Ocean Science Discussions

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Comment on os-2021-96

Anonymous Referee #2

Referee comment on "Interannual variation of a bottom cold water mass in the Seto Inland Sea, Japan" by Junying Zhu et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-96-RC2, 2021

This paper focuses on the interannual variability of the cold water mass in Iyo-Nada, Japan, using both observations along a transect and three sensitivity runs of a hydrodynamic model (POM). The main conclusion of this study is that it is the heat transport during the warming season (May-July), rather than initial temperature before warming (Dec-Feb) that dominates the cold water mass's interannual variability. In the end, this paper also compares with other coastal cold water masses using idealized equations to explain why the inter annual variability of the INCWM is not dominated by initial temperature in winter.

This is an interesting topic, however, the major conclusion is not successfully delivered through the three sensitivity runs. As Table 1 suggests, the authors increased local and remote air-sea heat fluxes by 10% during the winter (Dec-Feb) and warming (May-July) seasons, in order to evaluate whether it is initial temperature change in winter or heat transports in warming season that dominates the interannual variability of the cold water mass. Design of these sensitivity runs does not separate these two roles at all, thus not convincing at all. Their reasoning is like this, process A + process B will lead to phenomenon C. The authors changed 1.1A kept B the same, and it leads to 1.3C, but the combination of A and 1.1B leads to larger change in C, so B is the dominant role for C, instead of A, which is funny.

For example, authors finally found Case 2 - increase air-sea heat flux by 10% during May-July that brings the largest q and cr(T) values. In this case, the interannual signal in initial temperature is still kept in the model, it is not convincing to conclude as the authors suggested, only if the initial temperature signal on interannual time scale is totally removed. On the other hand, the increase value (10%) is tricky as well. Authors at least need other runs with different increase values to make sure this 10% is not sensitive and the conclusion is still valid. Not to say the results suggested by cr(A) are not supporting, which also need reasonable explanation.

Another confusing and disappointing point is, the authors do use a numerical model to study the inter-annual variability of this cold water mass, but none of heat budget terms from the model is analyzed, which is super crucial for determining which physical process that is mainly contributing. The authors write many texts for the last part (Section 4.2), which is actually not that interesting. Too many idealized assumptions, e.g. no interannual variabilities in rho, R, and H? And the final conclusion of this part is obvious without these texts.

Therefore, this paper is suggested to modify their reasoning method (three sensitivity runs) and the paper structure (too many uninteresting texts in discussion and few analysis about heat budgets). Other comments are listed below.

Major Comments:

- Section 2.1 does not include the time information of the observational dataset. Is it only collected in Jan/Apr/July/Oct each year for all eight stations? It seems not true, because Figure 2b compares between observational datasets and model results on a time scale of multi-year monthly climatology. Also, How many obs. in each month are collected? Do you calculated monthly averages first?
- Figure 2a. In the text, it says the R station has a depth of 75m, but the figure only shows the upper 50m. In the caption, it says the 18°C isotherm is considered to define the studied cold water mass, which is better to be illustrated in the main text, e.g. the method section.
- Section 2.2 Model validation: This paper focuses on the interannual variability of the cold water mass, but only verified it on the multi-year average seasonal cycle (Figure 2b), and concluded that "Therefore, this model is suitable for examining the factors influencing the interannual variation in the INCWM via sensitivity experiments. " More comparisons between obs. and model results are needed to confirm model validation, including episodic and interannual time scales, transections comparisons.
- Does the CONTROL run only have seasonal climatology or actually it is an interannual hindcast of this region? It should be clearly stated in the main text. If it only has seasonal climatology, it is not reasonable to compare with the three sensitivity runs, and evaluate the contributions of air-sea heat flux. If it is actually a hindcast, why authors do not show any interannual comparisons with observations to validate the model? Without validation of this model on interannual time scale, it's really hard to trust and interpret the interannual results from the sensitivity runs. On the other hand, if authors do have a hindcast of this model from 1994-2015 and it shows solid results comparing with observations, it is pretty interesting to quantify the cold water mass properties on interannual time scale using the model as well. Because the observational dataset is only present at 8 stations with very coarse spatial resolution, however, the model has a three dimensional distribution of the cold water mass.
- Ln 136-141: How the f time series is calculated? Is it area-averaged for each domain? How about delta f? Do they have units? If so, please add them to Table 2. The q is the absolute value of the relative change of Temp/Area. Therefore, it is not sure if it is a decrease or an increase in Temp/Area in sensitivity runs. Also, in Table 2, the q value for each case is ground to one value. Is it a multi-year average? More details needed for the calculations.
- Ln 193-194: April is selected as the initial month of the water mass formation, but figures only starts from April. To make the point, authors are suggested to show

correlation panels at least for March as well. Ln95-97: the authors stated that "water temperature in April depended mainly on the cooling process, the initial temperature of the INCWM is likely associated with local air-sea heat flux from winter to early spring", any reference for it?

- Ln 198-199: "correlation coefficient below 10 m is larger in May-July than in April (Figs. 4b-d)" Actually it is not true based on Figure 4. Most of the markers show almost the same values, some even decreases, only two particular markers show the increase.
- Figure 5. The blue dots does not match those in Figure 3. Some dots are not denoted with years. Please add their years in the figure.
- Ln 245-247: This is a really confusing conclusion. cr(A) and cr(T) provide opposite results, which is obviously weird, as T and A are always strongly related, but clearly authors choose to trust cr(T) instead of cr(A). More explanations needed here.
- Ln 253-256: Winds are weak during the warming season, how about cooling season, Dec-Feb? Case 1 should not be small, right? 0.006 and 0.06 are for case 2 and 3?
- Section 4.2 Repeated texts for the different cold water masses. e.g. Ln 275-279 vs. Ln 323-329

Minor Comments:

- Some sentences need to be improved in language., e.g. Ln 49-50: This sentence is suggested to be rephrased: "This study focuses on …".
- Ln 148: 1994-2012 or 1994-2015?
- Ln 169: Figure 3 and caption do not match. It says authors used "18.23°C" as the average temperature, but the figure suggests 18°C for that year. Should it change to "18°C" to match the figure?
- The color scheme of Figure 4 is too similar from 0.45 to 0.75, and the markers are too small. Values where $p>0.05$ are also suggested to show, in order to see the correlation patterns.