Responses to comments from Reviewer 2

Thank you for thorough and constructive comments. Based on the comments from you and another reviewer, we have revised the manuscript as follows:

>1. In the abstract, the author highlighted that this study also revealed the short-term acidification
>occurs more frequently during low-salinity events yet it is not well explained and emphasized in the
>whole manuscript

>2. Figure 7,8, 10 and etc.. The notations on the x-axis are not immediately recognizable to readers.
>Please change to the Year/Month directly. Also, in Figure 7, the notation of 2021.2 = 36.5 days after
>Jan is incorrect.
We have changed notations of X-axis in Figures 7, 8, 10, 11 and 12 to year/mon/date at each graduation. Explanation of X-axis in each figure is changed accordingly.

>3. Should report the pH scale of your pH measurement
Although we have already described in Section 2 that we have used total-scale buffers for calibration of pH sensors, we additionally specified in lines 295-296 that all pH values used in this paper is in total scale.

>4. In Figure 7 and line 271, the salinity change of Miyako Site is covered by the other stations and
>cannot be distinguished.
Y-axis and dot size of Figure 7b are adjusted so that two low peaks of salinity in Miyako Site can barely be distinguished.

>5. Line 271, citations are needed when linking the salinity to the precipitation events in different
>seasons.
A reference for coastal salinity variation and seasonal rainfall in Iwate Prefecture is added in Line 275.

>6. Line 278, it is crucial to provide evidence or citations to support the claim that the low dissolved
>oxygen (DO) observed during the summer is attributed to temperature rather than biological
>processes. Because other factors such as the late stage of algal blooms or other events may also lead
>to reduced DO levels in the water
Dominant contribution of water temperature to DO seasonal variation is proved by using DO(eq) in

Section 4.2. In the revised manuscript we add a notation in Line 281 to refer section 4.2 for more detailed discussion about this statement.

>7. Line 279 – 280 The manuscript should consider the potential influence of salinity changes on >oxygen solubility, particularly in regions like the Ohno Strait with significant salinity variations.
In these lines we refrain discussion about the causes of short-term DO variation and just described the existence of short-term variation. The origin of short-term DO variation is then discussed in In Section 4.2, where we mentioned that, as you pointed out, shot-term variation of salinity played major contribution to the observed DO variation in Hinase and Ohno.

>8. Line 280- 282, in addition, instead of discussing DO, AOU can serve as an appropriate indicator.
In section 4.2 we calculated DO(eq) and DO(diseq), and the later is substantially same variable with AOU. In this paper we use name of DO(diseq) instead of AOU, however, so that we can treat this parameter as a one piece of the biological component package "C_i(diseq)," such as pH(diseq) and Ω_{ara}(diseq).

＞9. Line 288, why the raw pH data were used not the pH data after drift correction?
Sorry for imprecise expression. All the numbers plotted in Figure 8 were pH values after drift correction. In the revised manuscript, we corrected expression in line 288 (now line 295) and the legend of Figure 8.

＞10. Figure 9, all the regressions lack of R squared values, RMSE and other statistical analysis details.
RMSE and r^2 value are added in each regression equation in Figure 9.

>11.Table 1, it is unclear how the uncertainty terms of the fresh endmember Talk were calculated. >Needs descriptions either in the caption or main text.
Uncertainty terms of freshwater endmember was calculated as standard error of sal = 0 intercept in least squares fitting. In the revised manuscript we add this information in the caption of Table1.

>12. Line 380m the pCO2, the partial pressure should be italic...
Typeface of partial pressure is corrected to italic.

>13. Major comments in Section 4.1 from line 435
>There are numerous questions concerning the comparison between short-term and long-term >variations. Firstly, it is unclear how the standard deviation (SD) was calculated at different time scales.

>For the annual SD, it is not specified whether all data points were used in the calculation. Similarly,
>for the monthly average of 10- days SD, it is unclear whether it was obtained through a moving
>average or only considered the first 10 days, 10-20 days, and 20-30 days of each month. The lack of
>clarity in these definitions and calculations significantly hinders the interpretation of variations, as
>the results heavily depend on how the standard deviations were computed.

Annual average and annual SD were calculated based on the calculated based on the last 1-year data to avoid biases that comes from different time length. We add this information in the caption of Table 2.

$\overline{SD}^m_{10}$ was calculated as the moving average of 10-days SD in each month. We modified the sentence for the explanation of $\overline{SD}^m_{10}$ in lines 455-456.

>14. I do not fully agree that the contribution of the monthly SD to the annual SD can be assessed as
>a ratio or percentage without taking sample size into consideration in the Section 4.1. These need ref
>support.
>Also, if the average monthly SD is smaller than the annual SD, it could be interpreted as the between-
>group variation, implying that monthly mean variations contribute more to the annual variation.
>However, if the average monthly SD is almost the same or even larger than the annual SD, it becomes
>challenging to determine conclusively whether the contribution to the annual variation comes from
>within-group SD (monthly SD) or between-group SD (monthly mean). Again, these discussions
>should have support from statistical literature or equation support.

Following to your comment, we introduced a new variable, annual SD of the monthly averages ($SD_{a<m>}$) in Table 2. contribution of short-term variation with time scale of less than 1 month is then discussed based on the difference between annual SD and $SD_{a<m>}$.

Also. we modified the text of section 4.1 to refrain from discussing about mechanism of temporal variation (e.g., temperature-driven vs biological-driven) at this stage, limiting our discussion to the relative contribution of variations with different time scales.

>15. Line 593. This statement requires further clarification. The thermodynamic process, which
>involves changes in temperature and other constants, also encompasses the air-sea gas exchange
>process. The equilibrium term mentioned in the manuscript is the combined outcome of these two
>processes. It is essential to note that the equilibrium term is often considered negligible because the
>effects of these two processes tend to counterbalance each other.

Please note that we defined C_i(eq) as the estimated concentration of parameter i in equilibrium with the current atmosphere under the observed water temperature and salinity. Variation of C_i(eq) hence only represents purely thermodynamic changes, and non-equilibrium term caused by insufficient gas exchange is incorporated in C_i(diseq) if exist.

In this sentence, we already know that variation C_i(eq) is less than 10 % of that of C_i(diseq). As variation of non-equilibrium term caused by insufficient gas exchange is less than that of C_i(eq) by definition, we can conclude that variation of C_i(diseq) caused by insufficient gas exchange is also negligible without any discussion.

>16. Lines 601-605. The contribution of the SD to the variation of pH should be carefully interpreted.
>Merely relying on the correlation of the SD may not conclusively indicate that pH variations are
>solely contributed by other factors. For example, a situation might arise where high pH variation
>(SDm 10) occurs in the first 5 days of a month, while high salinity variation occurs in the last five
>days of a ten-day period. In such cases, the correlation of the SD might not fully capture the
>relationship between the two variables. To ensure robust and meaningful conclusions, a statistical
>study and appropriate justification should accompany the use of the correlation of the SD.

In lines 601-605 in the original paper, we evaluated relative contribution of non-thermodynamic component to the observed annual variation based not on the correlation between SDs but on the seasonal amplitude of non-thermodynamic component that can be detected both from Table 4 and Figures 11 - 12.