Ocean Sci. Discuss., 4, S524–S527, 2008
www.ocean-sci-discuss.net/4/S524/2008/

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

The reviewer's comments are focused on sections two and three of the paper and are primarily concerned with the originality of the results and the relationship with previous and other current work.

1. On the question of previous work we strongly agree that when it comes to enthalpy 'many people have written papers on the subject'. In fact as enthalpy is a measure of the amount of energy available from a steam boiler we suspect that its use dates well before the time of Maxwell's relations (1871) and may have been fundamental in the development of thermodynamics

In the oceanographic literature, our impression is that, enthalpy tends to be treated as a useful variable (c.f. Gill, 1982, Eqn 3.2.10 and 4.8.3; Reid et al, 1981. Eqn 4 and 6)

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but with little physical understanding. We believe this arises because it is usually only of incidental interest to author's main argument.

In the present work our main interest was in analysing model data to understand how processes occurring within the Barents Sea contributed to the Arctic boundary current. We were concerned with the different physical mechanisms and so were more interrested in the physics behind the equations than the equations themselves. As such, we felt that a short derivation from first principals was of more help to the new reader. We referenced basic thermodynamics books but shied away from papers whose main interests lay in other directions.

In retrospect we were at fault in not flagging section 2 as a re-derivation of a standard result. However we still think it is useful for a new reader so propose to include it as an Appendix in a revised manuscript.

The derivation in section 3 we believe may be new, at least in the oceanographic literature - although with a subject as old as thermodynamics something similar is almost certain to have been derived in another context. We found the split to be particularly useful for making sense of the model results and so intend to keep it in the revised manuscript.

2. In writing the paper we were very conscious of the early work of Reid, Fofonoff and co-workers (one of us worked in the same department as Fofonoff for a number of years). However we were also conscious that their work was mainly to do with the experimental determination of APE at sea where the geopotential is not known. This is difficult to do and adds much complexity. From our point of view their main contribution to our Barents Sea problem came from the insight into the conservation of enthalpy under mixing at constant pressure - but then again you might say that when their papers were published the result was already well known.

Their interest, as with most of the other later authors referred to, also concerned the total amount of energy available relative to a spun down ocean. This is of great interest

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in itself and because of possible multiple spun down states it is very difficult to calculate - but it isn't the problem that concerned us.

Our problem is much more direct, we were interrested in the energy of the shelf water relative to a larger volume of water offshore. For this the minimum energy state of the whole ocean is not relevant and, as the volume of the offshore water mass is so much larger and its definition qualitative, the complexity of defining a joint minimum state (of the shelf and offshore waters) is not justified.

As a result none of the other papers struck us as providing a real insight useful for the present work. Maybe we were wrong in making this judgement and will have to rethink the references to include in the final paper.

3. On the other points:

3.1 The concept of internal energy was included as it is fundamental to all thermodynamics. Outside the introduction it is included in Eqns. 2, 3 and 7.

3.2 The reason the paper does not follow previous approaches is explained above. We do not agree that our definition of available energy needs to be changed into a global quantity as the energetics of the Arctic Ocean Boundary Current depend only on the properties of local water masses and not on the stratification of distant oceans.

3.3 'This reduction of the gravitational potential energy of the original water column' is included in the paper.

If a volume of water V_1 is extracted from the bottom of a column of water at pressure P_1, then the reduction of gravitational potential energy of the column is exactly the term P_1V_1 of Eqn. 4. If a unit mass of fluid is moved horizontally sideways a distance ds into a neighbouring column where the pressure is slightly different then the potential energy change is described by the term $ds \cdot V\nabla P$ of Eqn 12, where V is the specific volume and P the pressure.

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References

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Reid, R.O., Elliott, B.A., Olson, D.B., 1981: Available Potential Energy: A Clarification. Journal of Physical Oceanography, 11, 15-29.

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