

Interactive comment on “Inorganic carbon time series at Ocean Weather Station M in the Norwegian Sea” by I. Skjelvan et al.

I. Skjelvan et al.

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The following are my answers to the comments from M.Alvarez 12 Sep 2007:

First, thank you very much for your comments, which I have read with interest. I agree with some of them and do not agree with others, as will be explained beneath.

Abstract: In the text we write "annual increase of ... $\mu\text{mol kg}^{-1}$ " or "increase of ... $\mu\text{mol kg}^{-1} \text{ yr}^{-1}$ ", so when the whole sentences are red the correct meaning is also expressed. I will, as you suggest, add the numbers for annual atmospheric CO₂ increase in the text.

Introduction: Concerning the different references I agree to a certain point and will include "e.g." in the text to point out that the references used are based on a choice between several. In the second last paragraph of the introduction the water masses

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are described very briefly. This might of course, as you suggests, have been followed by a figure; however, since the data we are using are described later, we decided it was best not to show any figure here. A T-S figure could be used in connection to chapter 2 (Data) or 3 (Seasonal and interannual variability), though. The argument for keeping Fig. 6 is that this figure verifies similarities between the OWSM and TTO data, which is a requirement for the eMLR method.

Data: Precision of the Ct measurements; I agree on the contradiction in using Norwegian Sea Deep Water as a reference over time, and will re-write this. The precision is based on duplicate samples (two runs from the same sample). For the TTO data and reanalysis by Tanhua and Wallace (2005), we'll include some more explanation, however, we don't think that it is important for the present work to describe the reanalysis in detail.

Seasonal and interannual variability: I see the point of revising some the figures. A challenge is the difference in sampling time between the hydrography (five times a week) and the carbon (once a month), which might smear out the picture. I will discuss this further with the co-authors. However, I do not agree that an ODV figure of Ct could express the interannual increase in surface carbon content and the increase in MLD (and carbon content) over one winter in a better way than the existing Fig. 5.

Determining changes in Cant: I agree upon specifying the Cant method as "back calculation methods". When it comes to alkalinity, we have measured this parameter at only about half of our stations from OWSM (due to instrumental problems). At present we are not satisfied with our correction procedure and it is unclear if/when the data can be used. So for the present work we concern silicate as a better predictive parameter than alkalinity.

Discussion: I agree upon your comments and will include them in the manuscript.

Sincerely, Ingunn Skjelvan

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