

Interactive
Comment

Interactive comment on “Rate of Iceland Sea acidification from time series measurements” by J. Olafsson et al.

Anonymous Referee #1

Received and published: 30 June 2009

Review of the manuscript Biogeoscience

Title: Rate of Nordic Seas acidification from time series measurements Author(s): J. Olafsson, S.R. Olafsdottir, A. Benoit-Cattin, M. Danielsen, Th.S. Arnarson, and T. Takahashi MS No.: bg-2009-70 Special Issue: The ocean in the high-CO₂ world II

Decision: This manuscript is acceptable for publication after revisions

General comment:

Based on regular TCO₂ and pCO₂ observations, conducted in 1985-2008 in the surface and water column of the Iceland Sea, authors evaluate the change in pH and calcite/aragonite saturation states. For surface waters, authors use only winter measurements. For deep waters they used seasonal observations conducted between

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

1994 and 2008. In both surface and deep waters the decrease in pH is well quantified and appears faster than in other sectors where decadal pH change (but on an annual basis) has also been evaluated from ocean observations (subtropical regions). The manuscript is well written; it was a real pleasure to read and review this short paper that needs several clarifications. Main comments are addressed here and specific comments listed below. First, I strongly suggest authors to show and discuss the observations (TCO₂ and pCO₂) for all seasons before presenting pH (a derived quantity). This may help to explain what causes the acidification: is it 100% related to anthropogenic CO₂ uptake, or would it be possible that part of the pH variations are related to other factors (changes in circulation and vertical mixing, changes in biological activity and/or remineralisation at depth, etc....). Secondly, I'd like to know what are the trends deduced for other seasons. If when using summer data, the trends could not be evaluated, it is an important result to show. This could help future analysis when only summer data are available. Here I also note that productive season occurs in spring/summer, therefore it would be interesting to know what is the pH change during this season. Third, I think there is some confusion when comparing trends for surface and deep waters: the periods are not the same and when looking in more details the surface changes (Figure 3) the Omega (calcite and aragonite) are very stable since about 1997. Therefore the trends for surface should be evaluated for two periods, 1985-1997 and 1997-2008. The conclusion regarding the relative change in surface and deep waters would be much different (e.g. more rapid decrease in deep water for recent years compared to surface observations); this result should be discussed regarding internal processes and external forcing (NAO shift in 1996 ?). Finally, as it is recommended in many projects (e.g. CARBOOCEAN, EPOCA that have supported this analysis), I suggest authors to indicate somewhere in the paper where the published data are available (e.g. CDIAC, CARBOOCEAN, other data-base ?).

Specific comments:

The comments are presented following pagination.

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

C1: page 5253 line 2: authors indicate they describe 21 years of winter observations during 1985-2008. I have two comments on this. First, I'd like to see all data, including other seasons, so authors can clearly show that for other seasons the determination of the decadal pH changes cannot be well evaluated. This might help for future observations strategies and analysis based on historical data. Second, for the deep water analysis, authors only start the analysis in 1994, not in 1985. This might be obvious but it should be indicated somewhere (beginning of section 2.2) that column water sampling was started in 1994, whereas in 1985-1993 only surface samples are available (is it correct ?).

C2: page 5253 lines 5-6. Figure 2 is not very informative for this analysis. I suggest to delete Figure 2, and add figures to present TCO₂ and pCO₂ observations as well as T,S sections that may reveal water-masses variability, warming/cooling, freshening, changes in vertical mixing; the later process being only recalled in the conclusion..

C3: page 5254 lines 8. Feely et al (2004) not in references.

C4: page 5254 lines 10. Orr et al (2005) not in references.

C5: page 5254 lines 14: Steinacher et al 2008 should be 2009 (?)

C6: page 5255: Authors present an error analysis for pH and Omega values depending if calculation include or not nutrients (silicates, phosphates). This implies systematic errors but would that imply different results and conclusion for the temporal variations of pH and Omega ?

C7: page 5256, section 2.2. Authors should described first the sampling strategy (monthly ?, surface for 1985-1994 and then water column).

C8: page 5256, line 11-12: need a reference to the pCO₂ gas chromatography measurements

C9: page 5257: Section 3.1. I think the full pCO₂ and TCO₂ data-set should be presented before showing calculated pH trends (and T, S as well). Authors should

discuss the change in both TCO₂ and pCO₂ as observed (e.g. we expect an increase of both quantities due in part to anthropogenic uptake). Such figures would also show how variable are the summer observations from year to year and justify the use of winter data only to evaluate the decadal trends. This would also help to comment and explain why in recent years (1997-2008) Omega (both aragonite and calcite) are very stable. Figure 3 shows that the temporal change in Omega appears more like an abrupt change (around 1996 ?) rather than linear progressive decrease. Would this rapid variations around 1996-1997 could be related to NAO phase (positive to negative in 1995-96) or other external processes ?.

C10: page 5257: line 15-17. Results in Iceland Sea are compared to BATS and ESTOC but authors then specified this is not directly comparable as BATS and ESTOC use annual data. It would have been interesting to (i) estimate the trends for all seasons or (ii) use BATS and ESTOC for winter only. The abstract should be revised accordingly.

C11: page 5258: Lines 1-2. Results suggest that Omega(Calc) decreases faster than Omega(Ar). This appears different than what is expected ? (see authors comments on page 5264 lines 4-6).

C12: page 5258: lines 24-28 and page 5259 lines1-4: comparison of deep water and surface water changes are not for the same periods. It would have been interesting to evaluate surface pH changes over 1994-2008, same period as for deep samples. When looking at figure 3 (surface waters) there is a clear change for Omega trends over the full period. After 1997 Omega values are almost constant (or increase slightly ?). Therefore when comparing to the same period, the change in deep water would be much higher than in surface. This should be discussed in the manuscript (and corrected in the abstract).

C13: page 5259 lines 22-30: this section is more like a discussion for all results, not directly related to deep water (title of section 3.2). A Discussion section should be now introduced so authors can associate pH and Omega trends (in both surface and deep

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

waters) to specific internal and external processes occurring at regional scale in and around the Iceland Sea.

C14: page 5260 lines 3: this is strange to read the following reference: J.Olafsson pers. Comm. Please refer to another paper or indicate in this manuscript how do you get the CO₂ air-sea CO₂ flux value; need an error attached to this number.

C15: page 5260, lines10: conclusion for Omega(ar) change should be revised according to the variations observed between 1985 and 1996 and the relative stability after 1997.

C16: page 5260, lines11: above 1500m should be corrected: below 1500m

C17: page 5263, line 5: lattitude...latitude

C18: page 5264, table 1: regarding figure 3, trends for 1985-1994 and 1994-2008 should also included in Table1

C19: page 5264, table 1: does one needs so many digits in this table ?

C20: page 5265, table 2: use acronym TCO₂ as in the manuscript, not TIC

C21: page 5268, figure 3: for Omega I would draw two lines, one for 1984-1997 and one for 1997-2008.

C22: page 5269, figure 4: at 1750m Omega(ar) (equal 1) seems to be very stable. The rising as indicated in the abstract and conclusion 4m/yr is not so clear in figure 4.

Interactive comment on Biogeosciences Discuss., 6, 5251, 2009.

BGD

6, C953–C957, 2009

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper