Ocean Sci. Discuss., 4, S519–S523, 2008
www.ocean-sci-discuss.net/4/S519/2008/

Ocean Science
Discussions

Interactive comment on "On available energy in the ocean and its application to the Barents Sea" *by* R. C. Levine and D. J. Webb

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Received and published: 22 July 2008

We would like to thank Rainer Feistel for both his general and detailed comments. The comments mainly concern the derivation of section 2 and 3 - and unfortunately they seem to be seriously affected by a misunderstanding of Eqn. 10. We need to make this part of the paper clearer.

General Comments:

1. We agree that, where there is a stream line connecting the points, the first part of the derivation is well known, the equation appearing in different text books under various disguises (Landau Lifshitz, 1959, eqn 9.3; Pippard, 1961, eqn 6.35; Gill 1982, 4.8.2). The equation is also implicit in Maxwell's thermodynamic relations (Maxwell, 1871, Theory of Heat).

We therefor could have started with equations 8 or 9 and, after dealing with the virtual stream line problem, proceeded from there. However most oceanographers are not experts on enthalpy and our short derivation from first principals also helps to get some of the physical concepts across (see also the detailed responses below). Maybe placing the section up to equation 8 in an Appendix would be an acceptable compromise.

2. Dr Feistel suggests deriving the relationship from the Bernoulli function. In fact equation 8 is essentially the Bernoulli function, but only when the fluid is inviscid and diffusive effects can be ignored (and the two points are joined by a streamline). Arguing backwards is more difficult and it is noticeable that none of the above text books use this route.

3. The review points out that the final equations of section 3 are less accurate than equation 9. This also is correct but the primary aim of section 3 is to give a physical interpretation to the enthalpy difference, i.e that it splits into a contribution from the horizontal pressure field plus an Archimedes term. Most oceanographers 'know' that individually these terms are involved but will be unclear as to how they are involved and whether they are sufficient. As far as we know it is an original result.

In our case we found that the split was very useful for understanding physically what is happening in the Barents Sea region. Numerically it was useful because the model does not use an equation of state based on the Gibbs function. An accurate enthalpy function is therefor not available.

OCCAM, like most ocean models, includes the effect of compressibility when calculating the pressure field but uses a continuity equation that assumes the ocean is incompressible. This is equivalent to treating the specific volume term V of Eqn. 13 as a constant but allowing ρ and ρ_0 to vary. As a result for the model fields, Eqns. 19 and 20 are exact.

4. The reviewer comments on the change in Available Energy that occurs when adjacent water masses mix. A full discussion was thought to be too diverting to be included

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in the present paper but some useful examples are discussed in Levine (2005) (See http://eprints.soton.ac.uk/25111/).

5. Many of the detailed comments, and possibly some of the above general comments, seem to have arisen from a misreading of Eqn 10. In this equation H is not the in situ enthalpy of the ocean. Instead it is the enthalpy of the initial fluid particle when moved adiabatically from its initial position to some other point in the ocean. The same is true for the specific volume V and internal energy U in Eqn. 10.

These points will be made clearer in the final manuscript.

Detailed Comments:

The reviewers page numbers seem to be out by one. The following responses refer to the actual page numbers for the equations etc.

p 898, l 9. Our mistake. Corrected.

p 899, l 5. Point well made. The model does include ice formation and melting and at the beginning of winter this will have an effect on the salinity of the Barents Sea. The main model result is that the impact is negligible compared with the strong contrasts due to the saline North Atlantic inflow and the fresh water input from the rivers.

p 899, l 7. At this point we are really talking about the energy that arises from compression. As is discussed in the paper it is not included in the standard atmospheric definition of available potential energy but there is no a priori reason to think that it should not be included when discussing the ocean.

p 900, l 9. We do not understand this comment. It conflicts with the later discussion of Eqn 10 which agrees that the integral is independent of the path chosen.

p901, eq 2. Yes, the chemical potential terms could be added.

p901, eq 4. No. An infinitesimal amount of water is being moved from a reservoir outside the ocean to a point within a 3-D ocean. However because of the change in pressure, the infinitesimal amount of water has a change in volume that has to be allowed for. The volume element can have any shape in the reservoir. In the ocean, we assume that it stays near the injection point and that the pressure equals that of the injection point, i.e. the fluid lies in a thin essentially constant pressure layer.

p 902, l 6. The concept of raising the whole column by a certain distance is not needed when deriving equation 6 from Eqn. 5, but some readers may find it a useful concept when trying to understand where the energy is stored. We feel that changing the sentance to "If the column has area A, it is raised by a distance V/A", is less elegant and potentially more confusing.

p 902, l 10. No, the water comes from an external reservoir. If all the water below the injection point is incompressible, then the only effect of adding water is to increase sea level. However if it is compressible, then the small increase in weight will compress the water below the injection point, so sea level will not increase quite so much.

p 902, eq 9. See the general response above.

p 903, l 7. This comment was designed to emphasises the problem of starting from Bernoulli. We obviously failed. It is supposed to read that if the points are joined by streamlines, etc. (previous paragraph) then Bernoulli's results can be used. However when they are not joined by streamlines, etc. (this paragraph) we cannot use Bernoulli but available energy, as defined earlier, can still be used.

p 903, eq 10. Yes, z-bar is the unit vertical vector. We will define this in the revised manuscript.

As discussed in general comment 5 above, the integral does not involve the 'in situ' oceanic enthalpy. Thus the counter-example given is not valid.

p 903, eq 11; p 903 eq 12; p 904, l 2; p 904, eq 13; p 904, l 11. See general comment 5 above.

p 905, l 5. No. All we are saying is that because the density of the ocean varies by

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only a few percent, the specific volume term in Eqns. 17 and 18 are, to first order, a constant and that within this approximation Eqn 19 and 20 are true. Maybe we need to be more pedantic and emphasise that we are not assuming that the density contribution to pressure is constant everywhere.

p 905, l 10. We could approximate V in equation 11 by a constant, but it would not save much space in the paper and the reader would not see the effect of the approximation on the exact equations 17 and 18.

On the second point, we suspect that our method needs to be made clearer and that once this has been done a second derivation of the equations will not be considered necessary.

p 914, l 26. Yes, P_r should be 1 atmosphere.

p 915, l 4. It may be better to swap the argument so that the ocean is emptied at the bottom and moved to a reservoir where the pressure everywhere equals P_r. $U_1(x, y, z)$ is then the internal energy that water from point (x, y, z) has in the reservoir.

p 915 eq A3. We are sorry that the argument is not clear. We do not claim the final result (A4) to be original. It is included here in an Appendix mainly for readers who want to understand, i.e. not just be told, what Eqn. 1 should be.

References

Gill, A.E., 1982: Atmosphere-Ocean Dynamics. Academic Press, 662 pp.
Landau, L. and Lifshitz, E., 1959: Fluid mechanics, Pergamon Press, 536 pp.
Pippard, A., 1961: Elements of Classical Thermodynamics, Cambridge University Press, 165 pp.

Interactive comment on Ocean Sci. Discuss., 4, 897, 2007.