

## ***Interactive comment on “Data recovery of A06 and A07 WOCE cruises” by N. M. Fajar et al.***

### **Anonymous Referee #5**

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Fajar et al attempt to recover problematic inorganic carbon system data from WOCE sections A06 and A07 that had been discarded by their originators and flagged low quality by data centre operators. Their justification is the data scarcity in the equatorial Atlantic Ocean and, obviously, an interest to document long-term changes in the Atlantic Ocean. Anyone attempting to recover lower quality data for subsequent analyses and interpretation is to be applauded. These attempts, though, should be made with a clear perception of the role of original measurements, data sets, calibrations, adjustments, meta data documentation. These reconstructed/retrieved data will never replace the original, possibly flawed data, but as a new version of existing data allow another interpretation. These versions need to be distinctly marked and kept separately. This stringent logic is unknown or not relevant to the authors. They replace, change, adjust. . . They also use methodologies probably no suitable, and are not sure about the objectives and rationale of the different data bases, such as WOCE WHP,

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CCHDC, CARINA, GLODAP, . . . All this is barely accessible to the reader because of serious language problems. Partly the text reads as if dictated, partly it has never even been proof-read by the authors. And most seriously it has never been subjected to the scrutiny of a native-English reader. So what is left with the reviewer? Guess what the authors intended but couldn't argue, detect the flaw in their arguments behind a curtain of unclear language? The minimum a reviewer can expect from an author – and from the editor, too – is a readable manuscript. All else inhibits understanding. Am I to re-write the paper just to understand it? (1) The methodology they use has been used before. The argument to “produce suitable pH results if the CT/AT ratio is valid” is not stringent since they do not show the error bars for AT and CT. So they implicitly assume the errors in both measurements are such that the ratio is stable? What does the original documentation of the measurements of A06 and A07 say? Why were they discarded? The reference to Tanhua (2010) is the main documentation of their approach, but they use here the (3-DwMLR) referenced to in Velo et al (2011). This paper is submitted, therefore not available to the reader or reviewer. The reference should be struck out, a paragraph outlining the improvements over Tanhua 2010 added. They still do not prove that their application of the method allows substituting MLR results for measured data. (2) Regarding the cross-over analysis they simply follow a technical approach. They seem to ignore the constraints as there are parameter choice, variability, length scales. Understanding this might result in robust results. I suggest they read Gouretski and Jancke 2001. More importantly they do not indicate why they choose equal pressure surface and not density surfaces. Did they ever consider it? (3) The whole exercise results in “smoother” plots (fig.3, fig .5). Did they analyse the metadata of the original data to find out why they were discarded. Why do they have this high spatial noise in their plots? What is systematic in the errors and what is random. To judge the plots they show (3a,b; 5a,b) I suggest they mark station positions and sample depths.

To get a better understanding of the actual measurements and their presentation they should visit For A06: <http://www->

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And in addition look at the meridional WOCE section A16 [http://www-pord.ucsd.edu/whp\\_atlas/atlantic/a16/sections/printatlas/A16\\_TCARBn.pdf](http://www-pord.ucsd.edu/whp_atlas/atlantic/a16/sections/printatlas/A16_TCARBn.pdf)

In all I strongly suggest a complete re-write with an emphasis on understandable English. Taking the reviewers comments they need to argue convincingly what they intend to achieve and how they did. This could lead to a stringent argumentation. If so this paper could be considered for publication in *essd*, subject to another favourable review.

Only in that case I am willing to have another look at the manuscript again. I am still sceptical that there are generic flaws in the approach as I understand it now. The re-write might help.

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Interactive comment on *Earth Syst. Sci. Data Discuss.*, 4, 99, 2011.

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