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Ocean Science Discussions

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

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1 General comments

This is an interesting paper, applying a new form of diagnostic to an ocean model with physically interesting results. It makes clear the fact that certain aspects of ocean dynamics which have perviously been thought to be of interest only in the Antarctic Circumpolar Current (ACC) can equally well be considered profitably in other recirculating flows, such as subtropical gyres. That is not to say there is nothing different about the ACC compared to gyres, but there are no fundamental reasons to consider it in a different dynamical framework. I believe this paper should be published, although there are a number of subtleties to the theoretical framework which should be clearly aired in order to avoid the potential for confusion, and one presentational detail which should also be addressed.

2 Specific comments

The development of the equations is rather haphazard and confusing, given the variety of approximations made and the subsequent comparison to an ocean model which uses the equations in a different approximate form. For example, the last term on the left hand side of (1) is appropriate for the hydrostatic Boussinesq equations, but not for the non-hydrostatic case which is assumed by the form of the other terms in (1). Similarly, when it comes to the model diagnostics (shortly before Eq. 14), the equations derived in the non-hydrostatic, fully three-dimensional case are applied to the approximated equations used in the model, but then approximated based on scale analysis (dropping the w term in the Bernoulli potential, and considering only the vertical component of the vorticity). I appreciate that the derivations are simplest for the full 3D equations in vector form, but in this case there is a relatively simple derivation possible, starting with the form of equations actually used in the model.

The model uses the hydrostatic primitive equations as discussed in White and Bromley 1995 (Q.J.R. Met. Soc., 121, 399–418). If we also introduce the Boussinesq approximation by setting $\rho = \rho_0$ in the horizontal components but not in the vertical, then the HPEs may be written in vector form as

$$\underline{v}_t + \underline{q} \times \underline{u} + \nabla\Pi + \underline{\hat{k}}\frac{g\delta\rho}{\rho_0} = \underline{F}_h,$$

where $\underline{u}$ is 3D velocity, $\underline{v}$ is the horizontal part of $\underline{u}$, $\underline{q} = 2\Omega\underline{\hat{k}}\sin\phi + \nabla \times \underline{v}$ with the curl operator defined as in White and Bromley, $\Pi = gz + p/\rho_0 + \underline{v}\cdot\underline{v}/2$, and $\underline{F}_h$ is the viscous body force. Following the derivation through then leads to the same equation (4) but with the new definitions of Π and $\underline{q}$, and with a $\partial\underline{v}/\partial t$ in place of $\partial\underline{u}/\partial t$ in the final term.

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It is then clear how to diagnose these equations from the model, and also clear that using only the vertical component of $\underline{q}$ is an additional approximation, but ignoring the w term in the Bernoulli potential is not.

When it comes to equation (4), some more discussion is needed. The demonstration of an equality has been made, but then it appears that an arbitrary decision has been made to put certain terms on one side of the equation, and others on the other side, and equate each side with a vector $\overline{\underline{J}}$ which is then called the potential vorticity flux. The usual significance of a PV flux $\overline{\underline{J}}$ is that it satisfies a PV conservation equation of the form $(\overline{\rho}\tilde{Q})_t + \nabla \cdot \overline{\underline{J}} = 0$, and reduces to a simple advective flux (by the mean flow) of $\tilde{Q}$ in the absence of eddies, friction and diffusion. In fact, to satisfy the first of these points, the Boussinesq term proportional to g should be moved into the first group of terms, but should remain where it is for the second point. This peculiarity of the Boussinesq approximation also appears in a slightly different form in a 2-component fluid such as the ocean with salinity, and it is unclear whether it is better to let it appear as a source term in the cosnervation equation, or as a nonadvective PV flux in the definition of $\overline{\underline{J}}$, which remains in the absence of friction and buoyancy forcing. In any case, the reason for choosing a particular split of the equation, and hence a particular definition of $\overline{\underline{J}}$, should be made clearly to enable the reader to follow the development and understand the reason for the introduction of $\overline{\underline{J}}$.

Importantly, it should also be made clear that $\overline{\underline{J}}$ is in fact not the same as an average of the PV flux $\underline{J}_{HM}$ defined by Haynes and McIntyre etc. As an illustration of this, consider the statistically steady state. Ignoring the Boussinesq correction term (or moving it to the other side of the equation) gives $\overline{\underline{J}} = \nabla\tilde{\Pi} \times \nabla\overline{\sigma}$, whereas the Haynes and McIntyre form would give $\overline{\underline{J}}_{HM} = \overline{\nabla\Pi \times \nabla\sigma}$. Setting these to be equal would imply that $\overline{\nabla\Pi' \times \nabla\sigma'} = -\nabla\left(\frac{\overline{\underline{v}' \cdot \underline{v}'}}{2}\right) \times \overline{\nabla\sigma}$. If we imagine imposing buoyancy forcing such that $\sigma' = 0$, then this produces the unreasonable prediction that the eddy kinetic energy is a function of $\overline{\sigma}$. Not a proof, but a strong suggestion that the two fluxes are different.

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The reason for the difference is that (4) was derived with a different aim in mind. Although Bretherton and Schär (JAS 1993, 1834–1836) showed that the Haynes and McIntyre PV is the unique choice which reduces to advection in the absence of friction and diffusion, and which has a linear dependence on local values of the friction and diffusion terms, those conditions are not met here with respect to the instantaneous flow. Instead, $\underline{\overline{J}}$ meets similar conditions for the averaged flow. In particular, the friction and buoyancy forcing terms involve products of averages rather than averages of products, making them easier to calculate, and causing them to relate to a different definition of the PV flux. Similarly, the advective term represents an advection of the mean of ρQ by the mean flow, rather than the mean of the advection of ρQ by the instantaneous flow. The eddy advection terms may make up some of the difference between these two forms, but they do not appear to be of the correct form to produce an equality.

From the point of view of diagnosing these quantities from an ocean model, this seems to be a positive advantage. But it is important to clarify that the PV flux discussed here is not the average of the Haynes and McIntyre flux, and certainly not a generalization of it to include the effect of transient eddies, which are included in the original formulation (although the thinking behind the derivation could be considered a generalization of the concept of a PV flux), but rather is an alternative choice with useful properties from the point of view of model diagnostics.

Of course, for the main thrust of the paper this is irrelevant, since the concept that $\underline{\overline{J}}$ represents a PV flux is not important to the mathematical development (indeed (6) abandons the separation of the equation into two parts each called the flux, and just uses the equality irrespective of that division). However, it seems important to clarify what is being calculated here so that confusion with the Haynes McIntyre flux does not arise.

The significant presentational issue I mentioned concerns the contours chosen in figures 8,9,11,12 and 13. In the description it says that the flux was integrated over the areas enclosed by 20 contours, equally spaced in Π, then normalised by the area (inci-

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dentally, why not simply say that the pictures are of area-averaged fluxes)? The figures, however, each appear to show integrals over a different selection of areas between different pairs of contours, some of which are represented as solid lines and some as dotted. What's going on here? At least within each figure, the areas over which the integrals are performed should be the same for each term.

Finally, I was confused by the discussion at the bottom of p881 and top of p882 comparing the results with those of Drijfhout. These are clearly different kinds of analyses, but it is far from clear to me how they should be compared. Might not the bolus transport appear quite differently in (4) as compared to an average of the Haynes and McIntyre flux?

3 Technical corrections

Somewhere it should be mentioned that Boussinesq implies $\nabla \cdot \underline{u} = 0$, needed for (3).

Last line of p. 871: should be grad, not div.

p873, line 13 "either a buoyancy-forced *or a* frictional ..."

p873, section 2.3: I think the title should be "Approximate interpretation ..."

p881, line 10: AABW.

p881, line 10: Drijfout should be Drijfhout.

p884, line 15: unversally

p885, lines 9–11. This sentence seems confused, is it trying to say that it is probably not representative, or that it is unclear whether it is representative?

p885, line 15. "exact" should be qualified, since the precise form of this constraint relies on the Boussinesq approximation. An analogous exact constraint can be expected,

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but would no longer be a constraint on the vertical flux in the case of a 2-component fluid (for a 1-component fluid it could be applied to any closed Π contour, given the appropriate generalised definition of Π).

References: Rintoul et al, “Acedemic”.

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