

Response to comment on the review.
Sjoerd Groeskamp, 18-12-2017.

Dear Hochet et al, thanks for your comments on my review.

Before I provide a few specific and final comments, I need to address certain aspects of the comments to the review.

In general, reviewers are professionals and arguably experts in the field. In addition, they spent several hours (or more) to understand your manuscript and provide a review that can be used to improve the accuracy, presentation and impact of the reviewed work. Of course, I do not expect authors to fully agree with every review, but I do at the very least expect authors to appreciate the attempt to provide a review that improves the accuracy, presentation and impact of the presented work.

The response to the review of Hochet et al., is focused almost entirely to argue against the review comments. In addition, the comments are at some point even called “a jest”. This is unprofessional and will not benefit the review process, nor the improvements of the manuscript.

Even when the authors disagree with the review comments, authors should realize that if the reviewer(s) do not fully understand their manuscript after careful reading, many others will not either. Time would be better spent by providing a more carefully and clearer written manuscript, then only arguing against the review comments.

With that said, below are my final comments that explain my statement above and provide some more scientific questions.

About General comment number 1 (around C2)

First of all, perhaps I choose the wording poorly, i.e. “oversell” and “holy grail” and should have phrased my response more appropriately. I apologies. And indeed, I agree finding a materially conserved and as neutral as possible variable, is worthwhile.

This comment meant to address the issue that a clearer statement and discussion could be provided on the concept that variables are 1) either materially conserved, but neutrality optimized (and thus not exact), or 2) variables can be Neutral, but conservation is optimized (and not exact). Perhaps explain how this affects the interpretation of the results for the different products, and discuss why it is not said that one is better than the other, but it is simply a different tradeoff.

About the second general comment, on salinity.

The reviewer does not provide time-consuming professional reviews of scientific work, “for a jest”.

First of all, I would like to refer the authors to the Editorial note from the Journal of Physical (Spall et al, 2013). Admittedly, I could not find a comparable note from Ocean Sciences, but it may exist as it is available for many oceanographic journals. This note urges oceanographers

to use the TEOS-10 variables for observationally based work. Hence, my comment and the editorial notes was serious, “not a jest” as suggested by the authors.

Secondly, the whole and simple point of this comments is only to specify the salinity that is used. Currently it is specified as (last line, page 1):

... and S the (practical) salinity, ...

Hence, it is not clear which salinity is used. To reword it in terms of temperature as in the authors their response, it is not clear if you used Fahrenheit, Celsius or Kelvin. I don't mind which one you use, as long as it is clear and well-motivated and with the associated notation (S_p if it is practical salinity).

General comment at the bottom of C4 about KH and KV.

The authors state that it has been ‘clear’ that horizontal/vertical diffusion was applied in old models. The reviewer did not experience this to be so. Perhaps explain more clearly why one would go back to “old model” way of describing a diffusion tensor with K_h and K_v , while progress has been made by using a rotated tensor? Perhaps consider using Eq. (4) of Klocker et al (2009, A new method for forming approximately neutral surfaces) which provides a similar measure, but using isopycnal diffusion. If the authors have convincing arguments to stay with their current method, then that is fine, but that would still warrant a discussion between the two approaches and a motivation why to use the final chosen method.

The comment of C5, that follows from the previous comment.

I think that K^* in the comment described the Redi-tensor, which is different than using the isotropic version. That was exactly the point of the review comment, as described on page 2173 of McDougall et al 2014.

Comment C6

I now agree that I indeed pointed out things related to Isoneutral/isopycnal, etc. that were in fact already correct in the manuscript.

Comment around C9 about the Munk value.

The Munk number may be canonical, but we now know that that number is a combination of mixing at hotspots, and a smaller global background-type mixing. The latter is much smaller, while the former is much larger. So, it may be worth spending a few words on how much it means to compare this to 10^{-4} , knowing there are large spatial variations that may change the interpretation of the results. Also note that the Groeskamp et al (2017) estimates are global, while Waterhouse et al (2014) is a collection of local measurements.

Michael A. Spall and Karen Heywood and William Kessler and Eric Kunze and Parker MacCready and Jerome A. Smith and Kevin Speer and Mark E. Fernau. EDITORIAL. Journal of Physical Oceanography, 43 (5), 2013, doi:10.1175/JPO-D-13-082.1