Ocean Science Discussions

Interactive comment on “Retroflection from a double slanted coastline – a model for the Agulhas leakage variability” by V. Zharkov et al.

Volodymyr Zharkov, Doron Nof and Wilbert Weijer

Reply to R. Matano

Dear Dr. R. Matano,

Thanks for taking the time to carefully review our manuscript. Please see our responses (in italics) embedded in your review below.

Let me start this review with a hypothetical question. Let’s suppose that we take out all land above the 100 below sea level. In this imaginary world all coastlines would have disappeared but everything else would have remained as it is. The question is: would this drastic change eliminate the Agulhas Current or alter in any significant way its dynamics? It seems to me that the answer is not. The existence of the Agulhas Current, and its behavior, does not depend on what is on the top 100 meters of the water column but on what is below it. The continental slope acts as an insulator, preventing the leakage of energy from the large-scale circulation onto the coast. Thus all the energy transported by Rossby waves is accumulated in the continental slope, not in the coast.
In other words, the existence and behavior of the Agulhas Current does not depend on the shape of the coastline but on the shape of the continental slope. The difference might be of academic interest for places where the continental slope runs parallel to the coastline but not here. The “kink” of the African coastline is not mimicked by the continental slope. That is, the Agulhas Current does not follow the coastline; it follows the shelfbreak, a fact that is obvious in Fig. 1. Thus it seems to me that any theory of the AC that is critically dependent on the “shape” of the African coastline should be deemed with some suspicion. This includes the admittedly elegant but still (in my view) unrealistic study of Ou and de Ruijter. The authors can prove me wrong by running the proposed experiment. To that end they will have to abandon their reduced gravity model and use instead a 3-D model (e.g., POM, ROMS, etc).

Indeed, the reduced gravity model cannot take into account all the specifics of bathymetric effects on shedding regimes. However, our theoretical and numerical examinations of the role played by the coastline kink still have relevance. This is because, even when the shelf break and coastline are not exactly parallel, the shelf break still has a kink (though admittedly, a reduced one). Also, rings shed from the eastward-shifted retroflection propagate westward and avoid the southern part of the Agulhas Bank because its scale is smaller than the scale of one separated eddy.

My major criticism is not really a major criticism. I found this study an interesting and valuable academic exercise. The question of whether changes in the coastline influences the Agulhas Current could be rephrased in terms of whether changes in the shelfbreak or other topographic features orientation produce such changes. This article provides interesting answers to these important questions.

Minor criticisms:
Too many acronyms!

As mentioned in our response to the first reviewer, we have excluded the abbreviations SA and BE. For the acronyms we opted to keep (in accordance with either convention or readers' convenience), we have reviewed/revised the manuscript to make sure that our use of them is judicious. All acronyms are defined in our list of symbols (Table A1).

I don't think that there is any definitive (or even convincing) observational evidence that increases/decreases of the Agulhas C. transport influences the leakage. The proposed theory, however, provides an interesting argument to expect such a relationship. There is no evidence (that I know of) that north/south displacements of the wind stress curl should be followed by similar displacements of the AC. Our own experiments indicate that the AC is sensitive to northward displacements of the wind stress curl but not southward displacements. The anisotropy is explained by the presence of the ACC.
I echo the other reviewer's concerns about the model. It is at least an order of magnitude too viscous.

Please, see our answer to the other reviewer. Since the grid size is also large, the critical parameter -- the diffusion speed (viscosity divided by the length) – is adequate.

Are the results robust to changes in the boundary conditions? From the figures it appears that the outflow (Agulhas Return Current) is imposed just to the south of Africa. In reality it is much farther south (the tip of Africa is at about 34^0S and the zero of the curl of the wind is at 45^0S). How the results would change if the outflow is moved farther south?

If the outflow were moved farther south, the difference between the shedding regimes would probably be reduced, but not strongly so, because the regimes are defined mainly by the incoming current intensity and not by the outflow or ever the position of zero wind stress curl (see the 1st reviewer's note about the wind). In the case of the Agulhas Return Current, the outflow does not follow the zero wind stress curl position exactly but rather is located between 38^0S and 41^0S (see, for example, Lutjeharms, 2006, Fig. 1.2). However, concerning both your question and the other reviewer's note about the role of wind, in our opinion, the position of zero wind stress curl can play at least an indirect role in the change of shedding regimes. During periods of strong incoming flux, when the point of zero wind stress curl is probably shifted a but to the north, the stronger outflow can lead to intensification of the Indian gyre on the whole and Agulhas incoming flux in particular.

Interactive comment on Ocean Sci. Discuss., 7, 1209, 2010.