Ocean Sci. Discuss., 3, S487–S493, 2006
www.ocean-sci-discuss.net/3/S487/2006/

Ocean Science Discussions

Interactive comment on “Energetics of the layer-thickness form drag based on an integral identity” *by* H. Aiki and T. Yamagata

H. Aiki and T. Yamagata

Received and published: 22 September 2006

Final response to the two referees concerning manuscript no. OSD-2006-0027

My coauthor and I would like to thank Drs. Richard Greatbatch and Trevor McDougall for their constructive comments on the manuscript. Below we address each of the issues raised by the two referees, where the sentences in boldface are quoted directly from the revised manuscript.

Reply to R. Greatbatch (Referee)

`>Specific Comments:`

>1. Introduction, line 6: The authors state that the layer-thickness
>form drag has been unpopular in modern numerical applications of the
>ocean. I assume this is a reference to the common practise of putting
>the eddy parameterisation into the tracer equation? Nevertheless,
>the layer-thickness form drag plays a central role in theories of
>the Southern Ocean where it has received considerable attention
>(e.g. Rintoul, Hughes and Olbers, 2001).

We have fully revised the leading paragraph of the Introduction as follows: **“The vertical mixing of momentum in a stratified fluid can be induced by the residual effects of pressure perturbations (called the layer-thickness form drag in this paper, as detailed in Sect. 2.2), which has received considerable attentions in various research areas of atmosphere and ocean dynamics (cf. Andrews, 1984; Johnson and Bryden, 1989; Cushman-Roisin *et al.*, 1990; Lee and Leach, 1996). In contrast to the momentum transfer, the energetics of the layer-thickness form drag have received little attention in previous oceanic studies. The present study shows that an adiabatic formulation of an inviscid hydrostatic fluid yields a four-box energy diagram that elucidates the role of layer-thickness form drag in the connection between the mean and perturbation fields. This result can be regarded as fundament to introducing the parameterization of layer-thickness form drag in numerical ocean circulation models.”**

The papers by Johnson and Bryden (1989), Killworth and Nann (1994), and Rintoul *et al.* (2001) concerning the layer-thickness form drag (or the interfacial form stress) in the Antarctic Circumpolar Current are cited in the summary section of the revised manuscript.

>2. Top of page 545: The difference between the mean height density
>and the Eulerian mean density has been discussed in detail by
>Killworth (2001) "Boundary conditions on quasi-Stokes velocities in
>parameterizations", J. Phys. Oceanogr., 31, 1132-1155, where also
>the difficulty of using the mean height density near boundaries is

OSD

3, S487–S493, 2006

OSD

3, S487–S493, 2006

>discussed.

The corresponding sentence has been rewritten as **“Stratification of the MH density $\widetilde{\rho}$ is inherently sharper than that of the Eulerian mean density $\overline{\rho}$ (see Fig. 1; a related discussion appears in Killworth, 2001).”**

We generally agree with the motivation of Killworth (2001) to compare the distributions of the MH density and the Eulerian mean density: a finer numerical mesh might be needed to resolve the MH density near the sea surface. However, we have identified the contradiction that the numerical diagnostics in Killworth (2001) uses Equations (4b) and (11) of McDougall and McIntosh (2001), even though these equations violate the boundary condition at the top and bottom because of ρ' being nonzero at the boundaries (which is remarked on in McDougall and McIntosh, 2001). In order to obtain correct results for the properties near the boundaries, it is more natural to use either the backmapping method of de Szoeke and Bennett (1993) or Equations (4a) and (9) in McDougall and McIntosh (2001).

>3. Top of page 547: Strictly speaking, the definition for the
>quasi-Stokes velocity given here and attributed to McDougall and
>McIntosh (1996, 2001) is the definition found in the 2001 paper. The
>1996 paper is rather different since the total transport velocity is
>not the same as the thickness-weighted isopycnal averaged velocity
>in that paper.

We have corrected the reference, thank you for pointing out the error.

>4. Top of page 547: When talking about boundary conditions for the
>total transport velocity in the TEM theory one has to distinguish
>between the different versions of the TEM theory, e.g. Andrews and
>McIntyre (1976), Andrews and McIntyre (1978), Held and Schneider (1999)

>and more recently Eden, Greatbatch and Olbers (2006, in press in JPO).
>The version of Andrews and McIntyre (1976) clearly does not perform
>well as one approaches the (unstratified) surface mixed layer, but the
>version of Held and Schneider (1999) is designed specifically to deal
>with that situation. The version of Andrews and McIntyre (1978)
>essentially combines these two approaches into one, and the version of
>Eden, Greatbatch and Olbers (2006) takes account of rotational fluxes.
>Often (but not always guaranteed) the total transport velocity will
>satisfy the expected boundary conditions in a generalised TEM theory.

We understand the referee's comment. The TEM energy diagram referred to in the present paper is based on Andrews and McIntyre (1976). The revised manuscript simply states **"in sharp contrast to the total transport velocity used in Plumb (1983) and Kanzawa (1984)".**

>5. Section 3.6: There is mention in the text that use of equation (1)
>and (2) in an OGCM might result in barotropic currents and
>interactions with the bottom topography. Such effects are already
>anticipated in Holloway (1992), JPO, and Greatbatch and Li (2000),
>Deep Sea Research. It could be that the formulation of the energetics
>presented here could be used to put the ideas in both the above papers
>on a firmer theoretical basis.

Thank you for the information. The revised manuscript states that **"Greatbatch and Li (2000) have reported that a three-dimensional simulation adopting the momentum approach is successful in showing anticyclonic mean flow around a seamount."**

OSD

3, S487–S493, 2006

Interactive Comment

Reply to T. McDougall (Referee)

```
>The fifth line of section 2 should be "expressions in rho-coordinates"
>not "expressions in z-coordinates"
```

We have rewritten the sentence as follows: **“Readers not familiar with expressions in Sect. 2.1 are first referred to Bleck (1985) for the primitive equations (and energy equations) in isentropic and density coordinates.”**

```
>Just before equation (1), replace "are described by (1), (2)" with
>"can be derived from (1), (2)".
```

We have corrected this error, thank you for pointing it out.

```
>Five lines before equation (21) you refer to both the bolus velocity
>and the isopycnal mean velocity being three-dimensionally divergent.
>When Peter McIntosh and I pointed this out in the TRM manuscript, we
>had a huge fight with a referee and with the editor of JPO.  In fact
>that second TRM paper was rejected by JPO and I had to appeal to the
>chief editor to get it published; more than 3 years after it should
>have been published!  I insisted that this aspect of the paper remain,
>and so it did.  Since it was very controversial, perhaps a reference
>here to that prior publication of the divergent nature of the
>velocities may be warranted.
```

The revised manuscript cites McDougall (1998). Section 10 of McDougall (1998) (also Section 10b of McDougall and McIntosh, 2001) provides a detailed explanation for the difference between the bolus velocity and the quasi-Stokes velocity. We understand that the quasi-Stokes velocity has a series of mathematically convenient properties (i.e., three-dimensionally nondivergent and purely baroclinic). Our short comment

OSD

3, S487–S493, 2006

Interactive Comment

about the three-dimensional divergence of the bolus velocity is intended to avoid confusion in Sect. 4 (of the present paper), in which the eddy-induced velocity is described by using the bolus velocity rather than the quasi-Stokes velocity (it simply follows from Sect. 3.6). If we did not comment about the bolus velocity being three-dimensionally divergent, the overturning stream function shown in Fig. 3 (of the present paper) may result in some readers incorrectly assuming that the bolus velocity is three-dimensionally nondivergent and purely baroclinic.

```
>
>I really liked your section 3.5.
>
```

Thank you.

```
>Your equation (24) seems to be like two orders of vertical integration
>different to the usual down-gradient thickness idea for the bolus
>velocity.  That is, your baroclinic velocity scales as the vertical
>integral of the slope of isopycnals (ie the vertical integral of
>thermal wind), whereas the usual down-thickness-gradient assumption
>has the bolus velocity proportional to the vertical derivative of the
>isopycnal slope.  This difference, by two orders of vertical
>differentiation, might be worth pointing out.
```

We agree with the referee's comment. In fact, the difference of two orders in the vertical integration (and differentiation) has already been addressed by Aiki *et al.* (2004) with respect to the quasi-Stokes stream function. The difference of two orders appears also in the parameterized form drag in the present paper, and is emphasized in the revised manuscript as follows: **"Greatbatch (1998) suggested that the layer-thickness form drag can be parameterized by Fickian diffusion that transfers the geostrophic momentum in the vertical direction (cf. Ferreira *et al.*, 2005). In considering a**

OSD

3, S487–S493, 2006

Interactive Comment

similar form drag, Aiki *et al.* (2004) incorporated Rayleigh damping in the baroclinic component of the isopycnal mean velocity." In order to highlight the result of our scaling in Sect. 4.1, we have not changed the description of the eddy-induced additional velocity.

Interactive comment on Ocean Sci. Discuss., 3, 541, 2006.

OSD

3, S487–S493, 2006

Interactive Comment