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Biogeosciences Discussions

Interactive comment on "Carbon and nutrient mixed layer dynamics in the Norwegian Sea" *by* H. S. Findlay et al.

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Below are my comments to the Anonymous Referee#1 on 24th October 2007.

I would firstly like to thank the referee for their comments, which have been very interesting, and here we hope to clarify and rectify some of the issues that have been raised.

1. General comments. It has become apparent that there appears to be a misunderstanding in the specific aims of the paper, which has led to some confusion and which we, the authors, need to make clearer. While the paper addresses a temporal correlation of coccolithophore blooms to high carbonate ion concentration originally described in the model of Merico et al. 2006, we do not aim to demonstrate that coccolithophores are very important in producing the observed carbonate chemistry patterns. In the

discussion we state that coccolithophore blooms are comparatively small at OWS M compared to locations known for high coccolithophore concentrations. Merico et al 2006 did not have sufficient data to validate their model prediction that carbonate ion concentrations and saturation states are higher in summer than in winter, and here the model is used together with OWS M data to assess this. As such we will reword our aims: Replace Line 8 in Abstract with:

The available carbonate system data from other high-latitude ocean locations suggests that seasonal cycles resemble those at OWS M.

and change Lines 6-15 on page 3232:

This study aims to use an adaptation of Merico et al. (2006)s model as a tool to investigate nutrient and carbonate parameter dynamics in the Norwegian Sea. Specifically, we address the controls on nitrate and silicate consumption rates over the summer and the seasonal and interannual patterns of the carbonate parameters. We also assess the hypothesis that coccolithophore success occurs at times of high calcite saturation state, by comparing our model outputs with OWS M data and a detailed evaluation of other oceanic locations known to have high coccolithophore blooms.

This will hopefully clarify the aims of the paper, and in light of this we consider the rest of the comments.

1.1. Modelling the seasonal cycle. We agree that a Taylor diagram would aid the quantitative interpretation of this model and will carry out this analysis, along with a Monte-Carlo parameter analysis.

1.2. Coincidence between calcite saturation state and coccolithophore bloom. Two main points are raised here: 1) Dellile et al. 2005 mesocosm experiments show that coccolithophores grow in a variety of carbonate ion concentrations - we will admit in the discussion that they are interesting results and are apparently contrary to the idea proposed by Merico et al. 2006.

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2) Model fit and comparison with carbonate system - We agree that quantitative analysis will clarify the model fit to the data and corrected alkalinity data along with quantitative descriptions of coccolithophore concentrations should strengthen the validity of this papers evaluation of the hypothesis that coccolithophore blooms coincide with periods of high carbonate ion concentration. The degree of the shift in pCO2 data will be clearly stated and again we will make clear that the point of this was to show that the general pattern is the same, with the values being of less importance. The subject of the quality of the alkalinity data has also been bought up in the Addendum Alkalinity data by Anonymous Referee on 29th October 2007 and will be addressed as an Authors Comment specific to those comments.

In light of the aims of the paper, we feel the evaluation of the model, data and other locations is sufficient at this stage to give a general overview of the carbonate system dynamics in a variety of areas known for different levels of coccolithophore blooms. This furthers the investigation into the correlation between coccolithophore blooms and high carbonate ion concentration by evaluating the relative strengths of the seasonal dynamics at various locations. The model is here primarily used as a tool a) to complete the seasonal cycle of nutrients, DIC and Alkalinity, where specific data may be lacking (which will be greatly improved with quantitative analysis of model to data fit), and b) to then calculate the seasonal dynamics of the other carbonate system parameters.

1.3. Which complexity is required to model the seasonal cycle at OWS M? We will use quantitative mechanisms as described above to address the models suitability to recreate the seasonal cycle at OWS M under different model runs: with coccolithophores and without.

2. Specific comments. We agree that this is not novel and will remove reference to the non-biological except as suggested.

3. The rest of the technical corrections will be amended as recommended.

Yours faithfully, HS Findlay, T Tyrrell, RGJ Bellerby, A Merico, I Skjelvan

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