

Responses to reviewer 1

Thanks again for your various constructive suggestions/comments in second revision. Based on your comments, we modified the manuscript as follows:

>Please replace “this result suggests that the Japanese coastal area can function as the
>“sample shelf” of the coastal acidification in the world.” to “this result suggests that the
>Japanese coastal area can be considered as a “sample shelf” for coastal acidification
>studies.”

=>Sentence revised. (L64-65)

>Please mention that this is “the 1st synthesis effort of OA monitoring stations operated by
>different founers/programs” clearly in the text.

=>We inserted this sentence in line70-72, with slight modification in the following sentence.

> It is (Pierrot and Wallace, 2006) not (Pierrot et al, 2006).

>

=>Reference corrected. (lines 131-132)

>Please edit in Figure 10 and line 401.

>

=>corrected.

>The new format is “year.month”, and is different in Figure 8(b). I still find this confusing. I would
>suggest to add the month than the abbreviated year, e.g. Jan-20 or Dec.21, etc.

>

=>As we use 1.5-years data from 2020/5/26 to 2021/12/31, we cannot omit year from the X-axes. Instead, we changed the style of X-axes of all time series to that of Fig.8(b).

>Please add “for” before the Baltic Sea.

>

=>added. (Line 323)

>Please re-arrange the references from old to new.

>

=>corrected. (Lines 403-404)

>Please edit: "in the coastal area of East China Sea.."

>

=>corrected. (Line 671)

>Then please clarify that you mean anthropogenic nutrients' loadings in L680, 683, 710, 711,
>734.

>

=>We modified these sentences to clarify that we are discussing about anthropogenic nutrients'
loadings.

>Please edit: "The country's Ministry of..."

>

=>corrected. (L60)

>Please edit: "In most study areas, calculated freshwater endmember of TA-S regressions
>agreed with the observed TA of one of freshwater sources (Table 1), indicating that non-
>dilution/concentration changes of TA (Jiang et al. 2014) was relatively small in these areas,
>and..."

>

=>corrected. (Lines 355-357)

>Please edit: WDCGG, 2023

>

=>corrected. (Line 405)

>Please edit: "Such a result can be attributed to the perturbation of salinity that..."

>

=>corrected. (Line 477)

>Please edit: "variations with shorter time scale than one month contribute to some parts of the
>annual variation in the case of salinity."

>

=>Here, we hold our original expression as your modification changes the meaning of the
sentence significantly. We believe that we had been able to show the probability of the same
cause of seasonal variation of SD10 among DO, pHmin and Wara.

>Please edit: (unpublished data).

>

=>corrected. (Line 742)

>In the caption of Table 2, please edit: "...biases that come from different time length."

>

=>corrected.

>P27, L673: Please edit "..., in Hinase and Ohno,"

>

=>corrected.

>The discussion is still weak. Results can be better compared with regional and global studies.

>In addition, many of the discussed statements in the conclusion could be used in the

>discussion section, while keeping the conclusion section more focused on results and concise.

>

=>We added several sentences (e.g., Lines 279-280, 556-567) so that the readers can compare the observed variabilities of each parameter in this study with that of former studies.

Responses to reviewer 2

Thanks again for your various constructive suggestions/comments in second revision.

All time-series figures are now replaced to new versions. Also, all notations of $\mu\text{mol}/\text{kg}$ remained in the 1st revision were replaced to $\mu\text{mol kg}^{-1}$.

Responses to reviewer 3

Thanks again for your various constructive suggestions/comments in second revision.

In response to your comments on the complexity of SDs in our analyses, we re-edited the names of various SDs to have consistency in their naming strategy (Lines 452-460, Section 4.1 and 4.2). Also, we added a schematic explanation of SDs in Appendix 1.

Other responses to the comments are as follows:

>(Line 549) property > parameter?

>

=>corrected. (now Line 571)

>(Line 556) It is easier to understand if you describe that DO_{diseq} is AOU multiplied by -1.

>

=>thank you for this idea. I added this description into Line 578.

>(Line 559) mol > mole?

>

=>corrected. (now Line 581)

>(Line 569) The notation "DIC_{min}" is used, but this may be misleading since the DIC >calculated from pH_{min} is not smaller, but rather larger. The term "pH_{min}" is >introduced near Line 313. If it is explicitly stated after this sentence that "pH_{min}" is to >be used for subsequent pH calculations. "pH_{min}" and "DIC" can be referred to as "pH" >and "DIC," respectively.

>

=>Here, we had misspelled DIC_{max} to DIC_{min}. Thank you for this notification.

>(Line 652) You mention that the respiration quotient in the sediment is possible cause for a larger

>DIC/O₂ ratio larger the Redfield ratio. However, I believe there is another consideration to be made.

>The Redfield ratio of 0.77 assumes that both DIC and O₂ are no longer being exchanged with the

>atmosphere. Given that oxygen is gradually released into the atmosphere over time, it is reasonable to

>expect that the DIC/O₂ ratio should be significantly higher. The author should know that air-sea

>difference in pCO₂ can deviate significantly from zero while AOU approaches zero at the surface of the

>open ocean. If biological activity is occurring at a distance from the station, rather than in its immediate

>vicinity, it is plausible that the DIC/O₂ ratio could be inflated due to the release or absorption of oxygen
>into the atmosphere during the advection period.

>

=>Thank you for important suggestion. We agree that this process cannot be rejected without any
explanation, and hence we added description on how we conclude that this process is not main cause of the
observed high $-\Delta\text{DIC}_{\text{max}}(\text{diseq})/\Delta\text{DO}(\text{diseq})$ ratio in Hinase area (lines 676 - 689). The largest reason is
that we cannot observe high $-\Delta\text{DIC}_{\text{max}}(\text{diseq})/\Delta\text{DO}(\text{diseq})$ ratio in Ohno, while monthly wind speed is
rather higher in this place than in Hinase.

>(Line 675 and Figure 14) I do not agree with the theory that the slope changes with a difference in
>salinity greater than 1.0. This is because 1.0 salinity has no scientific significance. How is the slope of
>each area if not divided by salinity 1.0? According to Fig. 14, it may be obvious that the greater the
>anthropogenic load, the greater the short-term variation of pH in response to the short-term variation of
>salinity.

>

=> Generally, suspended particle export of the river increases linearly with that of river discharge, while
percentage of particle organic materials (POM) within suspended particles decreases along with the increase
of suspended particle export (e.g., Coynel et al., 2005; Point et al., 2007; Zhang et al., 2013; Bukaveckas
2022). As this result, increasing rate of the POM transport with that of river discharge tends to become
weak when river discharge becomes high enough (Coynel et al., 2005; Kim et al., 2020). If we assume that
short term pH variation is mainly caused by the degradation of POM discharged from rivers into coastal
areas and subsequential biological production by using nutrients released from the degraded POM, then we
can expect that $\Delta\text{avgSD}_{10^{\text{m}}}(\text{pH}) / \Delta\text{avgSD}_{10^{\text{m}}}(\text{sal})$ will also become small at the months with high river
discharge (i.e., high $\text{avgSD}_{10^{\text{m}}}(\text{sal})$). We added this explanation in the revised text (Lines 717-725).

Although there is certain reason why we apply two-lines regression, we have no specific reason why the
boundary of two regression lines become at the $\text{avgSD}_{10^{\text{m}}}(\text{sal})$ of 1.0. We determined this boundary so
that correlation coefficient of both lines become maximum, so there may be some undetermined relationship
between this value and low/high discharge boundary in Japanese rivers. even so, we cannot clearly explain
such relationship so far. We, therefore, changed Fig. 14 to show both all-data regression and high-
 $\text{avgSD}_{10^{\text{m}}}(\text{sal})$ data regression. Fig.15 has also changed to show these two results.

>(Line 681) the dose > those?

>

=>corrected. (now Line 728)

>(Figure 7, 8, 10, 11 and 12) The horizontal axis should be corrected to a month-year notation, such as
>Jan-21, Jan-22. This is an item that was also pointed out by two reviewers in the first peer review.

>

=>Sorry that we somehow failed to convert these figures to new ones. Now these figures are renewed.