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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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The paper consists of a theoretical part about "Available Energy"; and an application part of this concept to the oceanography of the Barents Sea. While I have no objections regarding the latter section, I suggest a major revision of the first part.

The aim of the theoretical part is well stated in the first sentence on page 899, as to "investigate the energy released when a parcel of water is transferred from one region of ocean, at a depth z1 and pressure P1, to a second region of ocean, at depth z2 and pressure"

The solution to this problem is well known and does not require to be re-invented. The presentation in the paper does not add anything new to this state of knowledge, instead, it contains numerous unnecessary weaknesses, assumptions and approximations.

The solution is properly mentioned on page 903, namely that "if the two points are joined by a streamline and if the flow is both inviscid and adiabatic, then the available energy equals the change in kinetic energy per unit mass (Landau and Lifshitz, 1959) along the streamline." For unclear reasons, however, this available theory is not applied in a straightforward manner.

My suggestion for this part is as follows:

The Bernoulli function b,

$b = h' + v^2/2$ (x1)

is conserved along a streamline if the flow conserves entropy and salinity, and if the pressure field is stationary (Gill 1982, section 4.8). Here h' = h + gz is the specific gravitational enthalpy and h the specific enthalpy. Thus, the gain of kinetic energy of a parcel moving from position 1 to 2 is, because of b1 = b2

delta Ekin = h'2 - h'1 (x2)

This simple expression is more accurate than eqs (19) and (20) used in the paper later (page 907, line 16) and can be their substitute since enthalpy is numerically available for seawater (Feistel 2005). If the authors insist in splitting the complete path of (x2) into a horizontal and a vertical step, they can easily introduce any intermediate point into (x2).

The energy (x2) consists of both turbulent and advective kinetic energy. Since the current speed can be estimated from the geostrophic velocity available from the pressure gradient and the SSH, even conclusions on the mixing intensity seem possible, thus enriching the paper.

If entropy and salinity at point 2 are different from point 1, mixing has occurred along the path. To estimate this, one may look for a second (or several) parcel at 1* that may be the counterpart for the mixing, suitably adjusting the mass ratio between the parcels to get the salinity-entropy properties observed at 2 from those by mixing 1 with 1*. We can imagine 1 and 1* propagating adiabatically to point 2, and then mix at that point to produce the entropy-salinity values observed at 2. This final isobaric mixing conserves enthalpy, thus the gain of kinetic energy can again be computed from the weighted sum of the Bernoulli functions of both initial parcels. This mechanism is explained in detail in the concept of Potential Enthalpy (McDougall 2003).

Thus, in this suggested revision the only assumption needed is stationarity of the pressure field. This is much less restrictive than what is assumed in the present version of the paper. The quantitative results of both approaches should be rather similar, though.

References: Feistel, R. 2005: Numerical Implementation and Oceanographic Application of the Gibbs Thermodynamic Potential of Seawater. Ocean Science 1, 9-16. http://www.ocean-science.net/os/1/9

Gill, A. 1982: Atmosphere-Ocean Dynamics. Academic Press.

McDougall, T.J. 2003: Potential Enthalpy. J. Phys. Oceanogr., 33, 945-963

Some concerns about the existing theoretical part are:

p. 897 l. 9: The word enstrophy does not occur in the entire paper. Write enthalpy.

p 898 l. 5: do not promise results regarding ice formation. Ice does not appear in the paper.

p. 898 l. 7 : here and many more times it is referred to internal energy, but actually internal energy was never evaluated in the paper. All that was considered is compression work. Both are directly related only if no mixing occurs. But the absence of mixing is not verified in the paper, on the contrary, mixing is claimed to be included (p. 902, l. 8).

p. 899 l 9: I have not found a prove for the path independence in the later text. For this,

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one needs to compare two arbitrary but different paths. The consideration on page 902 is contradictory in this respect, see below.

p. 900 eq. (1) Internal energy can also change by exchange of salt and water, rather than only by work and heat, as claimed here.

p. 900 eq. (4) This equation assumes the extreme simplification that the displacement happens in a cylindrical tube, that the parcel has the same cross section before and after the transfer, that no horizontal motion is possible, and that a cylindrical bump appears at the ocean surface. None of this is true in reality. The parcel will be sqeezed horizontally. The energy change will depend not only on the volume, but also on the shape of the parcel. Horizontal gradients have an influence. The model must be 3D rather than 1D.

p. 901 l. 6: Tricks like "raised by a distance V1" are confusing to the reader and error-prone to the author. Better use a distance and an area variable.

p. 901 line 10: The deeper water is not compressed since the same mass injected now was removed before at a different level. Rather, the water column between the removal and the injection point expands and lifts up the sea surface. This effect is ignored by the authors.

p. 901 eq. (9) is just the same as equation (x2) above. No long and intricate argumentation is needed to get there.

p 902 l 7: If two points are not linked by a streamline you may no longer talk of a parcel displacement. This leads into severe difficulties, see below.

p. 902 eq. (10) Explain the z used therein (the vertical unity vector, I assume). Since the integrand is a gradient, the integral does not depend on the integration path and gives the difference between the gravitational enthalpies of the ending points, namely exactly eq. (9). If these points are connected by a trajectory, this integral equals the result (x2) from the Bernoulli function. Apparently the intention of the authors is to gen-

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eralise this formula, but this makes little sense to me. Say, we have two neighbouring columns of water, both with identical vertical density profiles, but one much hotter and saltier than the other. Since there are no horizontal pressure or density gradients, the system is hydrostatic (even though it may be unstable with respect to certain fluctuations). Now we use the integral (10) along a horizontal path. Since the enthalpies of both columns are different, the integral can give an arbitrarily very big value. But what does that value have to do with available energy? I miss an explanation here.

p. 902 eq. (11) This equation is wrong since you ignore the spatial gradients of salinity and entropy resulting from the gradient of enthalpy. If there were no such gradients, the available energy would always be zero, trivially. While a Lagrangean process on a streamline (eq. 3) can be adiabatic, this does not at all imply that the (Eulerian) ocean is isentropic and isohaline everywhere.

p. 902 eq. (12) This equation has a clear physical meaning. It is an arbitrary path integral over the right-hand side of the Euler equation. But this is qualitatively different from eq. (10), in particular, this integral depends on the integration path if the integrability condition grad(V) x grad(P) = 0 is not fulfilled, i.e. wherever the ocean is baroclinic.

In the stationary case, upon integrating the Euler equation, this integral is equal to the difference of the kinetic energies between the endpoints of the integral, plus a path-dependent integral over the vorticity vector field. Thus, this is a different kind of "Available Energy" than introduced before. Even for a closed integration path it is nonzero.

p. 903 l. 2: If the ocean is hydrostatic, v = 0, the integral (12) will vanish identically, and (13), too.

p. 903 eq. (13) With "ambient density field" the authors seem to mean the same ocean but the field outside the integral path s ? If so, the density field is supposed to be discontinous outside s? For any arbitrary s chosen? I have severe problems to follow this derivation.

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p. 903 l. 11: Here the authors talk again about a water parcel moving from one place to another, i.e. they consider a streamline. I understood section 3 as a theory for the case when the points are NOT connected by a path along which a parcel is transferred. My suggestion is to drop this "field theory" and stay with the displacement of a parcel.

p.904 l. 5: The assumption of a constant density of the ocean is very restrictive. Perhaps it is meant that the density is approximately constant in a layer of constant depth ? This would of course imply that also the hydrostatic pressures (vertical density integrals) at that depth are the same everywhere, letting (19) vanish. Thus, a better argument is required to show that (19) is a good approximation of (17) under the conditions given.

p. 904 l. 10: If the changes of internal energy are completely ignored in the final formula, the lengthy discussion of this issue in the preceding sections can be completely omitted.

It may still be that the equations (19) and (20) finally used for the Barents Sea are a reasonable simplification of the correct expression (x2) applied to some finite volume. But for this purpose it is necessary to show in the theoretical part that the neglected terms are small compared to those kept within the relation under the conditions met in the application region. (if not simply the full expression (x2) is used).

p. 914 eq. (A1): $z = 0$ in the "reservoir" with $P = 0$ is apparently meant as the gauge pressure P. The pressure P in $H = U + PV$, however, is the absolute pressure, i.e. 1 atmosphere.

p.914 line 4: Why does the internal energy in the reservoir depend on the coordinates x, y, z ?

p. 914 eq. (A3): The filling procedure of the ocean constructed to get eqs. (A1) and (A2) is suspicious and not clear to me. Despite of this, (A3) is a trivial statement in that the total energy of any ocean is the sum of its internal, potential and kinetic energy.

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From this statement follows (A4) immediately without the need for reservoirs and filling oceans.

Some misprints etc:

p. 897, l. 7 principals ?

p. 898 l. 8 Bernouilli's ?

p. 906 l. 2 non-existant ?

p. 912 l. 22 Alantic inflow ?

PS. Just recently, by lucky coincidence, on the 401st Heraeus Seminar on "Evolution and Physics" I met David Blaschke, who gave a talk there about the early universe. http://www.virtualknowledgestudio.nl/staff/andrea-scharnhorst/heraeus.php He was pleased to see that he and his Armenian colleagues are cited in an oceanographic paper (first reference therein). I welcome this, too.

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