Ocean Science
Discussions

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Interactive comment on "Detailed temperature–salinity distribution in the Northeast Atlantic from ship and Argo vertical casts" *by* I. Bashmachnikov et al.

Anonymous Referee #1

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A new ocean analysis is presented, which is compared to other products (T, S, density) in the northeast Atlantic. The comparison is rather favorable, and suggests that most issues with data quality, interpolation, resolution of the important horizontal scales of variability have been tackled. It is thus a valuable effort and the resulting analysis is worth presenting. Nonetheless, there are a few points, mostly methodological, which require further explanation or that might not be optimal.

The filtering out of some of the data: the filtering out of meddies and nearby areas could result in lowering artificially the average S away from continental slope areas, thus increasing the horizontal gradients in the slope region. Could this be quantified? What percentage of profiles is removed that way and can the effect be quantified?

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Eliminating the sum of T and S deviations in the 1500 and 2000m for OSD profiles originating from XCTDs. This is interesting: a reference on depth estimation for XCTDs should be provided (I did not know that they were reaching so often below 1500m). What does close to zero refers to (be more quantitative?)

Removing PFL profiles due to salinity sensor drift. I suggest to use for Argo the Coriolis data base (also the way the corrections are applied in Coriolis should avoid artificial shifts of S by the correction methods which seems the case in the Argo DAC). At least compare the two (does the sorting out of the data has some effect)?

A lot of effort has been done to develop the adjusted beta-effect automatic sorting. This is interesting; how smooth needs the bathymetry be for the algorithm to work? However, on page 1485, line 19, two modes in R are commented, but the following discussion only focuses on the larger of the two. What about the smaller one? (I also noticed that in the later choices, even near the surface where data density is rather high, R retained is larger)

A few minor comments In the title of the paper, I suggest to drop 'Detailed'

p. 1490 l. 2: I am not sure I understand where and how the reference level is chosen. Is it the same for the different climatologies? What is the spatial pattern of this layer? The comparison might not be a proof, as the mean circulation is to some extent reconstructed from hydrography assuming a reference level (and also drifter data; depending on the version with different characteristics, in particular for inclusion of Gravimetric mission data).

p. 1490, l. 20: for quasi-stationary meanders of Azores Current, when mentioning where they are, it would be good to check if they are positioned in same place in AVISO product. Are these real features or resulting of inappropriate sampling of varying meanders... (we see afterwards that it was only present in winter season for Medtrans; is it similar in Aviso?). Also, here I gather that geostrophy and not cyclostrophy is used. The discussion of bottom of page 1490 and top 1491 is interesting (agestrophy), but it

would be useful in that case to estimate directly the cyclostrophic currents.

p. 1492, l. 9 and later, 'African coast' is a bit vague. It would be good to add latitude ranges. Bottom p. 1492. It would be interesting to know how much the currents at 700m are influenced by reference level. 1600m is also presented (which is even stranger/less reliable) for seasonal cycle.

p. 1496, l. 9 'to the northwest' (instead of 'to the northeast')

Throughout the text quite a few geographic or bathimetric features are cited, which would require them to be reported on at least one of the figures.

Interactive comment on Ocean Sci. Discuss., 11, 1473, 2014.

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