

Interactive comment on “Atlantic Ocean CARINA data: overview and salinity adjustments” by T. Tanhua et al.

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This is one of a series of papers that bring together data from a large number of previously unpublished cruises and integrate them into a series of three regional databases. The authors, a strong international team of researchers, are to be commended on the efforts they have made to try to ensure that the resulting data sets are of the same quality as those collected during WOCE. This has meant adjusting data in several of these cruises to take account of variations in standard concentrations or other calibrations, which may offend the purist, but should allow researchers to compare like with like. Although this paper includes references to a number of different parameters measured

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during these cruises, and includes the adjustments made to these parameter values, we are only given a detailed description of salinity data from the Greenland-Iceland-Faroes ridge to about 10°S, with a few additional stations to 30°S. This is therefore almost a North Atlantic data set.

There are several references to the way inversions were carried out on the individual sections, but as the details are only given in Tanhua et al (2009b), this isn't very helpful at the moment.

I have a question as to why the authors used values on constant pressure surfaces, rather than the more commonly used density surfaces, to make their comparisons (bottom of p. 248). This seems particularly strange given that many of the data come from the Labrador Sea, where deep mixing is common, or from the Mediterranean outflow region, where deep salinity maxima can be found. The authors even mention these points on p. 250. Tanhua et al (2009b) states specifically, in fact, that crossover analysis was generally done on density surfaces except in the Nordic Seas, so there seems to be a discrepancy here.

The authors point out that they have in several cases used multiplicative adjustments, while the GLODAP data set used only additive adjustments. But we are not told why they chose the one estimate over the other. This has been a long-standing argument (e.g., adjustments to the Discovery oxygen data from the Southern Ocean in the 1920s have been adjusted using both methods), so it would be good to know why they chose the adjustment methodology they did. On checking, I found that this is described in some detail in Tanhua et al. 2009b, so maybe there should be a reference to this paper where this is discussed.

Minor comments: 1. p.244, line 22. Do the authors mean World Hydrographic Program or WOCE Hydrographic Program here? 2. p.245, line 9. “Have” not “has.” 3. P. 248, line 22. Tanhua et al. (2009). 2009a or 2009b? 4. P. 249, section 4. I presume the authors are referring only to discrete samples for salinity and their equivalents from the

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CTD record if bottle samples for salinity were not taken. 5. P. 250, line 7. Maybe you could put a reference to the description of this cruise (on p.253) here. 6. P. 253, line 18. "...cruises conducted a long time after..." 7. P.254, line 2. "...but there are only a few deep data..." 8. P. 260, line 10. "MATLAB" not "MATALB". 9. Table 3. What do the second set of numbers after the slash mean for the multiplicative adjustments (e.g., salinity on cruise 29HE19920714 where the numbers are 0/-0-3, or silicate on cruise 317519930704 where they are 1.03/0.4 (2.6)? 10. Fig. 2. Do the year markers in Fig 2a come in the middle of each group of observations or before them? It looks as though they come in the middle. 11. Fig.3. I realize that the authors do not want to waste space and have therefore reduced the size of each segment of Fig. 3. However, at the size shown, it is almost impossible to see where any of the sampling lines went.

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