Review of "Transformation of an Agulhas eddy near the continental slope" by S. Baker-Yeboah, G. R. Flierl , G. G. Sutyrin , and Y. Zhang

This manuscript describes observations and model results on the evolution of Agulhas rings near the southwestern coast of Africa. The article is well written and shows interesting conclusions for this relevant topic in physical oceanography. For instance, the authors show novel results regarding the behaviour of heton-like dipoles in a simplified model, and discuss them in the context of detailed oceanographic observations. In general I recommend publication of the manuscript. However, there are some points which perhaps the authors will find useful to comment or respond:

(1) The formation of a cyclonic vortex due to the interaction of an anticyclone with a shelf is a result that has been addressed in previous papers. This phenomenon has been shown numerically in the works of Smith (cited in the paper), and by the laboratory experiments of Zavala Sanson and van Heijst (2000, JPO, Vol. 30) for barotropic flows. In fact, it is interesting to note that this new vortex is equivalent to the first patch of vorticity that a monopolar vortex creates in the presence of the beta effect. This is not so surprising when considering the (approximate) equivalence between planetary and topographic beta effects.

(2) In this regard, it would be helpful if the authors could quantify the nondimensional topographic beta number associated with the western coast of South Africa (see Zavala Sanson and van Heijst, 2000). This number would give a better idea on the model results that best describe the oceanographic observations.

(3) The problem of a dipole in presence of a step have been also published before. I strongly recommend to look into the analytical, numerical and experimental results of a dipolar vortex approaching a topographic step reported by Tenreiro et al. (2006, Phys. Fluids, Vol. 18, 056603). That study describes the conditions, in terms of the height step and the Rossby number, that determine the flow evolution.

(4) The authors use the expression "image effect" when refering to the motion of a single vortex along a very high step, as in Section 3.1. Strictly speaking, however, this is not due to the image effect. The image effect is associated with the "image" vortex located at the other side of the wall in order to satisfy the impermeable boundary condition; therefore, the vortex speed along the wall is directly related with the strength of the vortex. In the case of the step, in contrast, the speed of the vortex mainly depends on the step height. Unless the authors can show explicitly the equivalence of the two cases, I suggest to omit the expression "image effect".

Minor comments:

(a) Although the paper is pleasant to read, I think some paragraphs are too long. I recommend to clearly separate the information in shorter paragraphs, instead of trying to explain everything in only one (for instance, figures 7, 8 and 9 deserve an independent, short paragraph).

(b) Figures 2, 3 and 4 could be condensed in one single figure with maybe 12 panels. Probably Figure 6 could also be omitted.

(c) Figure 12 showing the lower layer dipole is somewhat redundant, since the results are very similar to the barotropic dipole in Figure 10.