

Interactive comment on “The subtropical Deacon cells” by J. A. Polton and D. P. Marshall

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General comments

I have found this paper to be an interesting and at times enjoyable read. Its goal is to make clear a somewhat obvious but rarely recognized point: that the streamwise-averaged dynamical framework used so successfully in the Antarctic Circumpolar Current (ACC) can also be enlightening when applied to other ocean circulation features characterized by closed streamlines. Accordingly, the paper’s most valuable achievement is that it shows that the cross-stream overturning circulations in the ACC and the mid-ocean gyres can to a large degree be understood as two distinct end members of a common set of ocean dynamics. At any rate, the paper would benefit from a considerable revision, as it contains several inaccuracies in its terminology, equations, analysis and figures that may confuse the reader.

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Specific comments:

The first aspect of the paper that I would like to question is its use of the term 'Deacon cell'. In the Southern Ocean literature, this term has been widely used to refer to the intrinsically adiabatic overturning circulation associated with the wind-driven Ekman flow in the upper ocean and a poleward return flow at great depth. Subsequently, Speer et al. (2000) coined the term 'diabatic Deacon cell' to describe an overturning circulation flowing broadly in the same sense as the Deacon cell but with the ability to cross isopycnals. This 'diabatic Deacon cell' is, in essence, the residual circulation arising from the interplay of wind-, eddy- and buoyancy-driven contributions to the overturning across the ACC. Thus, the authors may wish to refer to the 'subtropical diabatic Deacon cells' throughout the paper or, at a minimum, acknowledge the issues with their terminology in the Introduction. Also in this section, the authors may wish to note that the deep (NADW-AABW) cell of the Southern Ocean overturning cannot be adequately described by the classical ACC residual mean paradigm, as the conversion of NADW to AABW occurs mostly in subpolar gyres that are more akin to a subtropical gyre than to a quasi-zonal current.

I found the derivation of the equations in section 2 (in particular that of equation 4) to be a little jumbled and amenable to improvement in clarity. In particular, it is far from obvious in the authors' derivation why the expressions in equation 4 can be equated to the potential vorticity (PV) flux vector, which should perhaps have been defined in the context of the principle of PV conservation and decomposed into its components with an additional equation. This would enable the reader to better understand what advantages the switch to a PV-based formulation has for describing the dynamics of the model ocean. Note also that in line 20 of page 874, the statement applies to an eddy-induced PV *flux*. Further on, in section 2.4, I suggest that an explicit distinction be made between the role of eddy fluxes in 2.4.1 (adiabatic) and in 2.4.2 (diabatic), in order to facilitate comprehension of the important physical distinctions between the two limits.

Section 4 is generally quite clear but in subsection 4.1 the directions of PV fluxes appear to be inconsistent at times with the PV flux directions discussed in section 2.4. I suggest that the authors check this subsection carefully.

In section 5, I would argue that it is unlikely that the different geometry of the streamlines in the ACC and the subtropical gyres (circumpolarly connected in the former and not in the latter) underlies the apparently different importance of diapycnal mixing in each region, as is hinted at by the authors. I think this point requires a brief discussion: the bowled geometry of the gyre isopycnals is symptomatic of the presence of some amount of diapycnal mixing (whatever its nature) in the ocean interior, both in the subtropical gyres and in the ACC. In the latter, diapycnal mixing is not indispensable to close the local PV budget, but if it is happening in the subtropics it is likely to be happening in the ACC too. In relation to this point, the authors note that spurious diapycnal mixing is important in the deep subtropical PV budget of the model. But how intense is this required spurious mixing? Given the stratification in the model, is it broadly consistent with observed background levels of diapycnal mixing in the open ocean or does it suggest a need for regions of enhanced diapycnal mixing (e.g. in the western boundary current)?

In figures 7-8 and 11-13, it is unclear how the Bernoulli contours and their (rather variable) number were chosen. This probably bears some relation to the authors' choice of 20 closed contours evenly spaced in mean Bernoulli potential in section 3.3, but it is not obvious that this is the case. Perhaps the authors would like to clarify this point.

My final comment is a suggested addition to the final section of the paper. Having successfully demonstrated that the cross-stream overturning circulations in the subtropical gyres and the ACC can be understood with the same dynamical framework, the authors may wish to point out that there are still dynamical differences between the two circulation systems that elude the framework used in the paper - but these concern the along-stream rather than the cross-stream flow and boil down to the distinction between the predominantly Sverdrupian dynamics of the subtropical gyres and the more

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complex dynamics of the ACC. The fact that authors' formulation is valid and useful regardless of the dynamics of the along-stream flow is a point worth emphasizing.

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