

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

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Your main concern, which also comes up in the other referees' comments, is the way we present our opinion about relation (3). Indeed this was a source of discussions among us (the co-authors) and we thought we had agreed on a fairly balanced view. Taking into account the reviewers' remarks we propose some modifications of the text (the abstract and the final paragraph, for example) that hopefully improve the presentation.

Major comments The paper could benefit from a critical review of the assumptions inherent in (3), however this would make it lengthy. It would be worthwhile if we could analyze in detail which extra terms play a part and quantify them, but at the present

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time we do not have models diagnostics detailed enough to go further in this direction. This is a matter for forthcoming studies. We have expanded the paragraph after eq 3, by adding references to Marshall and Radko (2003) and Karsten and Marshall (2002). We have underlined that Radko and Marshall (2003) estimate that diabatic eddy fluxes in the mixed layer could be significant.

We agree that there is a link between our calculations (across time-mean streamlines) and Walin's formalism, but this link is not straightforward. Balances "à la Walin" and water mass transformation rates are calculated following instantaneous isopycnals and outcrops as they move. D. Marshall (1997) defines eddy subduction in the case when eddy effects can be parameterized as an advection, but it is unclear that eddy fluxes are purely advective in the surface mixed layer. Moreover, the time variability of isopycnal outcrops is mainly due to the seasonal cycle, and the interaction of the seasonal cycle with the eddies is difficult to unravel because of their similar time scales.

Minor points

1 - The abstract has been modified.

2 - Eq (1): agreed. The statement about the QG approximation has been removed.

3 - page 656, top: we emphasize the different diagnostic approaches in the text ("different methods") because differences in methods are an obvious candidate to explain discrepancies in previously published studies. The point is precisely that the use of different diagnostic methods in those studies does not allow the reader to conclude as to the importance of other model differences.

4 - Eq. 3: we have chosen to use (3) directly rather than re-derive it, because the derivation appears in many published papers. We have added a sentence to explain that it results from the potential density balance and referring the reader to Karsten and Marshall (2002). We also point out Marshall and Radko (2003) suggestion that the rhs of (3) should include lateral diffusive fluxes in the mixed layer.

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5 - page 659 : we agree to remove the sentence.

6 - legend of figure 1: a sentence has been added to explain the sign convention.

Discussion of Fig 5: After giving the results of the meridional residual circulation estimated from the surface buoyancy forcing (shown in fig 5) we have added the following sentence: Note that we are not able to calculate accurately the contribution of lateral diabatic eddy fluxes in the mixed layer from our model output, and thus consider only the contribution from surface buoyancy forcing in (3).

7 - page 667: the different ways of defining the residual circulation are now mentioned at the beginning of section 4. In fact, we had tried to use the Held and Schneider definition at first, but results were very noisy and difficult to interpret. Held and Schneider (1999) has been added to the list of references.

8 - Arrows have been added to figures 9, 10 and 11. Arrows made Fig 8 too cluttered.

9 - page 670: we have added a reference to Olbers and Visbeck to acknowledge that more complex models than Eg 3 can be derived, and changed the wording of the sentence “The main problem with (3)...”

10 - page 674: On that page we discuss the heat balance. The total eddy heat flux across the mean streamlines is large (0.73 PW at the ACC northern boundary); this is mentioned on page 673. It is likely that a large part of it is due to the advection by the eddy component of the residual velocity, but we do not attempt to reproduce the calculation of Lee et al (2007) who decomposed the eddy heat flux into its advective and diffusive components. Considering the similarity between other aspects of their model and ours, it is quite likely that we would find a similar result (i.e., likely that about half the total eddy heat transport would be advective?).

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