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Discussions

Interactive comment on "Hydrographic survey over the Carlsberg Ridge in May 2012" *by* Hailun He et al.

Hailun He et al.

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Response to reviewer # 2

This manuscript introduces hydrographic observations collected over the Carlsberg Ridge in the northwest Indian Ocean. The authors have created a hydrographic dataset that comprises observations collected by a ship and by Argo floats. They identify key water masses, conduct particle tracking experiments, and use the observations to evaluate two re-analysis products. With a few exceptions, the standard of written English is acceptable and the quality of the figures is, on the whole, satisfactory. I think that this manuscript will require substantial revision prior to publication.
Response: We are thankful to review's encouragement and constructive suggestions.

We made major revision on the early version manuscript.

Point 1. My primary criticism of this manuscript is that it lacks a clear message; I am unsure of what it is that the authors want me to remember as being important or new. The authors correctly point out that the northwest Indian Ocean is not well sampled, and so any new observations from this region are of value. However, beyond simply presenting these new observations, the authors, in my view, do not sufficiently demonstrate what we can learn from them. The discussion section, which is where the value of the new observations should be made explicit, largely restates points already made in the results section. It does not cite a single piece of literature. I think that the discussion section needs to be substantially revised: it should explain the value of the observations in the context of relevant literature and, ideally, it should set out a clear argument.
Response: The main motivation is to understand the ocean environment over Carlsberg Ridge (CR). The novelty lies on the extra information brought by in-situ CTD & XCTD data. The sectional snapshot therefore gives the vertical structures of temperature, salinity, density and geostrophic current. In the revised manuscript, we emphasized three valuable results: (1) The snapshot of water masses, (2) the structure of mesoscale eddy, and (3) the structure of west-propagating disturbance. We revised the Introduction and Discussion sections, accordingly.

Point 2. The methods outlined in section 2 need to be better explained. The authors note the depth-mean offsets between temperature and salinity observations collected by the ship's CTD and the expendable CTDs (xCTD), and between the xCTDs and an Argo float. Firstly, the implication is that these offsets used to calibrate the xCTD observations ? but this should be stated explicitly. Secondly, it is not clear to me whether: (1) xCTD observations are being compared to both ship and Argo observations; or (2) whether ship observations are being compared to xCTD observations, which are then being compared to Argo observations. If the former, which of the two sets of offsets are the authors using for the calibration? If the latter, are the authors calibrating

observations from all Argo floats using an offset calculated from just one Argo float? Furthermore, do publicly available observations from the Argo programme need to be calibrated? Are they already calibrated when they are made available for download?
Response: [Page 5, line 5 in change-track-mode revised manuscript] Good suggestions. In section 2.1, we just clarify the differences among CTD, xCTD and Argo float. We did not calibrated the xCTD and Argo float before further low-pass filter processing. To avoid misunderstanding, we add a sentence as
"We later use objective analysis method and low-pass filter to reduce the differences among CTD, XCTD and Argo."

Point 3. The authors point out several times that adding the Argo observations to the ship CTD and XCTD observations enables them to examine mesoscale processes. This may well be the case, but I think that they need to carefully consider the temporal and spatial scales of mesoscale activity in the Arabian Sea. For instance, they include Argo observations from up to 200 km from their section over the Carlsberg Ridge: is this distance less than the Rossby radius at this latitude? Furthermore, Table 1 indicates that the observations were collected over a period of one month. Are the authors confident that these observations may be presented in one section (Figure 4) as if they were a synoptic snapshot?
Response: In the monthly mean sea surface height (Fig. 2c in revised manuscript), two dynamics include meso-scale eddy and west-propagating disturbance are identified, we therefore confirm the two dynamics are beyond the synoptic snapshot.

Line-by-line comments.
Page 1, line 14. I am not sure what the authors mean by "renewed" in this context.
Response: [Page 1, line 14 in change-track-mode revised manuscript] We deleted the "renewed".

Page 1, lines 15–17. I am not really sure what this sentence means.
Response: [Page 1, line 15-16, in change-track-mode revised manuscript]
"Moreover, the monsoon builds up a meridional current in the NWIO, which changing

the form of the customary zonal current (as in the Pacific and Atlantic Oceans) into the meridional current."
was changed to
"Moreover, the monsoon is so strong to change the pattern of basin-scale circulation. The monsoon builds up a dominant meridional current in the NWIO, which changing the form of the customary zonal current (as in the Pacific and Atlantic Oceans) into the meridional current."

Page 1, paragraph beginning line 21. This paragraph is not relevant
Response: We deleted the paragraph.

Page 2, paragraph beginning line 13. This paragraph outlines the reasons for studying the hydrography of the Carlsberg Ridge region, but none of the points raised is revisited later in the paper. The paper would be much improved if, when discussing the results, the authors revisited some of these points for instance, saying how these new observations help to determine the movements of sporadic hydrothermal activity.
Response: [Page 13, line 21-26, in change-track-mode revised manuscript] We added the response to second reason in the Discussion Section, as
"Present paper restricts the sectional study in the upper 1050 m (Fig. 4-5). Within this depth, the water is easy affected by surface forcing. However, on the basin-scale wind-driven circulation, the surface wind forcing affects deeper ocean through quasi-geostrophic instability (Rhines and Young, 1982) and meridional overturning circulation. Generally, the hydrothermal plume in CR uplifts from sea bottom to water depth 2500 m (Murton et al., 2006; Wang et al., 2017). Because present paper concentrates on the CR as well as the cross-ridge current, the results provide potential use in the future study of hydrothermal plume event."

Page 2, line 17. I do not agree that the results presented "shed new light into the basic energy theory of ocean circulation"
Response: [Page 2, line 30, in change-track-mode revised manuscript] We weaken the statement as: "provides a reference to the basic energy theory of ocean circulation".

Page 2, line 33. It would be nice to have the distance between the CTD and the xCTD station given in km.
Response: [Page 4, line 1, in change-track-mode revised manuscript] We added the distance in km

Page 3, line 6. Again, it would be nice to have the distance between the Argo and the xCTD station given in km.
Response: [Page 4, line 6, in change-track-mode revised manuscript] We added the distance in km

Page 4, Figure 1. I think that panel (a) is too small to be of much use: I can't really see the detail because the symbols are too close together. Panel (a) might work better as a separate figure. Similarly, I cannot distinguish individual vorticity contours in panel (b). Contours are not labelled, and the contour interval is not given. The vorticity field should be presented using filled contours, similar to the ADT field in panel (d).
Response: We revised the Figure 1 according to reviewer's suggestions.

Page 5, line 28. I assume that the data extracted from the re-analysis products are along the same section as the observations, but this should be said explicitly.
Response: [Page 6, line 12, in change-track-mode revised manuscript] Yes. We added a statement in section 2.4, as
"For comparison, we extract the reanalysis datasets along the same section as the observation."

Page 6, line 8 (equations). The symbol w is not defined. I assume it is vertical velocity? The authors should state whether they are performing their particle tracking experiments using 2D or 3D velocity.
Response: [Page 6, line 18-19, in change-track-mode revised manuscript] In section 2.5, we insert two sentences, as
"w is the vertical velocity"
and

"we use the three dimensional velocity (U, V, w) to track the tracers,"

Page 6, line 10. The authors give the start and end time of the particle tracking experiments here, and not in the caption of Figure 6.
Response:[Page 6, line 20, in change-track-mode revised manuscript] Corrected.

Page 6, line 19. I do not understand what the authors mean when they say that the "wind stress curl highlights the strong seasonal variations". Also, wind stress curl is not shown in a figure.
Response: [Page 7, line 2, in change-track-mode revised manuscript] We deleted the sentence.

Page 6, line 27. The statement that warm-core eddies "seem to release footprints in the wind stress curl" does not make sense. I would have thought that the wind influences the eddies and not the other way around. Furthermore, when talking about wind stress curl, the authors refer to Figure 1 (b), but the figure caption says that this panel shows wind velocity and vorticity, not wind stress curl.
Response: [Page 7, line 10-11, in change-track-mode revised manuscript] We deleted the sentence.

Page 6, 29. It is not clear why the westward current is "remarkable". Has this feature not been observed before? Is it significant? Do the authors believe that it cannot be explained by their observations, or by existing theory?
Response: [Page 7, line 12, in change-track-mode revised manuscript] We noted the westward current "remarkable" according to the magnitude and zonal extent. As refer to Maximenko et al. (2005), present observation displays the vertical structure of this westward current. The related theory needs to be further confirmed.
Maximenko, N. A., Bang, B., and Sasaki, H.: Observational evidence of alternating zonal jets in the world ocean, Geophysical Research Letters, 32, L12607, 2005.

Page 7, line 7. Strictly speaking, the boundary between the tropics and the subtropics is around 23.5°N, so all of the observations being considered here are from the tropics.

Consequently, the use of the word subtropical is misleading. Subsequent uses of these terms should also be revised.
Response: We corrected the statement accordingly. We changed the "tropical band" to "equatorial band", and changed the "subtropical band" to "tropical band" in the whole paper.

Page 7, line 7. I do not think the authors are justified in grouping PGW and RSW as RSPGIW. In Figure 2, the water masses are observed to be separate, and the authors have acknowledged the different densities of the two water masses.
Response: [Page 7, line 28-29, in change-track-mode revised manuscript] We deleted the statements related to RSPGIW.

Page 7, line 18. The observations from the World Ocean Atlas presented in Figure 3 are from a climatology and should not, therefore, be described as a snapshot.
Response: [Page 7, line 30, in change-track-mode revised manuscript] We corrected the statement.

Page 7, line 19. The authors have not marked north and south on Figure 3, so it is difficult for the reader to make sense of statements such as "the thermocline... deepens northward".
Response: [Page 18, Figure 4, in change-track-mode revised manuscript] We therefore added the latitude in Figure 3 (Figure 4 in revised manuscript).

Page 7, line 22. I do not really agree with the authors's point about ventilation. The outcropping of isotherms visible in Figure 3 is from within the mixed layer? it is not clear that "subsurface" water is then able to take part in air-sea interaction.
Response: [Page 7, line 34, in change-track-mode revised manuscript] We argue that the mixed-layer is approximately well-mixed bulk layer, which can be defined as the layer of temperature within SST minus 0.1 (or 0.5) °C. In Figure 3 (Figure 4 in revised manuscript), the interval of temperature contour is 1.0 °C, therefore, we suppose that the outcropping isothermal line was below the mixed-layer, and argue the "subsurface"

water taken part in air-sea interaction.

Page 7, line 28. The authors do not explain why their observations show that saline intermediate waters are PGW and RSW ? nor do they explain why this is not clear in the climatology.
Response: [Page 10, line 5, in change-track-mode revised manuscript] Basically, we identified the PGW and RSW from T-S diagram. We added the reason in revised manuscript. For explain on the climatology, we added
"In other words, the comparison implies that the year 2002 is an anomaly year on the activities of ASHSW, PGW and RSW."

Page 7, line 31. Why have the authors chosen the 22 kg m-3 isopycnal?
Response: We chosen 22 kg m-3 because it was the first near-surface isopycnal in climatology, meanwhile, and it characterized the differences among four datasets.

Page 7, line 33. It is hard to compare Figure 1b and Figure 3, because Figure 1b uses degrees and Figure 3 uses km.
Response: [Page 18, Figure 4, in change-track-mode revised manuscript] We added the latitude in the Figure.

Page 7, line 33. I do not understand what is meant by projecting the outcrop point in the wind vector field.
Response: [Page 10, line 9, in change-track-mode revised manuscript] We deleted "when we project the outcrop point in the wind vector field".

Page 8, Figure 2. I am not sure why the mean temperature-salinity curves have been plotted. They are not mentioned in the text and they distract from the new observations.
Response: [Page 9, Figure 3, in change-track-mode revised manuscript] We moved out the mean temperature-salinity curves.

Page 9, line 1. I don't think that the thermocline can be described as descending "sharply".

Response: [Page 10, line 14, in change-track-mode revised manuscript] We changed "sharply" to "steeply".

Page 9, line 5. I don't understand what is "remarkable" about the southward extension of salty water.
Response: [Page 10, line 18, in change-track-mode revised manuscript] "the remarkable southward extension of salty water in the upper ocean"
is modified to
"the southward extension of ASHSW".

Page 9, line 15. Again, I don't understand why the velocity field in the upper ocean is "remarkable".
Response: [Page 10, line 28, in change-track-mode revised manuscript] We changed "remarkable" to "relatively strong".

Page 9, line 19. I think the authors need to label features of interest? eg CCE2 ? on Figure 4. As it stands, it is quite hard to see what the authors want the reader to look at.
Response: [Page 19 Figure 5, in change-track-mode revised manuscript] We added the position of CCE1, CCE2 and WPD in the Figure.

Page 9, line 22. The authors need to discuss, somewhere in the paper, the significance of the westward-propagating disturbance. At the end of the paper, I have no better idea of what is it and why it might be significant than I had at the start.
Response: [Page 2 line 22-24, in change-track-mode revised manuscript] In the introduction section, we added:
"The planetary waves at-least include Rossby wave, Kelvin wave and west-propagating disturbance (Rhines, 1975; McCreary, 1985). Specifically, the vertical structure of the west-propagating disturbance needs further investigation in NWIO (Maximenko et al., 2005)."

Page 9, line 26. When comparing the observationally derived geostrophic current and

the current fields from the re-analysis products, have the authors considered that the current fields in the re-analysis products might contain ageostrophic components, eg Ekman flow?
Response: [Page 11 line 17-22, in change-track-mode revised manuscript] In section 3.3, we added a paragraph on discussion the Ekman flow:
"Part of the difference between observation-based absolute geostrophic current and re-analysis current is due to the near-surface Ekman current. The climatological monthly mean mixed-layer depth in May is roughly 20 m at station 9.5°N and 59.5°E (on CR; Liu et al., 2018). Besides, the mean surface Ekman speed, which is approximately the difference between surface geostrophic current and in-situ surface current (from surface drifter), is within 0.1 m/s in northern IO (Saj, 2017). Therefore, the near-surface Ekman currents in reanalysis datasets are relatively weak to affect the main results as mentioned above."

Page 10, first paragraph. I do not understand the argument that this paragraph is trying to make. There are several ideas that are not fully explored and which are insufficiently referenced. The authors seem to be contrasting "customary ventilation theory" and its corresponding meridional flow with "potential vorticity analysis" and its corresponding zonal flow. This strikes me as being a massive oversimplification; at the least, it requires a much more detailed explanation. Much of this material " as well as the extra explanation I would like to see added " probably belongs in the introduction.
Page 10, line 1. In what way are explanations of ASHSW and RSW pathways "ambiguous". This statement needs to be referenced, evidenced and more fully explained, either here or in the introduction.
Response: [Page 2 line 10-16, in change-track-mode revised manuscript] We modified the explanation, as
"For instance, three water masses were defined in NWIO as Arabian Sea High-Salinity Water (ASHSW), Persian Gulf Water (PGW) and Red Sea Water (RSW). Regarding the pathways of these water masses, the mechanics are not clear. RSW is formed near the northern side of the NWIO; therefore, according to the customary ocean ventilation

theory, RSW sinks and moves southward along the isopycnal layer from the generation zone following the wind-driven current (Luyten et al., 1983). However, the feasibility of the ocean ventilation theory is still unknown for the northern IO, whose meridional extent is limited compared with the other two basins. In contrast, in situ potential vorticity analysis on RSW reveals that the flows generally follow the zonal direction (Beal et al., 2000)."
We moved the paragraph into the Introduction Section.

Page 10, line 5. This sentence is poorly expressed.
Response: [Page 2 line 14-15, in change-track-mode revised manuscript] We changed "However, the feasibility of the ocean ventilation theory is still under debate, especially for the northern IO, whose meridional extent is limited compared with the other two basins. "
into
"However, the feasibility of the ocean ventilation theory is still unknown for the northern IO, whose meridional extent is limited compared with the other two basins."
We moved the paragraph into the Introduction Section.

Page 10, second paragraph. The authors should explain clearly why they break up the particle tracking results into latitudinal bands? is this because they suspect different processes/currents are causing differences in circulation between these bands? Also, this paragraph should make some attempt to elucidate these processes and to explain what's new and important about these results. At present, the text just explains what the reader can already see in the figure.
Page 10, line 14. I do not agree that this looks like flow in the summer Somali Current.
Response: [Page 11, line 35 – Page 12 line 15, in change-track-mode revised manuscript] We revised the paragraph. We added:
"For better describing the trajectories, we separate the CR to three latitude bands as 2.3-5°N (equatorial band), 5-8°N and 8-9.8°N."
"In the equatorial band (Fig. 6e), the near-equator tracers come from the west side,

which is consistent with the north branch of East African Coastal Current during summer monsoon (Schott and McCreary Jr., 2001; Schott et al., 2009)."
"Meanwhile, for the relatively north-side tracers in equatorial band, the trajectories backtrack to east side, which is probably following a westward current or meso-scale eddy."
"For latitudes from 8-9.8°N (Fig. 6a), the trajectories emphasis the north branch of East African Coastal Current, meanwhile, the water at the north station of CR comes from the northeast side, and one station water shows the cross-equatorial current around 53°E (east of Southern Gyre; Schott et al., 2009). "
Accordingly, we deleted:
"For the tropical band (Fig. 6e), the water mainly follows the zonal movement, but the near-equator tracers are from west side and relatively north-side tracers come from east side."
"For latitudes from 8-9.8°N (Fig. 6a), the trajectories look like the flow of the summer Somalia Current (Schott et al., 2009)"

Page 10, third paragraph. Again, some discussion of processes is needed here.
Response: [Page 12, line 16-27, in change-track-mode revised manuscript] We rewritten the paragraph. Original version:
For the RSW in the intermediate-depth layer at 700 m, the trajectories in the tropical band (Fig. 6f) and at latitudes from 5-8°N (Fig. 6d) generally follow the zonal movement (Beal et al., 2000). The tracer movements at latitudes from 8-9.8°N (Fig. 6b) partly agree with those of the ventilation theory, and partly follow the zonal direction (Beal et al., 2000).
New version:
For the RSW in the intermediate-depth layer at 700 m, the trajectories in the equatorial band (Fig. 6f) show the zonal movement. Most of the trajectories move from the west side, which coincident with the potential vorticity explanation (Beal et al, 2000), that the RSW moves southward along the coast with the help of winter monsoon, and then leave the coast and shift to middle ocean via zonal movement. Other two trajectories

come from east side, and one extra trajectory moves from northwest with circular track. Hence, these trajectories display three kinds of pathways. Accordingly, in the 5-8°N band (Fig. 6d), the mainly eastward zonal movements agree with (Beal et al., 2000), meanwhile, some westward trajectories resemble the ventilation theory (Luyten et al., 1983; Qiu and Huang, 1995). At last, in the 8-9.8°N (Fig. 6b), new pathway directly from northwest is emerged, and the trajectories support 700 m waters are probably directly from east of the Horn of Africa (or Gulf of Aden) without southward movements along the coast.

Page 10, line 25. Are there papers or technical reports available that explain methodological differences between the HYCOM and SODA re-analyses? Would more thorough research negate the need for speculation?
Page 10, line 27. It is not clear to me what is meant by the "dynamic core" of the ocean general circulation model.
Response: [Page 13, line 3-14, in change-track-mode revised manuscript] We nearly rewritten the paragraph. We changed
"We speculate that although both SODA and HYCOM assimilate the Argo data into an Oceainc General Circulation Model (OGCM), the methodology of assimilation or the weight between OGCM and in situ observations, is sharply different. We assume the southward extension of ASHSW could be simulated by the dynamic core of OGCM, and the phenomenon was not captured in the Argo-only observation, therefore, HYCOM seems more to approach the dynamic model, and SODA weighted more on the Argo-only observations. Additionally, the finer horizontal resolution of HYCOM likely helps HYCOM involve more physically sound mechanics, such as the downwelling of salty water and wind-driven meridional movement."
to
"Although both SODA and HYCOM assimilate the Argo data into Oceainc General Circulation Models (OGCMs), the assimilation methods of SODA and HYCOM are considerably different. SODA adopts optimum interpolation (Carton et al., 2008), while HYCOM uses 3D variation scheme. One advantage of 3D variation scheme versus

optimum interpolation is the conservation of dynamical constrains (Zhu et al., 2006; Yin et al., 2012; Edwards et al., 2015). Therefore, HYCOM probably describes better on the wind-driven circulations, monsoon-induced coastal current and meso-scale eddy movement, which are all related to the southward extension of ASHSW."

Page 10, line 33. I dislike the description of geostrophic current as an "alternative result for the ocean current". Geostrophic flow is an important part of ocean circulation and is perfectly valid in its own right.
Response: [Page 13, line 15-17, in change-track-mode revised manuscript] The sentences were deleted.

Page 11, conclusions section. The particle tracking results are not mentioned in the conclusion.
Response: [Page 14, line 14-18, in change-track-mode revised manuscript] We added paragraph in conclusion section, as
"To explore the pathways of ASHSW and RSW during the expedition time, we set tracers in SODA dataset at depths of 100 and 700 m, and backtrack their trajectories via three dimensional Lagrangian description. Overall, for the 100 m depth waters, the results reveal the pathways related to the north branch of East African Coastal Current and the flow from northeast side (or Arabian Basin), while for the 700 m depth waters, the trajectories mainly follow the zonal direction from either west and east sides. The results give direct-viewing descriptions and call for further dynamical investigations."

Page 11, line 2. The authors have not discussed baroclinic modes in the results section, so it does not make any sense to the reader when the concept is introduced in the discussion section. Furthermore, are the authors certain that baroclinic mode is an appropriate concept in this instance?
Response: [Page 13, line 18, in change-track-mode revised manuscript] We changed the word "baroclinic" into "vertical".

Page 11, line 11. It is not correct to say that you integrate density to get the geostrophic

current.
Response: [Page 14, line 1, in change-track-mode revised manuscript] We changed
"we integrate the density field to obtain the absolute geostrophic current."
into
"we compute the absolute geostrophic current based on the density profiles and sea surface height".

Page 11, line 20. I do not think that the authors have shown how "the present analysis shows potential data applications for the future".
Response: [Page 14, line 9-11, in change-track-mode revised manuscript] We changed
"The present analysis shows potential data applications for the future, where the meso-scale eddies are relatively important but cannot be well described by the Argo-only data source."
into
"Over the NWIO, the meso-scale eddies are relatively important but cannot be well described by the Argo-only data source. The present analysis shows more data source for potential data assimilation experiment."

Page 14, Figure 3. Using km as the horizontal co-ordinate is unhelpful, given that in the text what is actually interesting is the location in degrees north. The authors should also mark on features of interest discussed in the text, such as the eddies.
Response: [Page 18, Figure 4, in change-track-mode revised manuscript] We added the latitude in the Figure.

Page 16, Figure 5. Given that the westward-propagating disturbance is so little discussed, I do not think that this figure adds anything to the paper.
Response: We removed this figure.