

Interactive comment on “Rapid increasing trend of CO₂ and ocean acidification in the surface water of the Ulleung Basin, East/Japan Sea inferred from the observations from 1995 to 2004” by J.-Y. Kim et al.

J.-Y. Kim et al.

djocean@kiost.ac

Received and published: 23 September 2013

General Comments

This paper describes the trend of ocean acidification in surface water of the Ulleung basin in the southwest East/Japan Sea on the basis of the datasets of fCO₂ acquired between 1995 and 2004. The authors claim that the rate of long-term fCO₂ increase in this region is 3.36 uatm/yr. This is about twice as fast as that expected from the rate of atmospheric CO₂ increase. However, the way of analyzing the rate, i.e. a simple ap-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



plication of linear regression for fCO₂ data that are distributed unevenly in both space and season, is too rough to believe the rate that authors claim. In addition, no possible cause of the faster pCO₂ increase than in the atmosphere has been discussed in this paper, in spite that it might have an important implication for the change in the carbon cycle in the marginal sea. In summary, I think this paper needs major revisions before accepting for publication.

RESPONSE: We added more data sets available (data sets observed in November, 2008 and in July, 2009). And re-analysis the data sets using harmonic function analysis not the simple linear regression for the annual increasing trend. Therefore, the values are changed. For example, the long-term fCO₂ increase is changed to 2.7 $\mu\text{atm}/\text{yr}$ from 3.36 $\mu\text{atm}/\text{yr}$. The title is also changed to “Long-term trend of CO₂ and ocean acidification in the surface water of the Ulleung Basin, the East/Japan Sea inferred from the underway observational data”. For the reviewers’ information the revised manuscript is attached in the form of a supplement. Please see the supplement attached.

Specific Comments

(1) Introduction: page 9575, lines 1-3: I don’t understand why “atmospheric CO₂ is constantly increasing at an unprecedented rate” disturbs “the carbonate system in ways that will make air-sea exchange difficult”.

RESPONSE: The sentence is removed.

(2) Section 2.1: page 9577, lines 15-20: How were the measurements of TA and pH standardized?

RESPONSE: We added the calibration of measurements using CRM into the text as follows; “Total alkalinity was calibrated by Dickson’s CRMs which are measured at every cruise.”, and “The extinction coefficients for m-cresol purple from Clayton and Byrne (1993) were used.”

(3) Section 3.1: Fitting of seasonally-varying time-series data to the combination of har-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

monic and exponential (or linear) functions has been made in analyzing the observed atmospheric CO₂ concentrations (e.g., Keeling et al., 1989: Geophysical Monograph, 5, 165-221). The residuals of data from fitting curve give the information on interannual variability. In using harmonic function like Eq.(2) to evaluate the long-term change, it is appropriate to add the term of long-term (linear) change $C\Delta t$; (t denotes time and C denotes coefficient that represents the rate of linear increase). However the number of data points (12) appears not enough to determine the rate of increase with small uncertainty. Authors need to provide the uncertainty of the rate they determined. I would also suggest authors to try multi-linear regression of fCO₂ as a function of time and SST (and some other variables) to evaluate the rate of fCO₂ increase as has been done by Inoue et al., 1995 (Tellus, 47B, 391-413), too, and compare its result with that from the fitting to a harmonic function.

RESPONSE: We added more data sets available (data sets observed in November, 2008 and in July, 2009). And re-analysis the data sets using harmonic function analysis not the simple linear regression for the annual increasing trend.

(4) Page 9582, line 17: Where did the coefficient 0.0376 come from?

RESPONSE: It was calculated from the following equation $(\partial p\text{CO}_2/\partial T)/p\text{CO}_2 = (275/17)/350.9 = 0.0462 \text{ }^\circ\text{C}^{-1}$ (This value is also changed) $\partial p\text{CO}_2 (= 275)$ is equivalent to the $\Delta f\text{CO}_2^{\text{therm}}$ $\partial T (= 17)$ is from the peak-to-peak amplitude of the seasonal SST $p\text{CO}_2 (= 350.9)$ is averaged value of the $f\text{CO}_2^{\text{sea}}$

(5) Page 9583, line 25: Fugacity has been used for the data from UB but partial pressure has been used for the data from Gosan. Are there any reasons for this difference?

RESPONSE: Data from Gosan are reported in the form of pCO₂. We cannot convert the value to fCO₂ because of the lack of atmospheric pressure and temperature data. Nevertheless, we compared the data because the fCO₂ values in the atmosphere are not significantly different from pCO₂.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

(6) Section 3.4. Re-evaluate the rate of fCO₂ change as mentioned in the comment (1). If the rate of fCO₂sea change re-evaluated still differs significantly from the rate of fCO₂air increase, discuss on the reason for the difference. One of the working hypotheses could be the long-term change in the ocean circulation. Strengthening of intrusion of subtropical water through Korea Strait may cause warming and annual mean fCO₂ rise together.

RESPONSE: We added more data sets available (data sets observed in November, 2008 and in July, 2009). And re-analysis the data sets using harmonic function analysis not the simple linear regression for the annual increasing trend. Therefore, the values are changed. For example, the long-term fCO₂ increase is changed to 2.7 μatm/yr, which is closer to the atmospheric increasing. However, the increasing trend is in good agreement with other studies in the mid-latitude region in the northern Hemisphere.

(7) Table 1 and Section 3.4: Give salinity value in Table 1. Since total alkalinity is largely affected by precipitation and evaporation (dilution and concentration), authors need to examine the relationship between total alkalinity and salinity. Total alkalinity in the studied region may have been changing with the change in vertical mixing or circulation change. I would suggest authors to try to derive empirical equations for total alkalinity as has been done by Lee et al., 2006 for open oceans.

RESPONSE: We added salinity values in Table 1.

(8) Page 9584, lines 26-27: Show plots of calculated pH value versus measure ones.

RESPONSE: This was already shown in the figure 7b (Revised manuscript).

(9) Discuss also on the rate of change in carbonate saturation index Omega for aragonite and calcite that are important for the growth of sea shells.

RESPONSE: This is good suggestion. But this suggestion is beyond the purpose of this manuscript.

(10) Table 3: Add Lenton et al. (2012), Midoriakwa et al. (2012), Ishii et al. (2012).

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Ishii et al. (2009) instead of Ishii et al, 2004.

RESPONSE: We modified the references in Table 3.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/10/C5265/2013/bgd-10-C5265-2013-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 10, 9573, 2013.

BGD

10, C5265–C5269, 2013

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C5269

