Ocean Science Discussions

Interactive comment on "Internal tides and energy fluxes over Great Meteor Seamount" *by* T. Gerkema and H. van Haren

Anonymous Referee #3

Received and published: 3 September 2007

This paper discusses methods for determining the vertical profile of internal tide energy fluxes from observations of the vertical profiles of velocity (using an ADCP) and temperature and salinity (using a yo-yoed CTD) converted to density. Both datasets were tidally analysed and density was converted to pressure before determining the vertical profile of the horizontal energy flux using $<p'u'>$.

The authors find that the precise energy flux profile thus derived depends crucially on the (unknown) constant that is applied to the vertical integration of p' in the presence of a bottom slope, although the total integrated flux is insensitive to this constant.

Theirs is an interesting idea and one that deserves serious consideration, although one of the reason that it has taken me so long to complete this review (for which I apologise)

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is that I found the presentation somewhat incomplete and impenetrable.

Some specific points are:

1. I think that the paper could have been better structured if it had started with the theory and then lead on to a couple of practical examples (say one from the numerical model and one from observations). I.e. in my view they should restructure the whole paper and turn it inside out.

2. To this end I think that the title is misleading since the paper discusses a far more general topic than that of the energy fluxes across the Meteor Seamount. I strongly recommend a change of title.

3. Furthermore I would recommend that references to the diurnal signal and its proximity to the inertial frequency in the abstract and in the discussion should be removed to bring a stronger focus to the main subject of the paper.

4. I am dubious about the suitability of citing momentum in its freely propagating form in equations 4 and 5. With $dh/dz \neq 0$ and $Q \neq 0$ there should be an additional forcing term in (4) at least. The inclusion of this term could have a significant impact on their argument which needs to be discussed.

5. The authors focus their discussion on the cross slope flux and effectively ignore the along slope flux. If the vertical profile of energy flux changes sign at a depth z then it must be because the phase of pressure has changed. Such a change must also affect the vertical distribution of the along slope flux, even though the basic premise (that the vertical profile of energy flux is unchanged if $dh/dy = 0$) is not invoked in that direction. Can the authors explain what is happening in this situation?

To conclude I remain a little sceptical about the validity of the main result, which I don't think has been discussed sufficiently rigorously.

Should this paper be published in Ocean Science? The manuscript raises an interesting point that merits investigation, and it will inspire me to think hard next time I am considering an internal tide energy flux.

However, the result seems at some level to be counter-intuitive, and I believe that a more thorough and rigorous discussion and presentation of the argument is needed before it could be generally accepted by the scientific community.

Interactive comment on Ocean Sci. Discuss., 4, 371, 2007.

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