Anonymous Referee #2

Referee comment on "Air-sea heat flux during warming season determines the interannual variation of bottom cold water mass in a semi-enclosed bay" 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.

Thanks for your careful reading and comments. Following your comments, we have finished a comprehensive revision on the original manuscript. Below is a point-to-point response.

We conducted a climatological simulation in the study. The two roles (initial temperature change in winter or heat transports in warming season) were not separated in the sensitivity runs because the climatological run did not contain the signals of interannual variability. The target of sensitivity runs about "1.1 process A + process B" and "process A + 1.1 process B" is to and explore the sensitivity of INCWM on air-sea heat flux. As your comment, it is not enough to get the dominant role for C, however, it can test for sensitivity. Though changed by 10% of a factor, we investigate the response of INCWM.

In the old manuscript, we try to quantify the contribution (*cr*) of one factor to interannual variation of INCWM using sensitivity coefficient q and the range of interannual variation of the factor (delta *f*). According to peer reviews and our deep thinking, *cr* has no significant physical meaning and is not suitable to account for the contribution of one factor on interannual variation of INCWM. Therefore, the content about *cr* is removed in the revised manuscript and focus on the sensitivity of INCWM rather than discussing the dominant factor.

After the comparison of different BCWMs with different influencing process for its interannual variation, we tried to explore the relationship among them which is helpful to understand the response of bottom water in coastal seas to climate change from a comprehensive perspective. The idealized derivation helps us to understand theoretically in Section 4.2 and this part is an extension of the study, so we keep this part. The structure of this paper has been reorganized in the revised manuscript.

Major Comments:

1. 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?

Thanks for your comment. We added the observation year for each month in Iyo-Nada and Hayasui Strait during 1994-2015 (Supplementary Table 1) and the relevant information about observation in Section 2.1. We collected the observation data from January to December and calculated monthly average first. In Fig. 2, We add the vertical spatial distribution of monthly average temperature in the revised manuscript.

2. 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.

   Thanks for your comment and suggestion. Observation only has a depth of 75 m at R station, while the deepest observation at other stations are around 50 m and 20 m. To avoid unreliable interpolation, we did not show the water temperature at 75 m of station R. In the Section Introduction, we demonstrated that "The depth of Hayasui Strait is more than 100 m and the water temperature is homogenous throughout the year because of strong tidal mixing (Kobayashi et al., 2006)" to illustrate the vertical water temperature at station R. Also, we added the vertical distribution of multi-year average water temperature at station R in Supplementary Fig. 1 to show the seasonal variation of water temperature at Hayasui Strait in Section 2.1.

   A fixed isotherm is often used as the boundary of a cold water mass, such as 2 °C for Bering Sea Cold Water Mass (Zhang et al., 2012), 10 °C for the Middle Atlantic Bight Cold Pool (Yang et al., 2014) and 10 °C for Yellow Sea Cold Water Mass (Chen et al., 2018). As for INCWM in this study, we can see obvious bottom cold water below 18 °C occupying the central Iyo-Nada in the spatial distributions of temperature in July from 1994 to 2015 in Supplementary Fig. 3. Therefore, we use 18°C isotherm to define the INCWM in the study. We add relevant illustration in Section 2.1 and Section 3.1 in the revised manuscript.

3. 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.

Thanks for your comments. In the study, the model is driven by multi-year averaged daily surface fluxes of momentum, heat and fresh water. The primary aim of using numerical model is to study the response of INCWM to atmospheric changes by sensitivity experiments. Under the circumstances, a climatological model is applicable to do some sensitivity numerical experiments. Therefore, we validated the seasonal evolution of INCWM by the comparison with multi-year averaged observation data. We add information about the model configuration in Section 2.2 to make it clear.

4. 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.

Thanks for your comments. we used climatological forcing (multi-year average) in numerical model rather than an interannual hindcast. We add information in Section 2.2 to make it clear. In the study, we investigated the interannual variation of INCWM using long-term observation for the first time. The model was used for just evaluating the response of INCWM properties to sea surface forcing changes. As you said, we plan to conduct a hindcast to quantify the INCWM properties on interannual time scale detailly as a continuation of this work. Your comments also give us a lot of help. We show the shortcoming of the work and future plans in the last paragraph in Section 5.

For the three-dimensional distribution of INCWM, we add the horizontal and vertical distribution of simulated INCWM based on model results in Section 2.1 and Section 4.1.

5. 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.

   Thanks for your comments. The calculation of q is changed as $q = \frac{T_{case} - T_{control}}{T_{control}}$ to show a decrease or an increase in Temp/Area in sensitivity runs. The q is calculated to demonstrate the relative change of INCWM characteristics between two cases (sensitivity run and Control). It is not a multi-year average. We changed the description in Section 4.1. As answered above, the content about cr is removed in the revised manuscript. As for f, it is only used in the calculation of cr. Therefore, we deleted the content about index (cr) and f in the revised manuscript.

6. 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?

   Thanks for your suggestion and comment. As your suggestion, we added the figures of correlation coefficients from previous December to March in Fig. 5 which have little significant correlation.

   The sentence "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" locates at Ln 195-197. Before April, the water mixes well under surface cooling and increasing wind in the previous winter, the air-sea sea heat controls the process of surface cooling, therefore, the air-sea heat flux is closely related to the water temperature at the early spring. Tsutsumi and Guo (2016) suggested the heat content inside the Seto Inland Sea (except for Bungo Channel and Kii Channel) from January to July mainly depended on air-sea heat fluxes through heat budget. Therefore, air-sea heat flux is an important factor for water temperature in April.

We changed the sentence as "*The water temperature in April is the result of cooling process in the previous winter. Tsutsumi and Guo (2016) suggested the heat content inside the Seto Inland Sea (except for Bungo Channel and Kii Channel) from January to July mainly depended on air-sea heat fluxes. the initial temperature of the INCWM is likely associated with local air-sea heat flux from winter to early spring*".

Reference:

Tsutsumi E, Guo X. Climatology and linear trends of seasonal water temperature and heat budget in a semi-enclosed sea connected to the Kuroshio region. Journal of Geophysical Research: Oceans, 2016, 121(7): 4649-4669.

7. 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.

Thanks for your carefully reading. We have changed the colormap to make the information clear. From the Figure 5 in the revised manuscript, it is clear to show that the correlation coefficient below 10 m in Iyo-Nada (Sta. 1-7) increased from April to May-July, especially May and July. In addition, the significant difference levels gradually enhanced from $0.01<p<0.05$ to $p<0.01$ from April to July.

Correspondingly, we changed the sentence as "*As Fig. 5e-h shown, the correlation coefficient below 10 m in Iyo-Nada (Sta. 1-7) increased from April to May-July, especially in May and July. In addition, the significant difference levels gradually enhanced from $0.01<p<0.05$ to $p<0.01$ during this period, which indicated that heat transport into the INCWM from May to July is also important for the interannual variation of the INCWM in July*." in the second paragraph in Section 3.3.

8. 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.

Thanks for your careful reading. We checked the dots and corresponding years and found two strong years (2006 and 2008) were omitted in Fig. 4. As your suggestion, we added table (Supplementary Table 1) to show strong and weak years of INCWM and replot Fig. 4 and Fig. 7 in the revised manuscript.

9. 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.

   As mentioned above, according to peer reviews and our deep thinking, *cr* has no significant physical significance and is not suitable to account for the contribution of one factor on interannual variation of INCWM. Therefore, the content about *cr* is deleted in the revised manuscript.

10. 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?

    Sorry for the rough description caused you to misunderstand. We mean that all the sensitivity coefficient in the experiments about wind are less than 0.00r and 0.06 for the temperature and area of the INCWM in the old manuscript. In the revised manuscript, considering the presence of essential strong wind in summer in the climatological model, we removed the results of wind experiments, and focus on the results of experiments about air-sea heat flux.

11. Section 4.2 Repeated texts for the different cold water masses. e.g. Ln 275-279 vs. Ln 323-329

    Thanks for your comment. We removed description on Ln 323-329.

Minor Comments:

12. Some sentences need to be improved in language., e.g. Ln 49-50: This sentence is suggested to be rephrased: "This study focuses on …".

    Thanks for your suggestion. We rephrased the sentence as "*In this study, we focuses on the BCWM in Iyo-Nada, which connects the Bungo Channel with the Hayasui Strait (Fig. 1b)*" in the third paragraph in Section 1.

13. Ln 148: 1994-2012 or 1994-2015?

    In the old manuscript, the air-sea heat flux and wind stress provided by Japanese 55 year

Reanalysis (JRA55) are from 1994 to 2012. In the revised manuscript, we removed the relevant content about the index *cr*, so we deleted the information about the JRA 55 dataset which only used in the calculation of *cr*.

14. 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?

    Thanks for your comment. In the old manuscript, we used a red star to indicate 18.23°C but plot at the location of 18°C for facility performance. In the revised manuscript, we changed the red star in Fig. 4 to match 18.23°C.

15. 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.

    Thanks for your suggestion. We have changed the colormap and size of markers in Figure 5, and show the correlation coefficients with p>0.05 to see the correlation patterns.

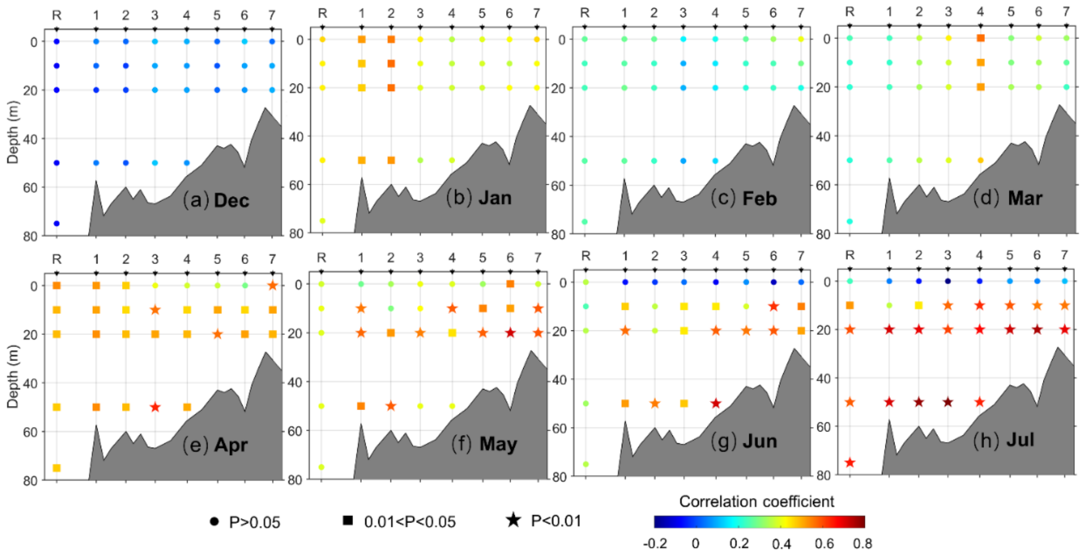

Figure 5: Correlation coefficients for interannual variation of water temperature at each station from previous December to July (a-h) and that of the Iyo-Nada bottom cold water mass (INCWM) mean water temperature in July during 1994-2015. Circles indicate that the significant difference level is less than 0.95, squares indicate a significant difference level is between 0.95 and 0.99, and stars indicate a significant difference level of more than 0.99.