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Biogeosciences Discussions

Interactive comment on "Coccolithophores and calcite saturation state in the Baltic and Black Seas" *by* T. Tyrrell et al.

T. Tyrrell et al.

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We thank the anonymous referee for their constructive comments.

The referee concurs with the importance of considering alternative explanations for the different success of coccolithophores in the Baltic and Black Seas. We are not sure, however, whether we agree with the referee about the significance of our results if salinity is the key difference. The referee proposes that, if it is the difference in salinity between the two seas that drives differential coccolithophore success, that this could have significant implications for the future, in terms of a fresher ocean due to climate change and ice melting. We consider in our MS extreme salinities (e.g. ~ 7 and ~ 13 in the two seas) compared to oceanic values (between 33 and 38), such that increased freshwater input would not be expected to perturb oceanic values to anything like those experienced in the two seas.

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Specific comments:

1. A map of the Baltic Sea area will be provided, and areas mentioned in the text will be marked on.

2. We will specify that these are fossil fuel emissions (i.e. of CO2).

3. We will justify the use of dissociation constants by Roy et al. We use these constants (and not compare to other sets of constants) because it is only the Roy et al constants that were explicitly developed for low salinity waters (down to salinity of 5).

4. For consistency we will change all instances of DIC to C_T.

5. It is not appropriate to add error bars in this way to the saturation state values in figure 2, because it is not appropriate to use other sets of dissociation constants that were not intended for use in low salinity waters.

6. We will provide a new table giving other carbonate system variables (pCO_2, C_T, etc) at each location and season where we currently show CaCO3 saturation states.

Interactive comment on Biogeosciences Discuss., 4, 3581, 2007.