Ocean Sci. Discuss., 6, C1121–C1124, 2010
www.ocean-sci-discuss.net/6/C1121/2010/

Ocean Science
Discussions

Interactive comment on "A model for predicting changes in the electrical conductivity, practical salinity, and absolute salinity of seawater due to variations in relative chemical composition" *by* R. Pawlowicz

R. Pawlowicz

rpawlowicz@eos.ubc.ca

Received and published: 4 March 2010

Reply to reviewer # 1

Reviewer 1 queries the reference to thermohaline circulation and suggests deleting it. The intent was to emphasize that important composition changes significantly affecting large-scale density variations (and hence transport) arise due to biogeochemical effects away from the surface, but I agree that this is unnecessary qualification and can be deleted.

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Reply to reviewer 2

Reviewer 2 has 3 questions. The first concerns the difference between constant chlorinity and constant conductivity calculations. The two procedures have different applications. The constant chlorinity case is useful in the case when we begin with an ampoule of standard seawater (SSW) and add material to it. In this situation the initial condition is known (say, practical salinity=35.000), and we are seeking the conductivity of the modified water (say, a practical salinity of 35.010).

However, water samples taken from the ocean do not (in general) have the composition of SSW. Instead we begin with the conductivity measurement of an anomalous water (say, a practical salinity of 35.000), and work backwards to find the ’initial condition’. This water will be composed of a SSW, whose practical salinity would be (say) 34.990, plus added material, and the sum of these must have a practical salinity of 35.000 (the observed value).

In the sense of mathematical formalism the constant conductivity procedure is more ’formally correct’ if we are seeking to correct ocean data.

However, for small perturbations, the size of the conductivity difference will be virtually identical whether we start at 35.000 and add material, or find a water to which added material gives a practical salinity of 35.000 The calculated anomalies will change only by about 10 parts in 35000, which is much smaller than the accuracy we expect from the model (or indeed can achieve for most chemical analyses).

The second question concerns the charge on an ion, which is indeed a typographic error (it should be -2 not -1).

Finally, Reviewer 2 wonders why the predictions of ionic equivalent conductivities are low in Fig. 3. A similar feature can also be seen in Fig 2b. The reason for this behavior is that equivalent conductivities generally decrease with concentration, but along an upward-convex curve. The parametric form used to model this variation is too simple to capture the full curvature, and although (by design) it fits well at low concentrations, it tends to have too little curvature, and hence underpredict, in the relevant concentration and ionic strength ranges for modelling seawater.

Unfortunately, a full solution for this problem is not straightforward. Although the parametric form itself can easily be made more complex so that it could theoretically capture this variation (and I have made some efforts in this regard), determining the extra unknown coefficients is difficult for some constituents due to the lack of available chemical data. Further complications arise when attempts are made to include ion pairing and temperature variations for these more complex parametric forms. Research on this issue is on-going.

Reply to reviewer 3

There are no comments that require additional clarification.

Reply to I. Shkvorets

I believe that I. Shkvorets is making two related points here. First, that my description of 'conductivity' in relation to seawater measurements is oversimplified, and second, that a technically better approach exists to implement a change in numerical values of the salinity scale.

It is true that the description of the details of PSS-78 provided in the paper is highly abbreviated. In fact we do not measure absolute conductivity at all with our instruments, instead we measure the conductivity relative to a calibration standard in a cell of fixed dimensions. The calibration standard itself is 'labeled' based (eventually) on comparisons with a known KCl solution. The procedure is not a strictly traceable standard as it relies on labeled seawater. However, while these technical details are important to the definition of a new standard, they are not critical to the exploratory analysis of this paper.

The issue of how to redefine PSS-78 (or whether one should do so) is an interesting

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one. However, I was not involved in the discussions that led WG-127 to the stated procedure, and so cannot comment on it. Rather, I accepted it as a valid starting point for further discussion.

I will add the reference suggested, and correct my expansion of the acronym PSS-78.

Additional comments.

Since publication of this discussion paper, a programming error was found, in which the molar mass of sulfur (S) rather than the molar mass of Silica (Si) was used in one calculation. The result is that the absolute salinity of NPIW is 35.21108, and the change in absolute salinity of 0.04051 is actually 0.03983 (a change of less than 2%). However, all other values in Table 3 are unchanged.

A second development is that WG-127 (with my participation) has developed a standardized notation to deal with anomalous seawaters, and if a revised submission is requested I will modify the existing notation to conform to this standard.

Interactive comment on Ocean Sci. Discuss., 6, 2861, 2009.