

Interactive comment on “The effects of biogeochemical processes on oceanic conductivity/salinity/density relationships and the characterization of real seawater” by R. Pawlowicz et al.

Anonymous Referee #1

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The paper

“The effects of biogeochemical processes on oceanic conductivity/salinity/density relationships and the characterization of real seawater” by R. Pawlowicz et al.

is recommended for publication in Ocean Science. For the first time, it gives insight into various chemical details of open-ocean composition anomalies and their quantitative effects on density and conductivity. The numerical model results compare satisfactorily with observations and lab experiments. Some, mostly technical, details may be

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improved:

Generally: SI discourages the writing of, say, t (°C) for the value of the temperature given in °C in the form of brackets attached to the variable. Rather, the equation, say, $t = 7$ °C is formally divided by the unit as if it were a quantity, i.e., $t / \text{°C} = 7$. Several expressions in the text and axis labels in the figures should be changed this way to be SI conform, similar to eq. (54).

p. 776: use brackets for the ratio uPS to make clear where the unit belongs to

p. 778: define “chlorinity” or give a reference

p. 778: when you mention the deep North Pacific as a test case, let the reader be aware already before p. 815 that all the equations given here are valid only for surface pressure

p. 778: give a short outlook at the end of the introduction what the subsequent sections are about

p. 779: the “apparent volume” above eq. (7) seems to be an “apparent specific volume”?

p. 779 bottom: the decomposition of a sum in two terms appears trivial. Rather than this, the additivity property suggests that the apparent volumes of the particular ions are approximately the same no matter in which combination / electrolyte they appear. This is more correctly explained on p. 782 and 784 than here

p. 781: “experimental data ... are fitted”: Say that the references for those data are given in the table captions. Give the reader an idea of how many variables were fitted to how many data points, and how large the resulting misfit is.

p. 784: Eq. (18) is strictly true for any electrolyte, not just MgSO₄, and irrelevant even for MgSO₄ if the concentration is very low. Thus, “unequal zero” is trivial and does not properly express what the authors intend to say, namely, that this deviation is relevant

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for MgSO₄ at concentrations of interest here.

p. 794: What is the reason for the particular values of TA and DIC chosen in SSW76 ? May pressure-dependent variations, neglected by the model, of the components of TA and DIC be relevant here? A hint would be suitable here on the reasoning given in the conclusion section.

p. 795: The DIC of surface water is rather variable with region, season and water temperature, e.g., almost 200 $\mu\text{mol/kg}$ amplitude may occur in the Pacific, J. Oceanogr. 61(2005)1075. Is the value chosen here for DIC significant as a better representation of North Atlantic surface water than the Reference Composition? How large is the difference between SSW76 and RC in comparison to the natural spatial and temporal variability? The difference reported on p. 814 is just above 100 $\mu\text{mol/kg}$. A hint would be suitable here on the reasoning given in the conclusion section.

p. 797: TEOS-10 densities are derived from measured data only. No Pitzer models are used.

p.809: add links to the definitions of the different variables, to make the conclusion section better readable without having in mind the entire paper

Interactive comment on Ocean Sci. Discuss., 7, 773, 2010.