**Ocean Science
Discussions**

Interactive comment on "Climatological mean distribution of specific entropy in the oceans" *by* Z. Gan et al.

Anonymous Referee #1

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REVIEW OF THE OCEAN SCIENCE DISCUSSION MANUSCRIPT

"CLIMATOLOGICAL MEAN DISTRIBUTION OF SPECIFIC ENTROPY IN THE OCEANS" BY GAN, YAN AND QI

This manuscript is concerned with dispelling a non-issue, namely that isentropic surfaces are different to potential density surfaces. It makes basic thermodynamic errors and it should not be published. From basic thermodynamic theory and from Feistel (2003) it is well known that specific entropy is very similar to potential temperature. The manuscript presents Figures 1 to 5 (17 panels) showing maps and cross sections of specific entropy and comparing these to maps of potential temperature and potential density, whereas it is well known that specific entropy and potential density are

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quite different things. The manuscript is wrong-headed, chasing the false premise that oceanographers "commonly" assume that a potential density surface is an approximation to an isentropic surface. Oceanographers do not assume this; rather it is a false assumption of the authors.

In summary, the manuscripts rests a false "common assumption" and then debunks it with all the maps from the ocean, whereas in fact, oceanographers have never assumed that potential density is similar to specific entropy. In addition, there are two fundamental thermodynamic errors in the manuscript. The same manuscript was submitted last year to JGR. It was rejected from that journal. The authors have learnt nothing from the reviews they received at that time, and this manuscript should be rejected from the present journal. The rest of this review fleshes out the problems that I have found in the manuscript.

In the abstract and in several other places the straw man is erected that "the traditional assumption [is] that isopycnal or neutral surfaces could be approximately regarded as isentropic surfaces in physical oceanography". This is false. Oceanographers do not assume that specific entropy is approximately the same as potential density, as the authors assert. It is true that, due to the roots of oceanography having come from meteorology, some of the older oceanographers used the phase "isentropic analysis" as a shorthand for what is really meant, namely "mapping properties on a constant density surface". In the atmosphere the relevant surface is an isentrope which is also a surface of constant potential temperature, which is also a potential density surface, and so there is no need for a distinction. In the ocean, salinity also contributes to density in an important way, and so the link between entropy and density is broken. The authors are attempting to educate oceanographers that an isopycnal is not the same as an isentrope; as far as I know, no one thought they were they the same, so the authors are addressing a non-problem. Hence Sections 1-4, pages 1-5 and Figures 1-5 are all unnecessary.

The last panel in Figure 5 and Figure 6 demonstrates that specific entropy is mostly a

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function of potential temperature. This is well known. This can be seen from Table 14 (at p = 0) of Feistel (2003). This can be seen from Equation (28) of Fofonoff (1962) (In The Sea) where it is seen that the specific heat is evaluated at zero pressure, as is the partial derivative of the chemical potential with respect to temperature. Note that this is different to the author's equation (4) which is wrong; the coefficient of the differential of salinity is not -mu/T, but rather is the partical derivative of mu with respect to potential temperature. That specific entropy is close to being a linear function of the natural logarithm of potential temperature is well known. The last panel of Figure 5 and Figure 6 merely confirm what is already known.

Another basic thermodynamic error that is made in the manuscript is in the introduction where it says that prior to the excellent work of Feistel (2003) specific entropy was only known up to a linear function of salinity because of the complicated thermodynamic nature of seawater. This is incorrect. The fact is that specific entropy is UNKNOWABLE up to a linear function of salinity. That is, whatever specific entropy one cares to define can have a constant and a linear function of salinity added to it, and no thermodynamic measurement or experiment can distinguish between the two different definitions of entropy. Both are equally good definitions of entropy.

An important issue remains undiscussed in this manuscript, and it an issue at the heart of our use of thermodynamic variables in physical oceanography. This is the issue of conservative-ness. That is, when two fluid parcels are mixed, what properties are conserved? Fofonoff discusses this issue at length. Mass is conserved, salt is conserved, and enthalpy is conserved when fluid parcels are mixed at constant pressure. We know that entropy is not conserved (this follows from the Second Law of thermodynamics) and we also know that potential temperature is not conserved. Since entropy is not conserved, why would physical oceanographers want to concern themselves with specific entropy?

In summary, this manuscript chases a false premise and it illustrates this false premise with many figures, all of which are unnecessary. There are two basic thermodynamic

errors in the manuscript. There is no redeeming feature of this manuscript that would argue for a re-write and re-submission. Rather, it should be flatly rejected by the journal.

Interactive comment on Ocean Sci. Discuss., 4, 129, 2007.

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