

***Interactive comment on “Southern Ocean
overturning across streamlines in an eddy
simulation of the Antarctic Circumpolar Current”
by A. M. Treguier et al.***

J. Marshall (Referee)

marshall@gulf.mit.edu

Received and published: 10 October 2007

I have read the paper ‘Southern Ocean overturning...’ by Treguier et al and would like to comment on it as follows.

The paper describes attempts to diagnose the overturning circulation in the southern ocean in an eddy resolving model. It uses a streamwise-average/density-coordinate approach chosen to clearly identify the relative importance of eddy and mean processes in setting the overturning circulation. The paper is well written and insightful — I recommend publication.

I have the following major comments which should be addressed in revision.

1. The central element of the discussion, and the equation to which the authors return again and again, is Eq.(3). They describe it at one point as 'surprising'. The authors find that (3) has little diagnostic value — e.g it suggests that Ψ_{res} changes sign over the ACC whereas direct computation shows otherwise. Given the importance of this relation — it is the building block of theoretical models of the overturning cell — the paper could benefit from a critical review of where it comes from and what assumptions are inherent in it.

I agree with David Marshall's comment, that something like Eq.(3) has to hold in a time-mean sense. Here's why.

The most general approach to deriving Eq.(3) is to adopt a Walin (1982) perspective, define a control volume bounded by σ surfaces which outcrop at the sea-surface and write (notation approximately as in Marshall et al, 1999 — see also Tandon and Garrett, 1995):

$$a = \frac{\partial \hat{B}}{\partial \sigma} - \frac{\partial D}{\partial \sigma} walin \quad (1)$$

where a is the diapycnal volume flux through the 'sides' of the control volume, $\hat{B} = \int B_s dA$ with B_s the air-sea buoyancy flux and D is the non-advective (diffusive) supply of buoyancy to the control volume (sorry about the conflict of notation — the present paper uses D for density, whereas in my comment here it is being used for diffusion).¹ Now if there is no mixing, $D = 0$, then we get what Garrett calls the ideal formula which can be written (noting that $dA = Ldy$ where

¹Perhaps the authors might consider changing notation so that connection to previous work be clear: the formula $A = F - \frac{\partial D}{\partial \sigma}$ seems pretty common now, where $F = \frac{\partial \hat{B}}{\partial \sigma}$ and D means diffusion (mixing).

L is the length of the outcrop threading around Antarctica and y is a meridional coordinate):

$$a = \frac{\partial \hat{B}}{\partial \sigma} = \frac{\partial \hat{B}}{\partial y} \frac{\partial y}{\partial \sigma} = LB_s \frac{\partial y}{\partial \sigma}$$

Hence, rearranging, we get:

$$\frac{a}{L} \frac{\partial \sigma}{\partial y} = B_{s\text{walin_ideal}} \quad (2)$$

Now, expressing the diapycnal volume flux per unit length of the outcrop between some depth on the σ surface and the sea surface, in terms of a streamfunction, one arrives at Eq.(3)².

- Given that Eq.(3) is then really just Eq.(1) with interior mixing neglected, what are the nature of the mixing processes that are important in supplying buoyancy to the surface layers of the gcm? Do they make sense? If they are included, then presumably Eq.(2) could be modified to include them. One potentially very important mixing term is the diabatic eddy flux directed laterally through the mixed layer. This cannot be represented as a skew flux and so ends up bundled in to D . This is not mentioned, but is discussed in some length in Marshall and Radko, 2003.
- I found the conclusions to be generally pessimistic, whereas, on reading the paper, I was generally encouraged by how well the neat diagnostics presented supported the growing consensus about the upper branch of the overturning circulation. I like Fig.12 very much — the details of the vertical variations of Ψ^* and how

²Note that to really get (3) one has first to go in to a residual-mean formulation thus:

$$\frac{\partial b}{\partial t} + \mathbf{v}_{res} \cdot \nabla b = -\nabla \cdot \mathbf{N}_b + \frac{\partial}{\partial z} B_s$$

where B_s is the air-sea buoyancy flux and \mathbf{N}_b is the diffusive supply of buoyancy to the control volume which will include residual (non-skew) eddy fluxes (see, e.g., Marshall and Radko, 2003). These may be very important in the near-surface layers of the ACC.

much they cancel $\bar{\Psi}$ will surely be dependent on the manner in which the eddy fluxes vary in the vertical and hence how eddy cascades are halted by numerics in the top few hundred meters of the ocean. Not an easy thing to deal with. Could a more upbeat conclusion be written?

Minor points

1. Abstract: much too pessimistic.
2. Eq.(2) — this is not really a q.g. result is it? The stratification σ_z can be allowed to vary in space which would be disallowed in qg.
3. page 656, top
are the eddying models giving different results because of formulation issues/resolution etc, or is it mainly the differing diagnostic approaches adopted by various investigators?
4. Eq.(3) — discuss where it comes from
5. page 659 – puzzled comment suggests the authors are skeptical about Eq.(3). Either explain or delete. Better to explain.
6. Fig.1 – legend. Clarify what a +ve flux signifies - in/out of the ocean?
7. Page 667 — there are other ways of defining Ψ_{res} — see, e.g. Held and Schneider, 1999: The surface branch of the zonally averaged mass transport circulation of the troposphere. J. Atmos. Sci., 56, 1688–1697 — that are very useful in vertically mixed layers. The Held Schneider definition was used by Marshall and Radko, 2003.

8. Figs 8 → 11 — suggest put arrows on to indicate the sense of circulation.
9. Page 670 — qualitative agreement — only qualitative because of the neglect of mixing processes in Eq.(3). But Eq.(3) will still provide a useful framework, suitably modified. Might also mention that the model almost certainly mixed more than the ocean.
10. Page 674 — what about the role of the residual eddy flux directed across mean streamlines. This is discussed in Marshall and Radko (2003). I would expect it to be important here.

Interactive comment on Ocean Sci. Discuss., 4, 653, 2007.

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