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Interactive comment on "Generation of Rossby waves off the Cape Verde peninsula; role of the coastline" *by* Jérôme Sirven et al.

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Summary and Recommendation This paper explores the possibility to interpret some chlorophyll observations off Cape Verde in terms of a Rossby wave excited by a wind burst, which is supported by some idealised shallow-water numerical experiments. One full section is also dedicated to the theoretical analysis of the impact of the coastline on some of the waves properties such as the existence of a critical latitude. The result that a wind burst should excite both Kelvin and Rossby waves is expected from the existing literature and can hardly be regarded as new. The main novelty seems to be the extensive theoretical treatment of how the waves are impacted by the presence of a cape, which is addressed by rewriting the shallow-water equations in a general orthogonal curvilinear system of coordinates that has one of its coordinate align with

the coastline.

The combination of observations, idealised numerical modelling and theoretical analysis can potentially make the paper a strong one. The paper as currently written, however, feels a bit random at times, as it is not always clear why the authors do what they do, or why it is useful to do it. Moreover, the observational justification for the study is not really satisfactory, while the theoretical part is very hard to follow. I therefore recommend that the paper be returned for major revision aimed at improving the observational part and simplifying/streamlining the theoretical analysis before it can be considered for publication.

Main comments

1. The chlorophyll observations suggesting the presence of a Rossby wave are sketchy at best and hardly convincing, especially given the fact that they are only a very indirect proxy for dynamical activity, in contrast to sea surface height for instance. I believe that the paper would therefore be considerably enhanced by:

   - Adding Hoevmuller diagrams $SSH(x, y_i, t)$ at a number of selected latitudes $y_i(t)$ that could possibly provide some observational evidence of the critical latitude discussed by the authors using AVISO satellite altimeter data.
   - Adding hoevmuller diagrams of $SSH(x(s), y(s), t)$ where $(x(s), y(s)$ are points along the coastline that would indicate the presence of Kelvin or coastally-trapped waves, using AVISO satellite altimeter data. Several studies by Chris Hughes and Alban Lazar for instance have demonstrated that AVISO data can reveal the existence of such coastally trapped waves.
   - It would also be of interest to document the presence of wind bursts from a wind product

2. Numerical experiment. Can the authors justify the way they construct their reference state? How does the reference state impact on the various solutions with and without the cape? Is the solution very different from just imposing the wind burst anomaly on a resting state with a layer of uniform thickness?

3. The theoretical analysis is quite complex and hard to understand. The authors should make an effort as announcing upfront at the beginning of the section exactly what they aim to achieve, and what kind of questions they are trying to solve, and how this is supposed to inform the results of the numerical section. They should also provide more guidance to the reader as why they are doing what they are doing.

   From a physical viewpoint, isn't it more usual to assume the alongshore flow to be geostrophic, i.e., to assume the following approximation

$$-FT_Y + a^{-1}\partial_X \eta = 0$$

$$\partial_t T_Y + FT_X + b^{-1}\partial_Y \eta = 0$$

$$\partial_t \eta + (C^2/(ab))[\partial_X(bT_X) + \partial_Y(aT_Y)] = 0.$$

   It seems to me that this would lead to a simpler equation that the author's equation (7). Can the authors comment on this? Would we expect the results to be different?

   As a result of the theoretical analysis, the authors arrive at some time scales, but it is not clear what these mean since they do not discuss how their shallow water model is calibrated, and to what extent it is representative of the characteristics of the region. Are these values sensitive to the choice of parameters?

Other comments

1. Abstract Line 5: "To verify this hypothesis" I would dispute that this hypothesis has been verified. To claim to have verfified this hypothesis, the authors need to expand the observational part of their paper to include Hoevmuller diagramms of SSH in the offshore and alonshore directions to check for th eexistence of westward propagating signals and coastally-trapped signals, preferanbly by correlating the with the presence of wind bursts.

2. Page 2, lines 7-9: "At a given frequency, there is a critical latitude at which the Kelvin wave no longer exists and are replaced by Rossby waves propagating westward" Probably need to add poleward of which Kelvin waves no longer exist.

3. page 3, Lines 7-8: "A well-defined "sline-like" pattern (figure 1): Could this be circled in the figure to make sure that there is no ambiguity for the reader as to what exactly the authors see in the figure?

4. Page 4, the model. Can the authors explain the usefulness of constructing a motionless background state that has a non-uniform layer thickness? How does that make the model more/less realistic? Can the authors demonstrate that this makes the stratification closer to the observed one in the region? How do the experiments differ if the perturbations are applied to a motionless constant thickness model instead?

5. Page 5, section 3.2. I think 'numerical resolution' should be 'numerical model setup' as I am not sure what 'resolution' mean here.

   - Can the authors explain how their model is calibrated exactly? Do they use one of the calibration methods proposed by Flierl (1981), and if so, which one? What are the climatological observations used for the calibration?
   - What are the boundary conditions used at the open boundaries?

6. Page 5, lines 27-28 "It thus suggest that the latter is a consequence of the existence of Rossby wave generated by a wind burst" THis is not implausible, as it is

difficult to think of what else this could be. Still, the authors should make an effort at significantly improving the observational basis for the hypothesis.

7. Page 6, lines 31: "comparable with the sine pattern observed" What is the basis for such a comparison? I don't really understand how a SSH signal can be compared with a chlorophyll signal, which are physiclally completely different kind of fields, one being passive, the other dynamical. Moreover, one should be proportional to $u' \cdot \nabla \overline{Chlorophyll}$, while the other is proportional to h'. Comparison would make more sense if westward propagation was seen in Aviso SSH data.

8. Page 8, Eq (7). Can the authors explain and justify the derivation of Eq. (7).

9. Page 9, Line 4, "When a and b are close to 1 [...]" This does not seem accurate, as for a pure rotation of the coordinates, which one would use do describe an inclined straight coastline, a and b would be exactly equal to unity, but F would be a function of both X and Y.

10. Page 9, Equation (10) In practice, this seems equivalent to assume the amplitude constant. How would you derive an equation for the amplitude otherwise?

11. Figure 2. Please indicate the timeline for each panel explicitly.

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