Figure. 3: It seems like this figure is not sufficiently discussed/cited in the main text, which questions the inclusion of all the four panels in the figure.

> Figure. 4: the figure needs to be improved. 1) The vectors are crowdedly clustered in the WBC region, whereas in the other region the arrow heads (aka directions of flow) are very difficult to see. 2) Are the three isobaths randomly selected? 3) units

are missing for the value of sigma_2; "28th layer" not just "28th". 4) the authors could consider making the plot with data from, say every 5th grid point, to avoid the busy vectors, or could consider plotting the stream function.

> L182: "Therefore, . . ." I don't see the logic of 'therefore' here.

> L188-192: the description on the WBC path is challenging for the readers to follow; please consider adding some schematic arrows in the figure.

> Figure. 5: I don't see the point of having both Figures 4 and 5 in the paper.

> Figure. 6: this figure just seems to be a variant of Figure. 3. I don't see much added value or information from it.

> L196-197: not necessarily; strong entrainment is expected to occur during the descent of overflow water.

> L202: how is EKE defined? It might be obvious for some, but not to all readers.

> L216-218: This seems a weak indication to me without knowing more key information from this sensitivity experiment. For instance, is the strength and distribution of the overflow and/or WBCs similar to that in the control experiment?

> Figure. 9: this is a very poor figure. Perhaps a short table would summarize what the authors want to say over here; but if they do would like to show the figure, please make sure that it is displayed clearly and is readable.

> L223-224: why is the intensity of the deep WBCs in case Exp-3A reduced relative to Exp-5? Could it be because more dense waters from the lower layers (e.g. 28 and 29) are mixed upwards due to the enhanced mixing in the northern SCS? If yes, perhaps more layers should be included for a fair comparison? The same applies to the other experiments. Furthermore, this might be too much to ask, how does the deep ocean T/S change with enhanced/reduced vertical mixing?

> L230-231: I don't understand this sentence here.

> Figure. 10: If I understand correctly, there are negative values (i.e. downwelling) along the deep WBC path (e.g. east and south of Zhongsha Islands). This seems to be contrary to what the authors claim in the text?