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

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We thank the two referees for their thorough review of our manuscript and constructive suggestions for improving the manuscript. In our respone we first address general comments and follow those with specific comments.

General comments

1 Referee #1 suggests that surface ocean observations and calculated properties, pH, from all seasons are presented, not only winter data. The specific comments C1, C9, C10 and C13 also relate to this issue. The referee supports this with relevant arguments. However, we disagree with him/her and maintain that the time trends for the Iceland Sea are best evaluated from winter data. We do acknowledge that Referee

#1 is absolutely right when calling for further arguments for the selection of winter data only and we will respond to that in the revised manuscript along the following lines.

In the Iceland Sea there is large seasonality in both physical and biogeochemical parameters as described in Takahashi et al. (Rit Fiskideildar,1985) and Takahashi et al. (GBC, 1993). In these papers we reported that the seasonal variation in pCO2 is not sinusoidal but more like a saw-tooth shape with a maximum platform in mid winter and a sharp drop to the annual minimum caused by the phytoplankton spring bloom. The annual pCO2 amplitude in our quarterly observations may reach 160 μatm, mostly depending on how close to the spring bloom the May observations are taken. The location and timing of the spring drawdown of CO2 varies due to the patchy nature of phytoplankton blooms. The pH values computed using the pCO2 and TCO2 data reflect these seasonal changes and show a similar pattern of changes. Thus, the spring-summer pH values exhibit large space and time variability, and yield a mean-time trend with large uncertainties. In contrast during high winter, biological production is minimum due to short daytime and cold temperatures and the conditions are governed primarily by physical and abiological processes such as sea-air CO2 gas exchange and convective mixing of subsurface waters. Hence, the winter values for pCO2 and pH in seawater exhibit much smaller variability and a more reliable time trend can be obtained. The results in Table 1 A on the winter observations show that 89% of the variability in the pH can be accounted for by time alone, and 91% can be accounted for by adding temperature to the regression. In contrast, if all the seasonal data are included in this statistical evaluation, the variability explained drops to 38%. Thus the statistical evaluation of the time trends becomes uncertain when based on observations from all seasons. Our goal in this communication is to describe the long term changes in the sea water carbon chemistry that relate to the rising atmospheric CO2 and changing ocean circulation. Since surface waters in winter represent the initial condition for seasonal modifications to follow in each year, the winter observations serve our purpose well. It is also clear that evaluation of long term trends from whole year data has to be carried out with caution. In this connection one must remember that the subtropical

ESTOC and BATS time series are located in the areas with smaller and smoother seasonal variations than the Iceland Sea series and have more frequent observations (i. e. monthly) than the Iceland Sea series (4 times a year).

2 Referee #1 calls for further data/figures to illustrate the link between atmospheric CO2 and sea water pH. We will add a figure which suggests that the observed decrease in pH may be attributed primarily to the uptake of atmospheric CO2.

3 Referee #1 questions the usefullness of Fig. 2. The information in this figure can be expressed in words and we will do so depending on the editor's opinion.

4 Referee #1 questions the comparability of surface water and deep water trends when the observation periods are not of equal length and makes a specific comment, C12. In the revised manuscript we will clarify this issue and explain the influence on the results.

5 Both referees comment on the availability of the data. The time series data are in the CARINA collection and this will be noted in the revised manuscript.

6 Both referees, in particular referee#2, comment on the structure of the manuscript and suggest the addition of a discussion section. We will act on these suggestions and expand the discussion as referee#2 suggests.

7 Referee#2 suggests for the discussion an elaboration on the potential consequences of the shoaling saturation horizon, effects on the benthos and prediction of saturation horizon depth, e.g. in 2100. We will include this important point in the discussion but since we are not modelling future processes and since we do not expect the observed linear time changes to continue unchanged, we are reluctant to predict future conditions. This relates also to the referees comments on Figure 5.

Specific comments

By referee#1

The specific comments C1, C9, C10, C12 and C13 are addressed above, General

comment 1 and C12 in General comment 4.

p5255, C6: We already stated that the errors due to neglecting nuts in the Iceland Sea are within our experimental errors.

p5258 lines 1-2: We are prepared to explain fully in a paragraph the relative rates of change of Ωca and Ωar, depending on the editor's instructions.

p5269, figure 4: The figure illustrates the overall shoaling of Ωar in the water column below the influence of seasonal variations. The scale of the figure is not apropriate for illustrating the shoaling of Ωar=1 which is therefore described in the text and illustrated in Fig.5.

By referee#2

p5252,line 5: See 2 in general comments above.

p5252, line 17: We will change from "solubility of calcium carbonate" to "dissolution of calcium carbonate".

p5254, line5: This new relevant reference is noted and will be added.

p5255, line 28, and p 5256, line 21: All the chemical equations used in this paper are stoichiometric. The dissociation constants for carbonic and boric acids and CaCO3 are for seawaters (complex chemical compositions with an ionic strength of about 0.7). Therefore, they are commonly called "apparent" as opposed to "thermodynamic" (for infinite dilution).

p5259, line 11: The aragonite saturation horizon is shallower in the Iceland Sea than the Arctic due most likely to the higher alkalinity/TCO2 ratio in the Arctic waters, that may be attributable to high alkalinity of river waters flowing into the Arctic basin and/or that generated by processes associated with formation/melting of ice. Lower ventilation rates and lower temperature than in the Iceland Sea may also play a part. Since waters with higher alkalinity/TCO2 ratios have higher pH and higher Ω values, greater

pressures are needed to produce undersaturation states for calcium carbonate.

p5259, line 18: Information on benthic biota that have become immersed in undersaturated waters is currently not available. This is an important issue and the subject of a new study which will hopefully be reported on in the near future.

Other specific comments of the referees

Many of the specific comments are for clarification and corrections of the text and we will respond to those when editing the final manuscript.

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