

Interactive comment on “Understanding mixing efficiency in the oceans: do the nonlinearities of the equation of state for seawater matter?” by R. Tailleux

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Received and published: 10 March 2009

I countermand the first two formal manuscript ratings as provisional only, since I cannot assess them without the third being improved. (The website forbids me from abstaining as regards ratings: it compels me to fill in something.)

I think this manuscript should be rejected, but reconsidered after (a) being rewritten in a much clearer way, and (b) resubmitted together with the unpublished manuscript on whose details everything seems to depend (Tailleux 2008). It is impossible to review the present manuscript without seeing the other one. Nor would it be possible for those viewing the public discussion to have any idea of what's being discussed. I trust the

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EGU website has the ability to display auxiliary manuscripts; if it does not have that ability, then a meaningful public discussion will hardly be possible in this case.

For clarity, specialist notations should be defined where they first occur. Among many examples I could mention "D(APE)" in the second line of the abstract, suffix "r" in the third line, and "G(KE)" in eq (3) (I'm guessing that "G(KE)" is meant to be the same as epsilon, but why should I be made to guess?). I can also guess what suffix "r" means; and if my guess is correct the definition is highly nontrivial, and not in the slightest obvious.

Another need is to make the definitions of γ_{mixing} mutually consistent. At present, (15) is inconsistent with (1). It is unfortunate that Osborn's original paper didn't clarify that the coefficient in (1) is not a true efficiency (whose numerical range is 0 to 1); that's why Peltier and Caulfield (and other respected practitioners such as Jim Riley) have recently adopted definitions more like (15).

Interactive comment on Ocean Sci. Discuss., 6, 371, 2009.

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