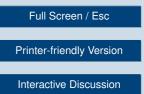**Ocean Science
Discussions**

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

Anonymous Referee #1

Received and published: 23 July 2010

The article discusses how the size and number of Agulhas rings formed from the Ag-
ulhas retroflection depends on the location of the retroflection. The authors continue
from their previous work to show that the "kink" of the African coastline around Port
Elizabeth plays a crucial role in the emergence of multiple regimes of Agulhas ring
formation.

I think the study provides some interesting thoughts on the variability of the Agulhas
system, and how its nonlinear behavior makes it a dynamically very complicated re-
gion. However, I do have some suggestions for the authors to make the article more
readable. I also think that the quality of the numerical simulation is substandard, and
that the authors might consider whether not presenting the numerical simulation will
actually benefit their article.

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The authors use the same numerical model as in their two previous studies, and just as in these other studies, the model is heavily affected by its unrealistically large viscosity. The last decades of numerical ocean modeling have taught the community that models with large viscosity are only of very limited use, and probably of no use in a dynamically complicated region such as the Agulhas. Also, the model is apparently so slow to run that the authors have to use unrealistically values of beta. Only in a linear world would changing beta have no other effect on the circulation than speeding up ring formation. The values of both parameters are thus disturbing to me, and they diminish my confidence in the model being able to realistically simulate an Agulhas retroflection. The authors acknowledge these problems in the manuscript, but do not find them too problematic to abandon the model. Given the current state of numerical model development, I find it amazing that the authors have stayed with a model produced in 1986. It might be that the authors have completely revamped the model to get it in line with the latest parameterizations, but if they did they should say so.

Furthermore, I worry that a model with a rectangular grid is not very suited to study the effect of small changes in coastline. Any slant will be a stair-case like profile on a rectangular grid, and the effect of this staircase like profile on the retroflection is unclear. There are now numerical models with unstructured grids available, that seem much more suited for the kind of studies the authors want to do.

I realize that it might be a lot of work to implement a completely new model. On the other hand, I have the idea that the manuscript without the numerical simulation still contains enough noteworthy new findings from the analytical model and the comparison with the Van Sebille et al 2009a study to warrant publication.

Another, related, issue is that on page 1224, line 13, the authors state that they did not use the strongly slanted section in their analysis of the NPR because "otherwise, the area of our numerically simulated retroflection shifts gradually to Concave III, meaning a restoration of the SIF regime". Does this mean that in their model, the NPR regime is unstable and the SIF regime is the preferred solution? If so, this would be very

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disturbing because it would mean that the behavior in the model is exactly opposite to the real world.

Apart from these major issues, there are a number of minor issues that I would like to see addressed by the authors:

- The number of abbreviations used throughout the text is far too high in my opinion. Furthermore, they are used inconsistently. Indian Ocean is not abbreviated, but South Atlantic is, which results in strange combinations such as on line 5 of page 1224. In my view the article would gain on readability if all two-letter abbreviations were completely written out. In any case, the abbreviations should be omitted from the abstract. The VS abbreviation should be changed to VSa (because there are multiple VS in the references) or (even better) written out completely.

- The authors should be more clear that the slant they are discussing is the zonal (or meridional) slant, not the vertical slant. This is important, because there have been studies of the effect of the effect of the steepness of the continental slope on the Agulhas retroflection. The authors should be more clear that they do not dices these issues.

- The role of the wind in this study is unclear to me. As I understand, the authors follow the VSa study and change the strength of the Agulhas Current inflow. However, at multiple locations through the text (also in the abstract, line 23), the authors relate strong inflow events to the location of the zero wind stress curl latitude. If the authors think these two are related, they should give citations for that. Otherwise, they should omit references to the latitude of zero wind stress curl.

- The authors suggest (page 1212, line 23) that they are the first to relate Agulhas ring shedding to the change in retroflection location. However, others have found that too (Van Sebille et al 2009 OS, Ou and De Ruijter 1986 JPO, Van Sebille et al 2009 GRL)

- On page 1213, line 23, the authors cite Fig 5 of Van Sebille 2009b, where they

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probably mean Fig 3.

- On page 1214, line 9, the authors state that none of the cited studies have addressed the dynamics involved in the anti-correlation. That is not true, as both Ou and De Ruijter and VSa elaborate on why there is an anticorrelation, from a dynamical viewpoint.

- On page 1214, line 24, the authors state that Ou and De Ruijter's theory can only produce cyclonic eddies. This is not true, as is shown in their Figure 15.

- On page 1214, the authors might also want to mention the role of thermocline out-cropping in the detachment of the Agulhas Current from the coast

- On page 1215, line 6, the authors might want to elaborate on what alfa exactly is. It is the control parameter for the rest of their study, so a few more words on its definition and interpretation seem suited.

- On page 1218, line 23, the authors give an approximation for Phi as a function of alpha. It is unclear whether that approximation is analytically derived from the analytical model, or empirically fitted from the results.

- On page 1224, line 17, the authors state that the rings radii "look" greater. Can they quantify this statement, and investigate whether it is really true?

- On page 1224 the authors use the mean square deviation. Do they by that term mean the squared standard deviation? If so, they should take the root of these numbers since otherwise the units do not agree

- On page 1224, the authors might consider putting the numbers from the last para-graph in a table for better readability

- On page 1225, line 11, the authors should write GFDL instead of GFDI

- On page 1225, line 12, what are the radii of the rings in the GFDL video?

- On page 1225, line 26, the authors state that usually 70% of the leakage is carried by

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rings. Can the authors provide a reference for that number. I know of two studies trying to find out how much leakage is carried by Agulhas rings (Doglioli et al 2006 GRL and Van Sebille et al 2010 JGR) and both find that less than half of the leakage is carried by Agulhas rings.

- On page 1225, line 27, the authors calculate that in VSa the alfa parameter is less than 0.13. Does this not mean that their analysis is in the wrong regime, and that they should really focus on the dynamics of the Agulhas Current retroflection between alfa=0 and alfa=0.2?

- In table 1, can the authors elaborate on how they have estimated these values? Furthermore, can the authors produce an estimate of the error, so that readers can assess just how different these values are?

- It is my understanding that in Ocean Science color production of Figures is free. If so, could the authors provide Figures 4 and 5 in color, which will increase their readability?

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

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