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Interactive comment on "A model perspective on the dynamics of the shadow zone of the eastern tropical North Atlantic. Part 1: the poleward slope currents along West Africa" *by* Lala Kounta et al.

Anonymous Referee #2

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This paper investigates the forcing mechanism of the poleward undercurrent at annual and semi-annual frequencies, focusing on the region between 10°N and 20°N. Four distinct processes are looked at: i) local generation of a poleward undercurrent in conjunction with coastal upwelling conditions ii) remote forcing of poleward flow with subsequent propagation in the form of coastal trapped waves iii) local modulation of the nearshore Sverdrup transport in relation with the seasonal cycle of the wind stress curl iv) Rossby wave modes at the semi-annual frequency. The analysis is based on a forced experiment with an OGCM at the resolution $\frac{1}{4}^{\circ}$ over the period 1979-2015.

Considering the scarcity of observations in that region, the model simulation is viewed

as material for improving our knowledge of the boundary current dynamics in this region at seasonal frequency, recognizing that the model set up has certainly limitations (e.g. resolution) to address all aspects of the variability. The analysis provides interesting insights in the variability in this region. It conveys in particular the idea that the westward propagation of energy away from the coastal guide is an important process contributing to the poleward attenuation of the boundary current. This westward propagation of energy is interpreted as resulting from the propagation of a semi-annual/annual extra-tropical Rossby wave, although the agreement between model and linear theory indicates that non-linear dynamics is certainly involved. The analysis also suggests that the poleward flow in the southern part of the Canary current system has a remote component originating from the Gulf of Guinea (i.e. wind forced), which contrasts with former studies that invoke an equatorial origin.

The analysis is based on the estimate of Sverdrup transport and geostrophic meridional transport within a priori determined depth ranges, so as to discriminate locally wind forced process and remote effects (equatorial origin or along the coastal wave guide). It makes use of the linear theory for the interpretation of the variability.

The paper is well written and provides a nice overview of all the potentially important processes in this region that are tested based on the medium resolution model simulation, offering a benchmark for other model analysis and material for the interpretation of data. While it is useful to have this broad perspective of the variability, the caveat is that it feels sometimes that the paper lacks focus making it difficult to retain the main result. I think this problem can be easily overcome through improvement of the presentation. For instance, the main conclusions could be clarified through providing a schematic summarizing the main processes and highlighting the findings of the paper. It might also be useful to summarize in a table the main processes that have been tested, through which diagnostics, and the consistency with previous studies.

They are also in some instances in the text some unclear (or too vague) statements (see specific comments).

General comments:

1) The title should reflect that the paper mostly investigates the seasonal variability of the poleward flow. In fact the paper appears to me as a study of the forcing mechanism of the semi-annual variability more than a study of the dynamics of the shadow zone which refers to the mean deep circulation?

2) The section 6 is devoted to the analysis of the meridional flow seasonal variability in terms of extra-tropical Rossby wave. On the one hand the authors suggest that a semi-annual Rossby wave can radiate off-shore at latitudes South of $\sim 15°$N but in Section 5 they also show that there is a possible remote source of semi-annual poleward flow off WA, implying the propagation of Coastal Trapped Waves at the semi-annual frequency (forced by the winds along the coast of Ivory coast). In the frame of linear theory, at a given frequency, a wave is either trapped along the coast or radiates off-shore, so could you try to reconcile these apparent conflicting results (or clarify the text). This calls also to clarifying what is the forcing mechanism of the semi-annual Rossby wave that is discussed.

Other comments:

p. 2, l. 28-29: "in part because the shape of the African continent produces a curvature of the trade winds" through which process?

p.3, l. 4: AWA not defined

p. 9, l.4-5: Not clear here if the amplitude the semi-annual and annual cycles was actually estimated in observations and model. How do you estimate the 50%?. It would be useful to indicate an error bar (dispersion) associated to the number of years that is retained to calculate the mean of Fig. 5 considering that the observations of Picaut corresponds to the period 57-64. You could select randomly chunks of 8 years in the model and provide the dispersion among the ensemble chunks. This would inform also on the possible influence of decadal variability on the validation.

p. 9, l. 7-9: "To put these biases into perspective it would be useful to know the degree to which the observations in Picaut (1983) are representative of a long alongshore stretch of ocean, as opposed to very local conditions that TROP025 cannot represent due to lack of resolution." Not clear. Do you mean that the observations of Picaut (1983) would account for coastal trapped wave variability, which may not be well simulated in TROP025 owing to the too coarse resolution? What is the critical latitude of the annual and semi-annual period? It could be useful to mention this information at that stage for clarity.

Figure 8: indicate latitude ranges? We assume it is the same than Figure 7?

p. 9, l. 28-30: "In winter and summer a core of poleward flow present a few hundred kilometers from shore is suggestive of westward propagation of the poleward undercurrents." This is not straightforward. It is more like there is an annual Rossby wave that is in phase with the seasonal cycle of the undercurrent? The concept of an undercurrent propagating off-shore as a Rossby wave is not straightforward since the undercurrent is usually tight to the bottom (slope) boundary layer dynamics. The text may need to be clarified.

p. 10, l. 5-6 "Zonal integration is performed from the coast to the first offshore location where the flow changes 5 direction, so the width over which this transport is achieved varies in time." Is it possible to plot the distance from the coast over which the zonal integration is performed on Figure 8a.

p. 10, l. 27-31: You could do maps of the annual (semi-annual) amplitude and phase of the meridional transport of Figure 10 in order to support the interpretation and provide more quantitative statement.

p.11, l. 30.31: It is not clear why it should be "resonant excitation of free Rossby wave modes". It could be locally forced Rossby waves or remotely forced Rossby waves? Can we have purely "resonant" extra-tropical Rossby waves at these latitudes without coupling with wind stress? Any reference to support such a hypothesis considering that the references that are provided are for resonant modes in the equatorial wave guide?

p.14, l. 20-21: "With respect to 4), z18 and V26.7,g are not precisely in phase as they are expected to be for theoretical Kelvin waves (Cushman-Roisin and Beckers, 2011)". This assumes a 1.5-layer (i.e. one barocline mode) dynamics. In a multimode context you can have a phase difference.

p. 14, l. 24 :" Due to Rossby waves pressure fluctuations associated with CTWs propagate offshore" This is not clear. Please rephrase.

p.15, l. 15: "…the presence of a semi-annual Rossby wave coupled to the coastal trapped wave activity". This is unclear. Do you mean the coastal trapped wave activity is concomitant with the semi-annual Rossby wave. From a theoretical point of view, you cannot have a Rossby wave and a coastal trapped wave at the same frequency. The wave is either trapped or radiate as Rossby wave.

p.15, l. 20: "Rn= 50 km which is approximately the value we find at 14°N,18,30°W for the first deformation radius, based on the model stratification". How do you calculate Cn in the model? More details on the method used to derive the baroclinic mode structure should be provided. In particular, do you use "continuous" (interpolated) profiles or does the modes are derived on the model vertical grid?

p. 16, l. 1.: "integration to a layer in which the poleward flow is concentrated, e.g., the upper 200 m, leads to similar results". This is surprising since the baroclinic modes are no longer orthogonal when integrated over such a shallow depth. You may expect contribution of cross terms (i.e. vn.vm)

p16, l. 2: "Mode 2 dominates over most of the ETNA except offshore at latitudes beyond 12°N where mode 1 dominates." Mode 2 seems to dominate over Mode 1 almost every where?

P16, l. 7: "Dominance of mode 2 is a well understood attribute of equatorial/tropical re-

gions (Philander and Pacanowski, 1980)" This depends on stratification and variability. This appears as an excessive generalization. For instance, this is not the case in the equatorial central Pacific. Please rephrase.

p. 16, l. 10-12: "Using the phase speeds indicated in Fig. 15, we find 22oN, 11oN and 7oN for the critical latitudes associated with vertical mode 1, 2 and 3 respectively, in agreement with previous estimates (Clarke and Shi, 1991)." From Table 1 of Clarke and Shi (1991) the first segment (∼8°S) is already beyond the critical latitude of the semi-annual frequency for the second baroclinic mode. Please rephrase. In fact is seems like that linear theory does not apply for the semi-annual cycle?

p. 16, l. 15-16: "Over the continental slope, the progressive deepening of the WABC with increasing latitude in Fig. 9 corresponds to a reduction in the contribution of high-order modes". I would say that this is the contrary, the deeper the WABC, the likely larger contribution of the higher-order modes. This statement would need to be either modified or supported by a specific analysis.

p. 16, l. 22-24: "The northern end of this sector coincides with the critical latitude for baroclinic mode 1 semi annual RWs, hence a potential resonant excitation of these waves because their group velocity vanishes (Hagen, 2005)." Not clear. Please clarify what you mean by "potential resonant excitation".

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