**Ocean Science
Discussions**

Interactive comment on "Retroflection from slanted coastlines – circumventing the "vorticity paradox"" *by* V. Zharkov and D. Nof

V. Zharkov and D. Nof

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Dear reviewer,

Thanks for taking the time to review the document. Please see our response embedded in your review below (in italics).

Comments on *Retroflection from slanted coastlines - circumventing the "vorticity paradox"* by Zharkov and Nof.

This is an interesting manuscript and it is essentially a sequel to an earlier manuscript by Nof and Pichevin (1996). In the earlier study, Pichevin and Nof introduced the "vorticity paradox" which was put forward as an explanation for the relatively small volume transport carried by Aghulas Rings into the Atlantic, relative to the volume transport of the Aghulas Current. This paradox follows from a simple momentum budget. The

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present study extends this result in two important respects: (i) extension of the analysis to include slanted coastlines; (ii) derivation of an analytical solution describing the growth of the rings and their volume flux. The key result is that the vorticity paradox disappears for relatively modest coastline inclinations of just 15 . Both of these extensions represent important contributions.

The manuscript is generally well written and the figures are of the high quality I have come to expect from these authors. I am therefore happy to recommend that the manuscript be published in *Ocean Science* subject to satisfactory corrections addressing the following minor points.

1. Agreement between analytical solution and numerical calculations. The authors claim that the agreement is very good (abstract, section 5.2, section 6), but this really is stretching the point too far. The qualitative agreement is not bad, once the authors allow α to vary due to frictional forces modifying the potential vorticity of the eddies as they grow. But there is still a significant offset ("as expected ... because friction tends to flatten out the eddy") - is there not a danger of trying to use the same argument twice here? I do not want to give the impression that the numerical experiments do not support the theory, merely that the agreement is currently being overstated. A more balanced discussion is needed.

We agree. In our revised version, we stated that the agreement of our model with the numerical simulations is "satisfactory" (instead of "very good"). The phrase about friction tends to flatten out the eddy is deleted from the caption to Fig. 8.

2. Acronyms. This is a personal crusade of mine, but can I plead with the authors to cut down on the use of acronyms? They only serve to reduce the lucidity of scientific papers, which are usually not read from start to finish. Some acronyms, such as PV, MOC, have become so widespread that they are almost as familiar as the full versions.

Others, such as AC and especially BE, are less obvious and could be a real barrier to someone glancing at the paper, or at just one or two of the figures. I suspect that the

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manuscript would be lengthened by just a few lines if these acronyms were avoided altogether.

While we sympathize with what you are saying and we would be happy to join you in your crusade, we think that this particular paper has just way too many places where the same acronyms are repeatedly used (e.g., BE). Their elimination will unnecessarily extend the paper considerably. Accordingly, we only eliminated NBC and EAC (which were used only once) from the acronym list.

3. Non-dimensional parameter, ε(section 2, page 6). This sounds like a fundamental nondimensional parameter although I do not recall seeing it defined elsewhere or being given a name. At midlatitudes, it is essentially a nondimensional deformation radius (relative to the Earth's radius) and it is also the ratio the long Rossby and internal gravity wave speeds. Is there a simple physical interpretation in terms of vorticity that could be helpful to the reader in the present context?

Yes, this is a non-dimensional ratio of the deformation radius to the radius of the earth, and we added a sentence stating that to the text. We are not aware of any interpretations of ε in terms of vorticity. We recognize, however, that it would have been nice to have such an explanation.

4. Basic eddy (section 2, page 7; section 2.1, page 9): How important is the imposed orbital structure (equation 1) of the basic eddy? Is there any simple way of addressing this point? The assumed circular shape of the eddy is discussed briefly on page 9. I assume that the authors will counter that the numerical calculations serve as a check on the validity of the analytical results, and perhaps this is the best one can do.

We agree and see what you are saying. In an attempt to make this clearer, we re-wrote the 3rd sentence after (6) as: "We will see (from our numerical simulations) that this approximation is satisfied for the mean, but not for very small α because, in this limit, the BE is very large (this will be discussed in a companion article)." The assumption regarding the orbital structure is important in the derivation of the expressions for F_3

and F_4, where (1) is used *(see 25-29).*

5. Streamfunction (section 2.2, page 9): Is the use of a streamfunction reasonable given that that u^*h is divergent? (I don't see how the moving reference frame circumvents this issue.) If u^*h is divergent, then to what extent, and how is the streamfunction uniquely defined? (though a projection onto rotational and divergent components?)

The stream-function is defined in the usual manner. This is ok because the vector field $h\vec{^*}$ is approximately non-divergent under our slowly varying approximation. We added an explanation to the text.

6. Bernoulli (section 3.2, page 12): "we also satisfy the Bernoulli along the wall" - Bernoulli what? This doesn't look like Bernoulli's theorem.

You are right. We rewrote this sentence as: "we also satisfy the Bernoulli integral along the wall". The analogous satisfaction on the outer edge of retroflected current is used to obtain (12).

7. Reduced gravity (section 6, page 18): $g' = 2 \times 10^{-3}$m s^{-2} sounds very small, corresponding to a density anomaly of just 0.2 kg m^{-3}.

This was our misprint. The corrected value is $g' = 2 \times 10^{-2}$ ms^{-2}.

We hope that you will find the revised version satisfactory.

Interactive comment on Ocean Sci. Discuss., 5, 1, 2008.

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