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 Referee 1

*Dear reviewer,*

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

The article discusses how the size and number of Agulhas rings formed from the Agulhas 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 region. 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.

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.

*As you mention below, the main idea of our paper is to consider the shedding associated with a coastal kink theoretically and numerically, using a one-and-half-layer nonlinear model. We surely agree that we could have used a more up-to-date and more realistic numerical model, such as one with an unstructured grid. However, the numerics that we use serve merely to confirm our analytical approach. For this purpose – comparison with our theoretical results – the simple one-and-half-layer model with the* $\beta$*-plane approximation is adequate.*

*Also, as mentioned, our theoretical model of eddy shedding is strongly nonlinear. The numerical simulations, where the interaction of eddies with each other and with the coastline boundary is strong, indeed require a relatively large viscosity in order to make the calculations stable for a length of time sufficient to complete the simulations. We do not believe that this is particularly surprising or unexpected. The same could be said for some other widely used models -- for example, HYCOM. Although the diffusivity and viscosity are high, the **grid size** is also large so that the diffusion **speed** is reasonable.*

*We could have restricted ourselves to 300-day simulations, the max shown in Figs. 6-9 (Figs. 6-7 and 9-10 in our revised version); in that case we could have used smaller viscosity coefficients. However, we also examined some statistics regarding the periodicity of shedding during times of NPR and times of SIF (page 1224). These investigations required longer simulations and, therefore, larger viscosity coefficients.*

*The case of a zonal coastline, where the strong viscosity coefficient of 1800* $m^2 s^{-1}$ *is required, was meant to be merely informative and had no bearing on the more central Concave I, II, and III cases. We have dropped mention of this zonal case from our revised manuscript. Also, despite the strong nonlinearity of the problem, the effect of* $\beta$ *on the eddy propagation rate is indeed almost linear, confirming our theoretical approach in this issue (see Nof, 1983).*

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

*We do not think so. In our numerics, we initialize the position of retroflection itself but do not consider its cause. Our intent was to examine the influence of coastline slant alone – only one factor among many in the region. We could not assess the influence of strongly slanted section because, without the influence of those additional factors, strong slant alone produces instability in our particular model. We don't mean to imply that stability/instability in our model reflects real-world "preference" for one regime or another. Addressing that question would require a larger regional model that includes the many additional factors influencing retroflection position (for example, connection to the South Indian gyre or perturbing forces like wind stress).*

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.

*We have deleted the abbreviations SA and BE and changed VS (which is repeated in mny locations) to VSa. However, we have kept some key abbreviations like the two-letter AC. Also, NPR and SIF are important, so we kept them even in the abstract. We also felt that the standard two-letter abbreviation PV is important and have opted to retain it.*

- 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.

*We added a clarification "...(i.e., inclined to zonal by a relatively large angle $\gamma$)..." after the words "strongly slanted" in the second item of Abstract (see page 1210, line 11).*

- 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.

*We expressed our opinion in the answer to Ricardo Matano's last question. Nevertheless, because we are not sure and do not know any citations, we omitted the 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)

*We agree that we are not the first to relate the intensity of shedding to a change in retroflection location. Our idea is to concretely relate the change in retroflection location to the change in the coastline* ***geometry****. We have revised the sentence to make this emphasis/distinction more clear: "We shall suggest here that coastline* ***geometry*** *exerts a fundamental control on these processes – the difference in shedding period is associated with the shifting of the retroflection location from a non-kink coastline to coastline with a kink."*

- On page 1213, line 23, the authors cite Fig 5 of Van Sebille 2009b, where they probably mean Fig 3.

*Here, we erroneously cited Fig. 5 of the OSD version of the Van Sebille paper. In the final OS version, this figure is* ***Fig. 6****, so we corrected our citation. Fig. 3 is related to the westward extension of the Agulhas Current, not to Agulhas leakage.*

- 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.

*In the sentence you mention, we changed "the dynamics" to "the dynamic balance involved in ... modeling." This sentence is not intended to apply to Ou and de Ruijter (1986). Our discussion of that paper begins in the next sentence (line 11 and thereafter). Concerning VSa, do you refer to some of their ideas as expressed on their page 517,* *between Eqs. (2) and (3), with references to Gill et al. (1974) and Pichevin et al. (2009)? We would be inclined to characterize these ideas as assumptions rather than elaborations.*

- 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.

*You are correct. We will revise this statement to read, "The main weakness of the theory of Ou and de Ruijter (1986) is that the curvature of coastline should be sufficiently large in order the rings to be produced, and it is impossible to trace the dynamics of shedding for weak concavities."*

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

*Thanks for your suggestion. We will add: "Also, according to this theory, thermocline outcropping is a necessary condition of detachment of the AC 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.

*OK. Instead of simply stating "twice the Rossby number," we will give an explicit expression for the eddy's orbital velocity that involves $\alpha$ as a coefficient.*

- 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.

*The formula we give follows from our theoretical model but the numerical results confirm it as well. In the revised version, we will have: "As expected from the analytical model, these values can indeed be approximated by $2\alpha(1+\cos\gamma)/(1+2\alpha)$. This result implies that the detached rings compensate for the momentum of both the entire retroflected current and the zonal projection of the incoming current."*

- 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?

*In our new version, we will change "look" to "are". We will also give the averaged values of radii below (page 1225, line 4).*

- 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

*You are right – we used confusing terminology. We meant here standard deviation, not its square. In our revised version, we will write "standard deviation (SD)".*

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

*We considered this but, in our opinion, placing the results in a table could make our comments concerning comparison with theoretical results and the effect of viscosity less understandable. Ultimately, we decided to keep the presentation in a paragraph to be modified as follows:*

*For an initialized $\alpha$ of 1.0, we obtained a mean generation period of 123 days (with a standard deviation, SD, of 40.2 d) and 93 d (with SD of 20.5 d) for SIF and NPR simulations, respectively. The SIF:NPR ratio is 1.32 (SD is 0.36), which is greater than our theoretical value of 1.06. For $\alpha$ of 0.4, the averaged SIF and NPR periods were 118 d (with SD of 43.9 d) and 110 d (with SD of 18.1 d); the ratio is 1.07 (SD is 0.31), which is very close to the theoretical value mentioned above. In our simulations for $\alpha$ of 0.1, the eddy very quickly collapsed due to the relatively greater importance of viscosity, and the estimates of average generation period have large relative errors (60%). The obtained values were 120 d (with SD 71.9 d) for SIF and 145 d (with SD 75.3 d) for NPR, so the ratio is 0.83 and the SD is 0.47. It should be noted, however, that such a variability in the average period was caused by viscosity rather than by the different initialization of $\alpha$. This is because during numerical runs, the eddies' PV was strongly altered by viscosity, so that the averaged values of $\alpha$ were about 0.20-0.25 for all the numerics.*

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

*You are right. We corrected this.*

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

*On average, about 205 km. We added this.*

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

*You are right, this was our mistake. In our revised version, we will have "30-45%" instead of "about 70%" and we give the references you've cited. In accordance with these estimates, we have extended the range of $\alpha$ used for the hatched rectangle in Fig.11, so that the parameter changes from 0.03 to 0.13. This revision does not significantly affect the conclusion we draw from Fig.11.*

- 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?

*Your question is reasonable. We are not sure about the range of $\alpha$ that would exactly fit all possible Agulhas Current regimes. Here, we did consider $\alpha$ values of 0.1 and 0.2 in particular (though, unfortunately, numerical stability decreases for these values). Also, our theoretical analysis of Concave I is more universal and could possibly be applicable to some other western boundary currents. For the North Brazilian Current, for example,* *a value of 0.5 is reasonable (Zharkov & Nof, JPO vol.2, 2010).*

- 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?

*All the numbers here are obtained by numerical realization of our equations (2) and (3), with an error of 0.1% or less. We will have this information in our revised version. Our confidence in the results depends almost completely on the correctness of our theoretical approach itself.*

-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?

*OK, we did it. Also, we provide Fig.1 (adopted from van Sebille (2009)) in a color version.*

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