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

Anonymous Referee #2

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General comment: This manuscript represents a valuable contribution to a very active field of environmental research, the impact of changes in carbonate chemistry of ocean waters in response to anthropogenic CO₂ invasion. The use of natural analogues is complementary to short term perturbation experiments typically carried out in the laboratory or in large-scale mesocosm facilities. In this particular case, Tyrrell and co-workers confront observations from the Baltic and Black Seas with the objective of understanding what controls the presence or absence of pelagic calcifiers in these environments. They authors review several environmental parameters: salinity, carbonate chemistry, silicate concentration etc. and propose that the peculiar carbonate chemistry of the Baltic Sea explains the near absence of coccolithophores in this environment. While the importance of carbonate chemistry and in particular low saturation

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states with respect to calcite during winter might contribute to the lack of success of coccolithophores, the paper does not convince me that this is the prime control. The salinity in particular seems to be as likely a candidate. I suppose that the authors will agree with me that at this stage the case can not be settled.

I very much appreciated reading the paper. It is well written and the arguments are presented in a clear manner. I recommend publication with minor revisions.

Specific comments:

1. The selection of dissociation constants: The constants by Roy et al. (1993) are recommended for use in artificial seawater (Mojica Prieto and Millero, 2002, *Geochim. Cosmochim. Acta* 66, 2529). For studies dealing with natural seawater samples, those by Mehrbach et al. (1973) refitted by Dickson and Millero (1987) should be used. Please plot error bars along with your saturation state estimates on Fig. 2.

2. Are all Baltic Sea data by Schneider et al? Including those of the Bay of Riga and the Bothnian Bay, as well as the composition of river waters?

3. Add a map showing the geographic area, location names and sampling side. I would also appreciate a map showing salinity, alkalinity and DIC distributions across the Baltic Sea. This is however only a suggestion. Alternatively, the alkalinity and DIC measurements could be summarized in a Table.

4. With respect to the Black Sea data, it is not clear to me where samples have been taken and what has been analysed. Thank you for clarifying this.

Editorial issues: 1. Paasche (2001), quoted as Paasche (2002) in the text; 2. page 3584, line 26: Holligan, 1993a should be Holligan et al., 1993a

Missing references: 1. Balch (2004) 2. Riebesell and Rost (2004) 1. Ehrenberg (1863)

References listed, but not used in the text: 1. Berge (1962) 2. Birkenes and Braarud (1952) 3. Head et al. (1998) 4. Kleypas et al. (1999) 5. Mihnea (1997) 6. Nausch et

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al. (2004) 7. Riebesell et al (2000) 8. Takahashi et al. (1993)

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