Interactive comment on “Rapid increasing trend of CO\textsubscript{2} 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.

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General Comments

The manuscript reports twelve observational data sets of surface fCO\textsubscript{2} between 1995 and 2004 in the Ulleung Basin of the East/Japan Sea. Using these data sets, the authors try to reconstruct the decadal increasing trend of air and surface water fCO\textsubscript{2} and pH in the study area. This is an interesting and important research topic to better understand how the carbonate system in marginal seas would respond to the rising atmospheric CO\textsubscript{2}. However, I don’t think that the authors have reached this goal with the limited data sets. Please see below for my comments. The annual data sets used in this study were collected in different seasons and with different sampling frequencies during each sampling year. For instance, the data sets were collected once in summer 1995, spring 1997, summer 1999 and spring 2002. For the other sampling years, data sets were from more than one season (that in 1996 was from winter and summer, that in 2003 was from summer and winter, and that in 2004 was from spring, summer and fall). If the interested parameter is characterized by a strong seasonal variation (such as temperature and fCO\textsubscript{2}sw), directly using the data collected in different seasons and/or with different sampling frequencies to reconstruct the long-term trend can lead to a misleading result. This problem can be clearly seen in the attached figures (Please see the following page). As shown in Figs (a)–(c), the increasing rates of temperature, fCO\textsubscript{2}air and fCO\textsubscript{2}sw between 1995 and 2004 are +0.26âŠŒ/yr, +1.97 âŠŒ/yr, and +3.25 âŠŒ/yr, respectively. Apparently, the increasing rate of temperature (+0.26 âŠŒ/yr) could be problematic, which is much higher than that in any other regions in global oceans. Furthermore, reprocesing the same data for the period 1995–2003, we can find that temperature and fCO\textsubscript{2}sw did not show an increasing trend any more, but show a decreasing trend (-0.20âŠŒ/yr for temperature, Fig. (d); -1.47 âŠŒ/yr for fCO\textsubscript{2}sw, Fig. (f)). In contrast, fCO\textsubscript{2}air still shows an increasing trend with a rate very close to that for the data from 1995 to 2004 (Figs. (b) and (e)). This discrepancy can be largely explained by the fact that the seasonality of fCO\textsubscript{2}air is much smaller than that of temperature and fCO\textsubscript{2}sw, and thus suggests that the derived long-term trends for the season-dependent parameters (such as temperature and fCO\textsubscript{2}sw) may be very questionable. In summary, the above analysis demonstrates that the limited data sets, collected in different seasons and with different sampling frequencies, hinder the authors from obtaining a convincible long-term trend for fCO\textsubscript{2}sw and pH. Since the following discussions in this paper, including 3.2 seasonal variability, 3.3 decadal trend and 3.4 acidification rate, all depend on a reliable increasing trend of fCO\textsubscript{2}sw, which is very questionable with the limited data, I do not recommend the manuscript.
RESPONSE: It is quite difficult to understand since the data include large seasonal variation as well as long-term trend with the limited data set. So, we added more data sets available (data sets observed in November, 2008 and in July, 2009). It shows the increase of fCO2sea clearly. And we also 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 atm/yr from 3.36 \mu atm/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.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C5257/2013/bgd-10-C5257-2013-supplement.pdf

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