Review of Ditkovsky et al.

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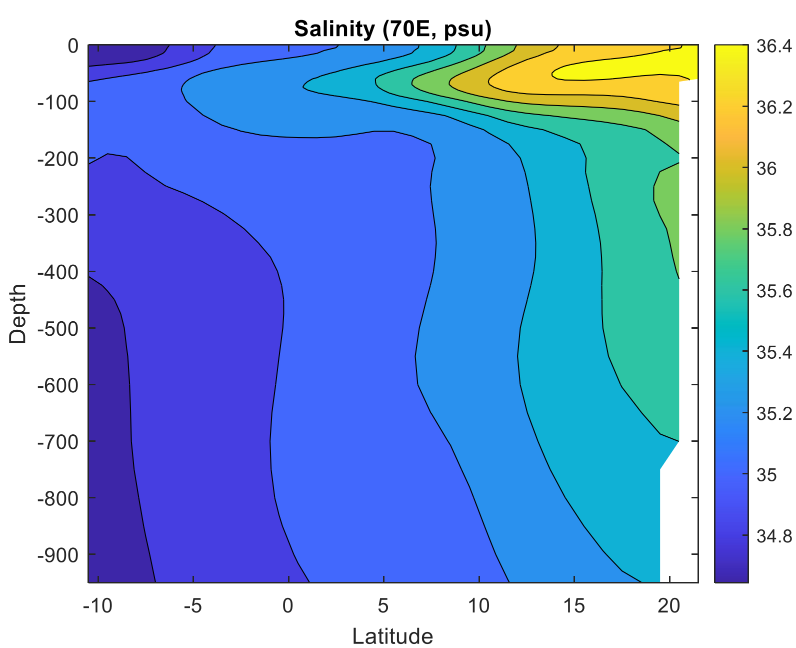
This paper considers drivers of changes in hypoxia in the Indian Ocean, a critical region for artisanal fisheries and one whose behavior under global warming has not been well characterized. The paper finds three regimes of oxygen change, which correspond to three different driving mechanisms for that change. The authors kindly credit me with distinguishing "single-pipe" from a "mixing network" models and describe how this can be used to distinguish the changes in the northern, central, and southern Indian Oceans and how it needs to be updated to do so. Frankly, I think their mapping onto the different regimes is clearer than what I wrote about- this is an elegantly written paper. The argument that there are three separate regimes is generally well made.

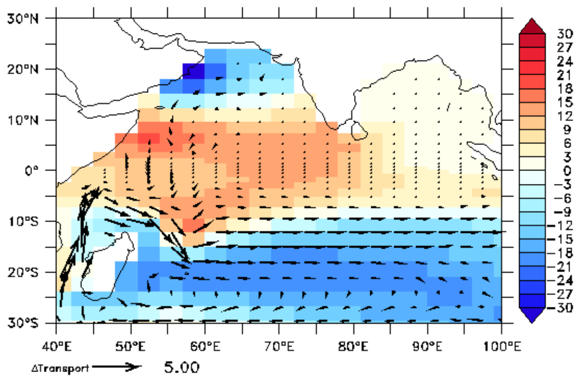
I have three comments, that in some ways parallel those made by the other reviewer. Normally, I would give this a "major" revision since they will require some new analysis, not merely clarification of existing analysis. But I think the basic analysis is sound and don't want to suggest "reconsideration" of the paper, so I'll call this minor.

The first comment regards the role of the overflows. There is a lot more to overflows than the volue that they deliver to the ocean. It's been a problem for a long time in models to get the depths of injection of overflow water correct. If I look at the salinity along 70E (Figure R1), it's clear that there's a signal from high-salinity shelf waters that penetrates for hundreds of m. It is unlikely that models, which generally have problems with numerical entrainment, correctly capture either this process or its sensitivity to changes in climate. This is even true for relatively high-resolution models (see Seddigh-Marvasti et al., 2015 for some discussion of this). It would be good to at least evaluate how much of a problem this is, rather than simply accepting the results of the MMM (cross-sections of the watermass fractions might be useful to look at this and discern whether there are any systematic errors here). This wouldn't need to be an extra figure in the main text but would make a good one in the Supplemental material and would be useful for evaluating whether there are any systematic errors here.

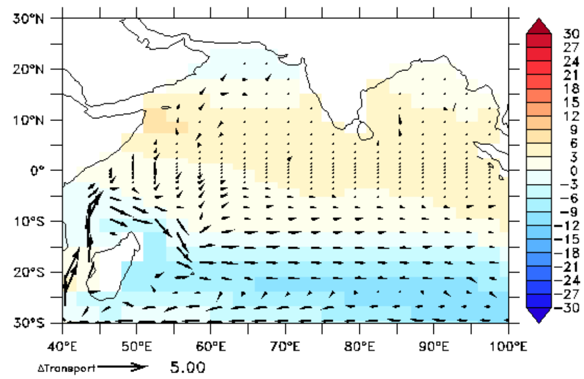


2. The neglect of changes in productivity is understandable, but it I note that it was also picked up by the other reviewer. One way of addressing this is to look at how much of the change in the oxygen can be accounted for by changes in the O2:age slope and how much can be accounted for by changes in the age itself (i.e. to the extent that $AOU = \overline{J_{O2}} * age \rightarrow \Delta AOU = \overline{J_{O2}} * \Delta age + age * \overline{\Delta J_{O2}}$) Showing not just the correlation coefficients but the regression coefficients might help with this.

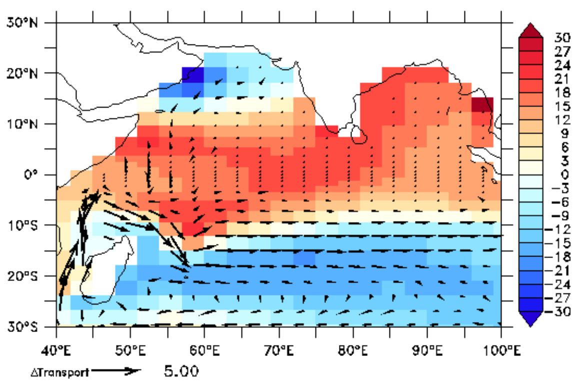
3. Finally, I wanted to see whether this picture seemed to work in my own suite of coarse models reported in Bahl et al., 2019. These models don't have a Red Sea or Persian Gulf, and also fail to generate an oxygen minimum zone in the Northern Arabian Sea (this, incidentally, supports the point of this paper and others that resolving the impacts of such water is important).I show results for two cases with low and high lateral mixing in the figure below. Interestingly, it does seem that the same 3 regimes show up.



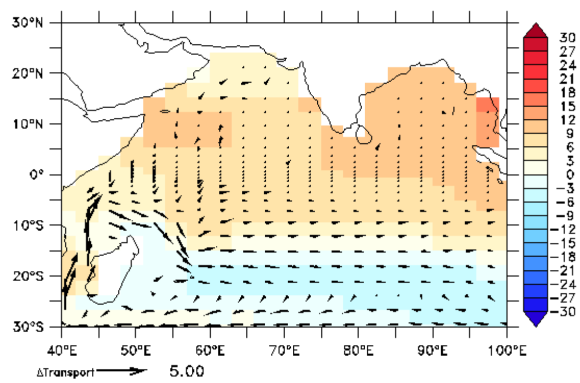
(A) ΔO_2 (mmol/m^3), A_{Redi}=400



(B) ΔO_2 (mmol/m^3), A_{Redi}=2400



(C) $\Delta PO_4^{Remin} * (-150)$ (mmol/m^3), A_{Redi}=400



(C) $\Delta PO_4^{Remin} * (-150)$ (mmol/m^3), A_{Redi}=2400

Since I have a remineralized phosphate tracer in this model I can also directly attribute the changes to changes in accumulated phosphate, and again, this dominates the pattern at low mixing, though somewhat less at high mixing. Ultimately I think this highlights an interesting question of whether the real world Indonesian throughflow acts as a barrier to or enhancer of tracer mixing. It also raises the question of how much of the intermodel variability is due to how this subgridscale mixing is handled.

That the basic framework seems to work in this model suite as well is encouraging and supports the publication of the manuscript.

Note,however, that despite not having marginal seas, we still get a drop in the North. This seems to be driven by a shallowing of mixed layers (echoing something noted by the other reviewer), which reach over 100m on average in the winter in the Northern Arabian Sea in this model, but shallow substantially under global warming. This may be difficult to capture with the watermass analysis alone, as it is not clear (at least to me) that the MMM will necessarily capture the differences between overflow and surface watermasses in this region. It would be worth examining changes in mixed layer depth to see to what extent this plays a role in the more realistic models.

References (which you don't need to add to the paper unless you really want to)

Bahl, A.A., A. Gnanadesikan and M.A. Pradal, Scaling global warming impacts on ocean ecosystems: Results from a suite of Earth System Models , *Frontiers in Marine Science*,7, 698, https://doi.org/10.3389/fmars.2020.00698. 2020

Seddigh Marvasti, S., A. Gnanadesikan J.P. Dunne, A. Bidokhti and S. Ghader, Challenges in simulating spatiotemporal variability of phytoplankton blooms in the Gulf of Oman, *Biogeosciences.* 13, 1049-1069, 2016.