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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

Anonymous Referee #3

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A review of On available energy in the ocean and its application to the Barents Sea by R. Levine and D. Webb

The general topic of energetics is revisited and the authors suggest a way to compute available energy of one location relative to another. The analysis proceeds assuming adiabatic transitions, in keeping with the classical derivation of available potential energy, and breaks the result into two pieces, one due to depth change and one due to density change. The procedure is then applied to several stations in and around a numerical Barents Sea, with the authors examining the energetics structure and history of flow from the Atlantic to the Arctic Ocean. It is concluded that cooling at mid-basin in the Barents Sea plays a critical role in affecting the AE of the throughput, increasing the AE relative to the Arctic significantly. Then, it is argued, this reservoir is drained by

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bottom drag as the water moves to the St. Annes Trough. AE is advanced as a tracer for following water masses and as a useful diagnostic. The paper concludes with a discussion of the role and importance of turbulence.

There are some nice ideas in this paper, mostly in the application part (the development seems less novel, with a reasonable number of previous attempts appearing in the literature), and the potential for the approach recommended herein to give us useful insights into ocean dynamics. But, I do have a few concerns as outlined below. In so many words, energy as a tracer in the ocean seems to me to suffer from some drawbacks. On the other hand, if issues such as external sources and sinks can be constrained, the approach advocated here by the authors could well lead to some useful results. I outline my concerns below in the hopes that the issues I have found confusing can be remedied.

A few minor comments about the paper:

First there are several typos and misspellings which do impede the progress of a careful reader.

Second, not everyone reading this paper will be immediately familiar with the regional geography. It would be very helpful to a reader if a map where things like the Spitzbergen Current and the East Greenland Current and the Bear Island Trough were labeled. Having said this, the authors do a nice job in describing the overall hydrography and descriptive physical oceanography of the area. Latitude and longitude are frequently used for orientation in the paper. This is fine, but latitude and longitude lines are only labelled in Fig 1, and then very coarsely. It would be nice to label them in subsequent plots and to perhaps include a few more lines on one of them.

Perhaps more substantively, I do not fully appreciate the points the authors were trying to achieve. I see the thermodynamics behind their approach etc, but I do not really understand where they were going with this. Energy is not a conservative tracer, so I am uncertain how to think about detailed comparisons of this quantity along stream-

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lines. For example, it appears to me that the convection occurring at mid-basin in the Barents Sea might well have the effect of 'erasing' the energy signature of the inflowing North Atlantic water. There are also external energy sources that may have a role to play in the overall structure of the flow. It is pointed out that along the model computed trajectories, there are potential temperature and salinity changes of the water masses. Of course, associated with this are modifications to the numbers that will be computed as AE. But, the source for this diabatic mixing is likely to the an external one, like the winds or the tides. Nowhere is there a mention of this external influence in the present paper.

There is a statement that the flow through the Fram Strait is 'smaller' than it might be, given the AE of the Atlantic relative to the Bear Island Trough. But the Atlantic may export significant amounts of its energy advantage in the form of baroclinic instabilities and waves. Again, there is not discussion of how the energetics considerations of this paper are affected by these other dynamics, and to me this tends to confuse the picture the authors are constructing.

At the close of the paper, a brief discussion ensues of the importance of 'turbulence' in permitting flow to cross geostrophic contours. What exactly is meant here by turbulence is not really stated, although the discussion would suggest the authors have in mind bottom boundary layer frictional effects. I was not sure what point the authors were trying to make in this discussion, but it is possible also that eddies can allow flow to cross geostophic contours. This is turbulence, of course, but of the geostrophic type. Do the authors mean to include this in their use of the term turbulence? It is mentioned that without turbulence, there would be no flow via the Barents into the Arctic. I am not sure I would agree with this statement if the authors did not mean to include geostrophic turbulence into their definition of turbulence. It is my understanding eddies are quite active in the Arctic Ocean, especially in moving shelf waters into the open Arctic Ocean.

Finally, the authors suggest a signature of AE can be used as a tracer of a water mass. I was not certain I believed this, given that external energy sources and sinks

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to exist. Also, I wonder if as a tracer, AE has anything to offer aside from potential temperature and salinity. If the AE tongue were not characterized by following tongues of potential temperature and salinity, I would doubt that it was following a water mass. But to compute the AE, it is necessary to first measure the potential temperature and salinity, thus seeming to remove the utility of AE as a tracer. In fact in this paper, much of the use of AE has come from following trajectories suggested by a numerical model. Without this additional information, how would one make use of maps of AE?

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