Ocean Sci. Discuss., 5, S219–S221, 2008
www.ocean-sci-discuss.net/5/S219/2008/

Ocean Science Discussions

Interactive comment on "Turbulence closure: turbulence, waves and the wave-turbulence transition – Part 1: vanishing mean shear" *by* H. Z. Baumert and H. Peters

Anonymous Referee #1

Received and published: 22 December 2008

This paper is an attempt to extend second order turbulent closure out of its traditional area of application in turbulent flows into the wider parameter space of the mixed internal wave and turbulence regime, i.e. very high Reynolds numbers, but low Richardson numbers, found in the ocean and atmosphere. This regime presents particular difficulties because the usual tools of turbulent closure, algebraic closures of the moment equations, have yielded few results. One should remember that these traditional approaches, e.g. diffusive closure of moments, are not strictly justified; they cannot be derived from the fundamental equations, but are ad-hoc. However, they have been fairly successful for turbulent flows because they embody sensible physics. They are now decades old and are thus enshined in lore.

In the wave dominated regime, much of the dynamics is different. Waves interact in very different ways from tubulence, think about critical layer dynamics. Fluxes are not always down-gradient, in fact they are not even necessarily local. Some new approach is therefore called for. The authors approach this boldly by adding new terms to simplified versions of traditional second order closures, again attempting to embody sensible physics, and explore the consequences for the structure of the model predictions. Their first attempt (Baumert and Peters 2004) showed that adding a loss term for waves to second order closure yielded reasonable, if not perfect, agreement with data. Here, they attempt to extend these results far into the wave dominated regime by incorporating the well-known and very successful closure of Gregg (1989) for dissipation rates in this regime. The goal is to reproduce the wave-turbulence closure suggested empirically by D'Asaro and Lien (2000). The paper succeeds in doing this, unlike any previous second order closure.

It is certainly possible to criticize details of this model. For example, the flux of TKE is probably not represented properly in second order closure by a downgradient approximation as done in equation (2). This is, of course, true in almost all second order closure schemes and does not condemn this one in particular. The choice of length scale, here by the Omega equation, is always difficult, with different practitioners making different choices. Furthermore, the addition of a wave loss term, W, into the TKE equation is bold and ad-hoc and will offend those who want a more obtuse way of introducing their arbitrary assumptions. Finally the model is almost certainly WRONG in some of its predictions. This is true of all models, particularly in turbulence. The purpose of the exercise is to figure out how to improve the models. This paper is a good step in this direction because it moves into unexplored territory and shows some success.

The presentation of the results is confusing at time. In particular, the statement (Page

OSD

5, S219–S221, 2008

557 - Line 11) that there is no turbulence for Ri $>$ 1/4 is confusing and, as stated, wrong. Clearly in stratified flows with waves there is turbulence and mixing at very high Richardson numbers. That is the problem that this paper is trying to solve. In the model formulation, however, the dissipation and mixing is done both by the turbulence (i.e. the last term in the K equation) and by the waves (the last 2 terms in equation (24). Thus there are two types of turbulence in this model - the resolved "K-Omega" turbulence and the unresolved, intermittent "wave breaking turbulence". At low Ri, K-Omega does all the work in the traditional way. At high Ri, wave breaking does all the work. The total turbulent fluxes, energies and dissipations are the sum of these as discussed after equation (45). However, this ideas should be introduced as the very start, perhaps even in the abstract, since it is the really new assumptions and contributions of this approach.

Detailed comments:

Page 548 - line 20 paragraph- The argument for using omega as the second state variable is not compelling.

Page 549 - top- Baumert's vortex model is not universally accepted and therefore is a distraction here.

Page 549 second Paragraph- This starts out very abruptly and its hard to figure out how it relates to the previous text.

Page 555 - A reference to the Wilcox models would be helpful.

Interactive comment on Ocean Sci. Discuss., 5, 545, 2008.

OSD

5, S219–S221, 2008

Interactive Comment