



EGUsphere, referee comment RC1
<https://doi.org/10.5194/egusphere-2022-453-RC1>, 2022
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Comment on egusphere-2022-453

Anonymous Referee #1

Referee comment on "Seasonal variation in eddy activity and associated heat/salt transport in the Bay of Bengal based on satellite, Argo, and 3D reprocessed data" by Wei Cui et al., EGUsphere, <https://doi.org/10.5194/egusphere-2022-453-RC1>, 2022

This manuscript uses altimetry, Argo and re-analysis data to identify eddies in the Bay of Bengal (BoB), map their trajectories, and estimate their heat and salt transport. The authors consider the behaviour of cyclonic and anti-cyclonic eddies separately, as well as separately considering the behaviour of eddies in five sub-regions of the BoB. They find that pronounced eddy-driven heat and salt transport occurs in those parts of the BoB where eddies are most prevalent. I think that these results are suitable for publication – however, the manuscript lacks the discussion necessary to help the reader understand their significance. Consequently, I recommend major revisions.

The English is generally clear, but there are a few sentences that I struggled to understand, as well as a few minor mistakes (e.g. singular/plural verbs). I would recommend having the revised manuscript proof-read by a native speaker if at all possible.

Comments

In its present form, this manuscript consists primarily of a long (over 400 lines) results section (Sections 3 and 4). I found parts of this quite difficult to follow and fairly repetitive: I would encourage the authors to consider whether all of this material is strictly necessary, and whether much of it couldn't be summarised more effectively. And to expand on my comment above, the sheer length of the results leaves no room for discussion. The reader is therefore unable to decide whether any of the results presented are actually important. Statements such as that starting on line 649 – "knowledge of the seasonal variation and vertical structure of eddies is vital..." – and that at the very end of the manuscript therefore appear to be little more than unsubstantiated assertion. Apart

from some very general statements in the introduction (e.g. lines 26 to 30) nowhere in the manuscript is the need for this knowledge explained, nor do the authors describe how their results make a difference to the field. The authors quantify transport in Section 4 – are these big numbers? Would we expect these transports to make a big difference to the temperature and salinity distributions in the BoB? The meridional salinity gradient of the BoB is particularly pronounced and the re-supply of salt across the BoB's southern boundary has received attention in the past (Vinayachandran et al., 2013, GRL, 40, 1777–1782): do these transports strengthen it or weaken it?

I think that the method needs a little more explanation and justification in places. Section 2.2.2 in particular seemed to be a brief explanation of a complex process. Why were the Argo profiles modified relative to their distance from the eddy? What does "transformed into the normalised eddy co-ordinate space" mean? What are the units of the 0.1-by-0.1 grid? How were the "composites of 3D thermohaline structures" created? Does this use just the argo data, or does it use e.g. re-analysis data? Indeed, why is this seemingly complex method necessary? Wouldn't it be far simpler to just use the closest Argo profile as it is?

Finally, I encourage the authors to provide some more justification of their decision to divide the BoB up into five sub-regions, as outlined in Figure 10. Statements such as "The S2 section is the exchange channel between NB and CB" (line 536) imply, to me at least, that these sections have some sort of geophysical significance. But I cannot find in the manuscript any explanation of why the sections were drawn as they have been. I do appreciate that sometimes we just have to put a line somewhere; but if the authors' choice here is fairly arbitrary, perhaps some discussion of the sensitivity of the results to an arbitrary choice would be welcome?

Line-by-line comments

Line 122. Hydrography should be presented as conservative temperature and absolute salinity.

Figures 2, 4 and 9. I recommend plotting these figures using a diverging colour bar (e.g. red to blue) with zero in the middle.

Line 240. Do the authors mean significantly in the statistical sense? Or do they just mean "a lot"?

Line 243. Throughout the manuscript, the authors appear to use eddy intensity and eddy amplitude interchangeably. I would recommend sticking to amplitude to improve clarity.

Line 250. I wonder whether Table 1 wouldn't work better as a figure? Maybe a series of bar charts?

Figure 4. The arrows on this figure don't show up very well against the coloured background.

Line 309. What do the authors mean by a "relatively concentrated distribution"?

Line 361. This paragraph promises discussion of how the BoB's water masses influence eddy properties, but no such discussion ever appears. Instead, this paragraph is mainly introduction-style material about water mass hydrography.

Figures 7 and 8. The shading representing standard deviation is too faint to see clearly.

Line 420. What do the authors mean by a "concentrated" eddy?

Line 423. "Difficult to cause a large salinity change" doesn't make sense.

Line 455. I don't quite understand what the authors mean by "confused positive salinity anomalies".

Line 483. What do the authors mean by "almost present"?

Line 496. What do the authors mean by "it" in this sentence?

Line 573. I don't know what "monotonous" means here.

Line 603. The word "basically" sounds vague here and should probably be avoided.

Line 706. The comparison with the results of Gonaduwage et al (2019) would make a interesting discussion point. It would be nice to see this expanded.