Point to Point answers of the Reviewer's comments

No: CL7528

(Name: Radharani Sen ROLL NO: 14CL92R04)

1) Response to Foreign Reviewer's comments:

General Comment: This PhD dissertation is well written and is supported with on peer-reviewed publication and two are in review/preparation. I strongly recommend the award of Ph.D. Degree by Indian Institute of Technology to Ms. Radharani Sen (14CL92R04). Congratulations to, Radharani (student), Prof. Arun Chakraborty (Supervisor) and Dr. P.A. Francis (Joint Supervisor). "I recommended that this thesis be accepted for Ph.D. degree in the present form".

Answer: Thank you very much for appreciating this work, providing very useful comments and constructive suggestions to improve the quality of the thesis. All the suggestions have been addressed and incorporated in the revised thesis.

Other Comments:

Comment 1: Narrative structure/grammar could have used improvement. Formatting of paragraphs, etc. needs to be double checked.

Answer 1: Thanks for your valuable suggestion. In the revised thesis, all the grammatical mistakes and other formatting mistakes have been corrected.

Comment 2: "There are no detailed studies on the changes in volume transport or continuous survey on the monthly/seasonal transport of the BoB western boundary currents". A few things here. First, the EICC is the only western boundary current in the BoB, it's just seasonally reversing. Second, how sure is the author that absolutely no studies have studied EICC transport variability? Need justification, because lot of studies are there on EICC.

Answer 2: Thanks for this comment and suggestion. In the revised thesis, the sentence "There are no detailed studies on the changes in volume transport or continuous survey on the monthly/seasonal transport of the BoB western boundary currents" has been corrected (page no. 2-3).

There are plenty of studies available on the EICC volume transport in the BoB but, these studies are based on in-situ or observed data. Moreover, these studies cover specific period or season. There are no continuous survey on the EICC volume transport on the seasonal timescale using high-resolution ROMS simulation.

Comment 3: "The primary purpose of this thesis is to find out the seasonal transport variability of the EICC and to determine dominant oscillations". I find it hard to believe that this is completely new.

Answer 3: Thanks for this comment. In 2nd part of answer 2, it has been explained in detail how the seasonal transport variability of the EICC is different from other previous studies.

Again, McPhaden et al. (2009) reported that in the Indian Ocean (IO), the intraseasonal oscillations of temperature in the thermocline region are maximum using RAMA data. In this work, it has been checked whether high-resolution numerical simulation able to capture strong intraseasonal oscillations in the BoB. To find out the dominant frequencies in the BoB, FFT analyses are done on four parameters D23, MLD, ILD, and OHC.

Comment 4: Climatological river runoff is included in the model "to improve salinity simulation in the BoB" but there are studies showed that you need more than climatology to accurately depict salinity in the northern Bay, and given that the author wants to claim they're studying intraseasonal variability, I find it difficult to believe that including climatological river runoff is going to do much to help.

Answer 4: Thanks for this comment. The ROMS simulation output used in the 3rd chapter is entirely different from the interannual ROMS simulation used in chapters 4th and 5th of the thesis. In the 3rd chapter of the thesis, the EICC volume transport is analyzed by the ROMS output forced by the climatology of the river runoff fields. In the 4th and 5th chapters of the thesis, the interannual ROMS simulation, where SSS is relaxed to climatology, has been used to analyse the intraseasonal thermocline variability and its impact on the upper ocean.

Comment 5: Figures 1.1 and 1.2 are redundant. Both don't need to be there.

Answer 5: Thanks for this comment. Figures 1.1 and 1.2 of this dissertation work describes the "domain of this study" and the surrounding countries of this basin respectively.

Comment 6: Figure 1.13 was clearly not made using daily data. Section 1.6 completely ignores many of the major ISOs which occur on timescales shorter than 30 days

Answer 6: Thanks for this comment. Figure 1.14 is the wavelet of daily AVISO SSHA data averaged over the BoB (page no. 22). Following Girishkumar et al. (2013a), in this thesis, intraseasonal period within 30-100 day band has been considered.

Comment 7: No description of ISO dynamics is provided other than the time period and a few wavelets, which is insufficient.

Answer 7: Thanks for this suggestions. In chapter 1 of the revised thesis, the ISO dynamics have been mentioned (page nos. 20-22).

Comment 8: Section 2.3.6 claims to describe bandpass filtering but it's one sentence and it's completely unhelpful.

Answer 8: Thanks for your comment. In the revised thesis, bandpass filtering is explained in detail (page no. 29).

Comment 9: Figure 3.1 is stretched and doesn't look good.

Answer 9: Thanks for your comment. In the revised thesis, Figure 3.1 has been modified.

Comment 10: "The possible reason has already been discussed in the last paragraph".

Answer 10: Thanks for this comment. In the revised thesis, this sentence has been modified (page no. 55).

Comment 11: ROMS SSS is at least 1 psu off from WOA for the entirety of the SW monsoon season. That is not good.

Answer 11: Thanks for this comment. This is due to the presence of salinity bias in the model. The climatological ROMS derived SSS shows approx. 1PSU difference with WOA SSS from July to September. In the 3rd chapter of the thesis, EICC volume transport has been analyzed. The equatorward EICC is seen during October to December. Thus, the salinity bias during July-September does not affect southward EICC transport.

Comment 12: Figure 3.5 is illegible.

Answer 12: Thanks for this comment. Figure 3.5 is modified in the revised thesis (page no. 59).

Comment 13: Figure 3.6. authors combine zonal and meridional transports so it's completely useless as a figure.

Answer 13: Thanks for this comment. Figure 3.6 is the spatial structure of the zonal and meridional currents' transport integrated from the surface to 800 m depth. From this figure one could easily understands how much EICC transport throughout the year.

Comment 14: Transport calculation results need to have been better reported than what was shown. Interesting results were likely obtained, but the manner in which they were presented did not represent the findings to a satisfactory degree.

Answer 14: Thanks for this comment. The suggestion is taken care of in the revised thesis.

Comment 15: Figure 3.9 is a squished, blown up mess.

Answer 15: Thanks for your comment. In the revised, Figure 3.9 has been modified.

Comment 16: Labeling of figure boxes is inconsistent in Section 3.3 is inconsistent which makes interpretation of results difficult.

Answer 16: Thank you for the comment. In the revised thesis, the labeling of Figures 3.12 and 3.13 have been corrected, which explain section 3.3.

Comment 17: Section 3.3 is out of place in chapter 3 and has extremely little to do with EICC volume transport.

Answer 17: Thank you for the comment. In the thesis, the EICC volume transport analysis is the major work of chapter 3. Section 3.3 of chapter 3 explains that in the BoB, the intraseasonal fluctuation is the strong oscillations. Since there are strong intraseasonal signals in MLD and D23, this aspect has been investigated in subsequent chapters.

Comment 18: Figure 4.1 is impossible to read and is squished.

Answer 18: Thanks for your comment. In the revised thesis, Figure 4.1 has been modified.

Comment 19: Color bar chosen in Figure 4.8 is not good and would be invisible to someone who is colorblind.

Answer 19: Thank you for this comment and suggestion. In the revised thesis, the colorbar of Figures 4.8, 4.10, 4.13, and 4.16 has been changed (page no. 82, 85, 87, and 91).

Comment 20: Authors spend a great deal of time in Chapter 4 examining 30-70-day thermocline variability, and include SSHA, and D23 variability, but spuriously attribute this to planetary waves and Ekman dynamics without any supporting information or actual dynamics involved. The actual ISO dynamics are never discussed, just vaguely insinuated, which is insufficient for a dissertation.

Answer 20: Thank you for this suggestion. In the revised thesis, the actual ISO dynamics has been explained in detail (page nos. 20-22).

Comment 21: Figure 5.1 is redundant, and it is also unclear why bathymetry is used in this figure.

Answer 21: Thank you for your comment and suggestion. In the revised thesis, the bathymetry plot has been removed.

Comment 22: Many figures in Chapter 5 seem to be redundant based on figures in previous chapters.

Answer 22: Thank you for this comment. In the revised thesis, all the repeated figures, shown in chapter 4, have been removed.

Comment 23: On page 102, authors admit that "since the model simulation has been carried with SSS relaxed to climatology, ROMS is unable to capture the accurate salinity trend at R15". This is the exact reason that MLD, etc. is wrong on intraseasonal timescales and I've been saying that since the introduction.

Answer 23: Thanks for this comment. The ROMS simulation output used in the 3rd chapter is entirely different from the interannual ROMS simulation used in chapters 4th and 5th of the thesis. In the 3rd chapter of the thesis, the EICC volume transport is analyzed by the ROMS output forced by the climatology of the river runoff fields. In the 4th and 5th chapters of the thesis, the interannual ROMS simulation, where SSS is relaxed to climatology, has been used to analyse the intraseasonal thermocline variability and its impact on the upper ocean.

Comment 24: This is a dissertation. "Example" and "with respect to" should both be fully spelled out, not written in shorthand.

Answer 24: Thank you for this comment and suggestion. In the revised thesis all these mistakes have been corrected.

Comment 25: There should be a space between a depth measurement and meters. (Ex: 50 m not 50m).

Answer 25: Thank you for this suggestion. In the revised thesis, a space is given between a depth measurement and meters.

Comment 26: Authors claim that MLD is in good agreement, but then say that r = 0.10 and RMSE is 17.77 m, which is not good agreement.

Answer 26: Thanks for this comment. Despite weak correlation and high RMSD of simulated MLD with RAMA, simulated MLD shows good matching with observed MLD during the summer (2013 and 2014), fall (2013 and 2014) and early winter season (November-December 2014) at R15. Simulated MLD does not match with the observation in late winter (January-February 2013, the second half of February 2014), in spring (March-April 2013, March-first half of April 2014), and in December 2013.

Comment 27: Chapter 5 results suggest that ROMS captures the seasonal cycle reasonably well, but intraseasonal and daily variability performs very poorly, which does not make this model useful for ISO studies in this region, except maybe SST, but even then you could just use a satellite instead.

Answer 27: Thanks for this comment. In chapter 4 and 5, model's error in capturing intraseasonal fluctuations have been pointed out and the possible mechanisms behind these errors.

Comment 28: Figure 5.10 is so distorted it's difficult to read.

Answer 28: Thank you for this comment. In the revised thesis, the Figure 5.7 has been modified (page no. 110).

Comment 29: The listed major contributions/conclusions section: impactful results need to be clearly spelled out.

Answer 29: Thanks for this comment. This suggestion has been taken care of in the revised thesis.

Comment 30: Few recent ISO papers need to be cited.

Answer 30: Thank you for this suggestion. In the revised thesis, the ISO related papers have been cited.

Response to Indian Reviewer's comments:

General Comment: The present thesis focusses on studies on seasonal variations of EICC, thermocline variability at intrasesaonal scale and finally talks about the impact thennocline variability on MLD and SST in the Bay of Bengal. For this purpose, researcher has used ROMS model and forcings from ERA and NCMRWF .For the validation, RAMA buoy data have been used. The scientific questions addressed by the researcher are very pertinent and needs detailed investigation. In that context, the work carried out in the present thesis is very apt and enhances the scientific understanding.

Following are the major strengths of the present thesis

1. Volume transport estimation associated with EICC using state of the art model and investigation of the associated dynamics with it is the high point of the thesis and fills up the missing gaps in the understanding at seasonal scale.

2. Very in depth analysis of thermocline ISV in Bay in relation to forcings vis a vis remote effect is yet another highlight of the thesis.

Answer: Thank you very much for appreciating this work, providing very useful comments and constructive suggestions to improve the quality of the thesis. All the suggestions have been addressed and incorporated in the revised thesis.

Comment 1: There is a need to strengthen the abstract. I advise the researcher to kindly mention 1/2 salient findings w.r.t each of the problem investigated in the study. As mentioned by the student, there are 3 objectives that she has addressed in her work. Hence it would be apt to focus specifically on these three aspects.

Another aspect, which needed to be talked, is about how model simulations of salinity can be improved, by data assimilation or by changing mixing physics? Satellite derived SSS is available over a long period now. How can you make use of that to diagnose the issues with salinity simulation on a basin scale? These aspects should have been mentioned as a part of future work.

Answer 1: Thanks for your comment and suggestion. The suggestions has been taken care of in the revised thesis.

Comment 2: In chapter #2 description on Methodology, it is advised to write Methodology and Computations. Thermocline depth, MLD etc cannot be methodology. Please correct accordingly.

Answer 2: Thanks for your comment and suggestion. In chapter 2 of the revised thesis, the "Methodology" section is replaced by "Methodology and Computations" (page no. 35).

Comment 3: Page# xvii Are the model setup different for climatological and intraseasonal variability studies? I guess only the forcings are different. That doesn't mean the setups are different.

Answer 3: Thanks for your comment. The model setups are different for climatological and intraseasonal ROMS simulation.

Comment 4: What is the difference between Conclusions and Concluding remarks? Both can be combined, if it sounds fine.

Answer 4: Thanks for your suggestion. In the revised thesis, section title of chapter 6 has been modified (page nos. xviii, 119-123).

Comment 5: Figure 1.3 Is this monthly wind from QuickSCAT for the particular year or mean value constructed over many years?

Answer 5: Figure 1.3 is the climatology of wind. For making climatology, the monthly QuickSCAT data were averaged from January 2000 to December 2008 (page no 35).

Comment 6: Figure 1.8 These currents are for which year? Is it computed using gridded data of TOPEX from AVISO? Another query is, has the researcher used SSH or sea level anomaly SLA? This is because normally from altimeters, one uses SLA and not SSH due to inaccuracy in the geoid.

Answer 6: Figure 1.8 is the climatology of current and it is computed using gridded data of TOPEX from AVISO. In the revised thesis, the caption of Figure 1.8 is corrected (page no 13).

Comment 7: Figure 1.13 Has this figure been taken from some published work? Please mention what the black curve indicates? Intraseasonal bands are significant at what level? Please mention this in all the wavelet figures. Also, it will help if the identified ISV bands are explained in terms of some physical processes associated with them.

Answer 7: Thanks for the query. Figure 1.13 has not taken from any published work. The black line of Figure is the cone of influence (COI). It is the region of the wavelet spectrum in which edge effects become important. This e-folding time is chosen so that the wavelet power for a discontinuity at the edge drops by a factor e−2 and ensures that the edge effects are negligible beyond this point (A. Grinsted, J. C. Moore, and S. Jevrejeva 2004, Application of the cross wavelet transform and wavelet coherence to geophysical time series, *Nonlinear Processes in Geophysics*, SRef-ID: 1607-7946/npg/2004-11-561.) Intraseasonal bands are significant within 30-100 day bands. The physical processes associated with ISV bands are explained elaborately in chapter 4 of the revised thesis.

Comment 8: Section# l .7 It is not clear how seasonal variability of EICC will be beneficial for fishing zone identification. Suggest to downplay this aspect. I also see some mismatch in the three objectives outlined later and three scientific questions outlined in this section, specifically with respect to i.

Answer 8: Thanks for this query. The strong anticyclonic gyre modulates the northward-flowing EICC during the pre-monsoon season. Anticyclonic circulation is associated with upwelling. Nutrient-rich water uplifts in the ocean's surface layers due to the substantial upwelling, which is a suitable situation for a strong fishing zone.The proposed objectives and scientific questions in chapter 1 have been modified in the revised thesis.

Comment 9: The scientific questions that are posed and objectives outlined are not in complete sync. While the objective#l talks about seasonal EICC transport, the same is not identified in the science question. For the future work, it would be good to use daily satellite current which are available from www.mosdac.gov.in.

Answer 9: Thanks for this suggestion. Section 1.7 of the revised thesis has been modified (page no. 25). Since satellite current data are available only in the surface level, it is not possible to calculate transport of EICC, which extends down to ~800 m depth.

Comment 10: What depths are considered as h1 and h2 in eqn 2.5?

Answer 10: Thanks for this query. In this thesis, the oceanic heat content is calculated from the surface to 100 m depth. In equation 2.5, h1= 0 (surface), and h2= 100 m.

Comment 11: It is suggested to show gradient profile to confirm that 23 Deg C isothenn is proxy for thermocline.

Answer 11: Thanks for your suggestion. It is shown in chapter 2 (section 2.3.7, page no. 37, Figure 2.2, page no. 38).

Comment 12: Any reason why the model setups are different for climatological and inter annual runs? In4th chapter, HOOFS is used and in 5th chapter ROMS is used.

Answer 12: Thanks for the query. In chapter 3 climatological ROMS setup is used. In 4^{th} and 5^{th} chapter of the thesis, interannual (HOOFS) ROMS setup is used for the analysis of intraseasonal thermocline depth, MLD, and SST.

Comment 13: Section 3.2.1 Nowhere in the text it is mentioned whether it is climatological comparison if yes, then what is the period of model run or for a particular year? Needs to be mentioned here again, even if it has been mentioned in previous chapter. How was the climatology generated from Argo and AVISO? Can you please talk about reason for differences in Model simulated SSH and AVISO SSH? What is the reference surface in model? In AVISO, if you are taking SSH, it is reference ellipsoid.

Answer 13: Thanks for your suggestion. I have modified section 3.2.1 of the revised manuscript (page no. 54). The information of the observed data used in chapter 3 are mentioned in chapter 2 (page no. 30-35).

Comment 14: Figure#3.4 It is suggested to mention, why correlation is poor with Argo and so good with WOA. Good correlation with WOA is due to model being initialized with WOA clunatology? Please dwell on this.

Answer 14: Thanks for your comment. This suggestion is taken care of in the revised thesis (page no. 56).

Comment 15: Figure#3 .6 Transport is computed over what depth?

Answer 15: Thanks for your query. The EICC volume transport is calculated calculated in the top 800 m of the water column.

Comment 16: Figure#3.9 These correlations are significant at what level? Also, contour labels should have been smaller in size for clarity. Why negative correlation between WSC and transport signifies no correlation, as stated in the thesis. Negative correlation rather indicates the direction in which two parameter vary.

Answer 16: Thanks for your query and suggestion. Figure 3.9 is modified in the revised thesis. It shows correlation at 99% significance level. Figure 3.9 is explained in chapter 3 of the revised thesis (page no. 65).

Comment 17: Figure#3 .12 and 3.13 Is there any connection between 30 100 days ISV in MLD, ILD and seasonal transport due to EICC? Or, it should be stated that since there are strong ISV signals in MLD, ILD etc, we investigate this aspect in subsequent chapter. This way connectivity between chapters are more logical.

Answer 17: Thanks for your suggestion. Section 3.3 of the revised thesis has been modified (page no.67).

Comment 18: Chapter #4 It is stated that Primary aim of to assess the sensitivity of ROMS to different atmospheric forcing. However, as per objective#2, the primary goal is to study the ISV of thermocline. The statement can be modified accordingly.

Answer 18: Thanks for your comment. In chapter 4, the analysis of ROMS-NCMRWF (ROMS forced by NCMRWF atmospheric forcing data) simulated thermocline in the intraseasonal timescale has been done. Before doing this, it has been checked how well ROMS-NCMRWF captures upper ocean parameters by comparing with ROMS-ERA5 (ROMS forced by ERA5 atmospheric forcing data). Since, ROMS-NCMRWF simulates better upper ocean parameters compared to ROMS-ERA5 next, the analysis of the intraseasonal thermocline variability has been continued with ROMS-NCMRWF simulated output.

Comment 19: Section#4.2.1 Please mention the analysis period.

Answer 19: Thanks for this suggestion. Analysis period is mentioned in section 4.2.1 of the revised thesis (page no. 74).

Comment 20: Section#4.2.1 Thennocline ISV in the equatorial Indian Ocean is primarily driven by wind Pl see Mankad et al., 2014. Since Bay is largely remote forced and partly locally forced, it is quite possible that in Bay too, local wind is responsible for thermocline variability. In your case too, better wind field from NCMRWF is responsible for better simulation. You can emphasize on this as equatorial RAMA buoy data also has been used in the present thesis.

Answer 20: Thanks for this comment. This suggestion is taken care of in the revised thesis (page no. 71).

Comment 21: Chapler#5 Please refer to Agarwal et al 2007, where they have studied 10 20 day ISV in SST is governed by net heat flux and vertical diffusive mixing and essentially it is 1 d process.

Answer 21: Thanks for this comment. In chapter 5 of the revised thesis, Agarwal et al., (2007) has been cited (page no. 99).

Comment 22: Since different model runs are being used in different chapters, it would be good to know which model performs better in Bay. This can be answered during defense.

Answer 22: Thanks for this comment. In the first objective (3rd chapter of the revised thesis), climatological ROMS output has been used. In 2nd and 3rd objectives (4th and 5th chapters of the revised thesis), interannual ROMS simulated output has been used. The interannual ROMS-NCMRWF (ROMS forced by NCMRWF data) simulated output shows better results in BoB than the ROMS-ERA5 simulation.

Comment 23: Figure#5.5 Since ROMS fail to capture any variability in SSS, it's impact on MLD is clearly seen in Figure 5.6 at RI 5, where simulated MLD is much deeper as compared to RAMA. This should be mentioned and discussed. Poor performance of model SSS is only because of SSS relaxation, or it could be due to improper prescription of vertical Mixing? I think later has a larger role to play.

Answer 23: Thanks for this comment. The performance of simulated MLD at R15 has been extensively discussed in sections 5.2.2 and 5.2.3 of the revised thesis.

Comment 24: Figure#5 .8 I think it is better to add simulated word in the caption.

Answer 24: Thanks for the comment. This suggestion has been taken care of in the revised thesis.

Comment 25: Figure#5 .13 What is sub surface processes which is defined in the unit as DegC day? Is it rate of change of sub surface temperature? If yes, then what depth or integrated over what depth?

Answer 25: Thanks for the query. Subsurface processes is calculated using equation 2.11 mentioned in chapter 2 of the thesis. In this equation, T_{-h} indicates temperature integrated up to MLD and T_{ml} indicates temperature at MLD. It is the rate of change of temperature.

Comment 26: Please check page# 122 and 138. Something is missing.

Answer 26: Thanks for the comment. These two pages have been kept blank.