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Ocean Science Discussions

Interactive comment on "Inferring the zonal distribution of measured changes in the meridional overturning circulation" *by* A. M. de Boer and H. L. Johnson

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This mansucript is, essentially, a comment on the paper by Bryden et al (hencforth BLC). Had that paper appeared in a regular journal, such as Journal of Physical Oceanography, then this manuscript could, and probably should, have appeared in the Notes and Correspondence section. In fact, Nature also allows responses to its articles, via short Letters, and I'm a little surprised that the authors didn't follow that route.

The authors main point is that Sverdrup balance puts severe constraints on the meridional flow, and so that another, 'more likely', intepretation of the BLC data is that there have been changes in the nonlinear regime off the Florida Straits. I have a major comment and a minor comment.

1. I don't wholly follow the author's arguments, and I conclude that they are not expressed as clearly as they might be. Sverdrup balance provides a relation between the local wind stress and the vertically integrated flow above a level at which the vertical velocity is zero. This level might be taken to be the bottom of the ocean (especially if the bottom is flat) or a level of no motion. In either case it doesn't really constrain the upper level flow. And my interpretation of BLC is that only the vertical distribution of the flow had changed. Their figures are hard to interpet, but in table 1 there is a great deal of cancellation in the flow at various levels, and one interpretation of that table is that the vertically integrated flow has hardly changed, but its vertical distribution has, and this does not violate Sverdrup balance. If de Boer and Johnson are making an different argument they need to be more clear about it.

2. At a more minor level, there is really no need to invoke Ekman dynamics to derive Sverdrup balance. One just integrates the frictional linear vorticity equation from the top of the ocean down to a level of no motion. That is, begin with

$$\beta v = f\frac{\partial w}{\partial z} + \rho^{-1}\nabla \times \frac{\partial \tau}{\partial z} \quad (1)$$

Integrate this from the top (where $w = 0$) to a deep level where $w = 0$ and $tau = 0$ and one obtains

$$\beta[v] = \rho^{-1}\nabla \times \tau \quad (2)$$

where $[v]$ is the vertically integrated wind. This is their equation (6). Alternatively, one could integrate down from the base of the Ekman layer. But it that case, note that dBJ's equation (4) is really incorrect as written, because the upper limit of the integration should be the base of the Ekman layer, not $z = 0$, if the vertical velocity is w_E, the Ekman pumping velocity. It seems that in any case dBJ could begin with their (6), as it is to be found in textbooks.

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Interactive Comment

If the authors can clarify point 1, and fix point 2, I'd be happy to see the manuscript proceed. But if the paper is based on a misunderstanding of the constraints of Sverdrup balance, it should not. In any case, I would caution against using such fighting words as 'more likely interpretation' in the abstract. 'Alternate interpretation' would seem more reasonable. Finally, the authors of the original paper (BLC) should comment.

Interactive comment on Ocean Sci. Discuss., 3, 1653, 2006.