Ocean Science

Discussions

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Interactive comment on "Genesis dynamics of the Angola-Benguela Frontal Zone" *by* Shunya Koseki et al.

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As said in the papers introduction, a quantitative analysis of the frontogenesis in the ABFZ is really not done yet. So, the paper deserves interest and may enhance our knowledge on the dynamics of the boundary between tropical and subtropical waters in Eastern Boundary Currents of the southern Atlantic. So I recommend to publish it. However, I propose to revise the paper first, for two reasons. It has capacity to gain better scientific quality at some points and some steps in the reasoning are either not well described or not justified. The main crititics are on the way how the "residual" term is treated.

Let us go through the paper in detail. Remarks are made in the "order of appearance". This mixes important and less important ones.

40) Does the SST and the ABFZ really influence the rainfall activity or are anomalies

of both related to the same process?

52) To reduce the SST bias in model results, a more realistic wind forcing is needed. This would reduce the overestimated poleward transport of tropical water. Understanding the ABFZ dynamics only does not reduce the model bias. But for sure, understanding something helps to deal better with it. It was a major outcome of the PREFACE projects that the SST bias is mostly due to shortcomings on the atmosphere side.

56) I think, Mohrholz et al. 1999, already gave some insight in the frontal "dynamics" in that sense that processes moving the front are identified. But you are right, a quantitative discussion in terms of hydrodynamic equations was not given there.

71) Just to identify the "current systems", "Benguela current" and "Angola current", is not a discussion of the dynamics. The Benguela current has a more complex structure, one part that should contribute to the frontal dynamics, is the wind driven coastal jet. For the Angola current coastally trapped waves are important.

73) What is meant here? How does the specific cloud cover modify the heat flux components? Is it reducing or enhancing influence?

99) .5 deg resolution is not really good? There are other ocean model products available with better horizontal resolution, e.g. from the ECCO group. What is the motivation of your choice?

102) This justification for using the potential temperature does not hit the point. Please look into some textbook (say Olbers et al., Ocean Dynamics) for the in-situ temperature transport equations. It is a highly complex one. The corresponding analysis of the frontogenetic terms would be a nightmare. Potential temperature or conservative temperature do not stand for a the thermodynamic quantity "temperature". But you are right, potential temperature at the surface is numerically equal to the in-situ temperature. Hence, potential temperature is suitable for your analysis and results can be presented as "temperature". Moreover, the approximate transport equation looks like

a classical advection diffusion equation. May be you just say that you are considering potential temperature? Thats it.

119) What is the relation between Lagrangian and Eularian differentiations? As far as I see just commutation and product rule of differential calculus are considered.

121) In the beginning, you made the very basic assumption that the front orients zonally. Now you consider the transformation of zonal gradients into meridional gradients. Do you consider zonal temperature gradients or do you neglect them? You basic assumption needs a better justification or zonal components of the fronts should be included? My view is that a major contribution to frontogenesis is upwelling and equatorward transport of cold water with the coastal jet. So zonal temperature gradients are very important. See the images in Mohrholz et al. 1999!

131) The equation is not correct. The vertical divergence of the solar radiation is missing.

141) What do you mean with "production remains linear relation". I guess you mean that as long as the temperature is constant within the OML, the vertical averaging over the horizontal velocity can be carried out. Vertical mixing is not "replaced" by the heat fluxes, it is the upper boundary condition of the integrated mixing term. Q_b is the lower boundary condition for the vertical heat flux.

150) Please explain in which sense the mixing at the bottom is "higher order". In Fig. 4 the amplitude of the residual term is the largest of all contributions.

161) This needs to be clarified. Which climatology? Do you calculate velocity and temperature climatologies first and from this the different contributions? Gaps in the model results? How can this be?

170) I would propose to make clear somewhere that "strength of the ABFZ" or "intensity of the ABFZ" means the maximum value of the meridional SST-gradient, doesn't it?

187) Here it must be said that "dynamics" means analysis in terms of forces and re-

sponses. This is not done is this paper. Considering time changes or trajectories without considering the reasons, i.e., the forces, I would speak about "kinematics". In oceanography is becomes colloquial to say "dynamics" to address "time development". This is not correct in the original meaning of the word.

196) Today this cell is mostly called "Kunene upwelling cell", which is related to a very persistent wind patch off the mouth of river Kunene. Please check this.

225) This is not RESD of eq. (3.5). Please explain better.

226) If you calculate the time average, the result is independent of time. Your reasoning is not convincing. Please describe, what you are really doing and how the quantities displayed in the figures are really calculated. Why don't you just calculate the residual term in 3.5? If you integrate the left hand side of 3.5 in time, you get the difference of the meridional temperature gradient between end and beginning of your time series. Is this difference really zero or small? You said earlier that the interannual variability is much higher than the seasonal variability. So, please add a figure that this difference is smaller than all the other terms. If this is not the case, your RESD is wrong.

236) As said before, TILT and CONF are kinematic terms. So they do not "drive" the generation of the front. Something drives the currents causing tilt and confluence. This is mostly the wind (local and remote) and vertical heat fluxes. But for sure TILT and CONF are the main contributions to the OFGF.

238) The phase shift is an interesting point and needs more attention.

242) The box integral stands for the time derivative of the north-south temperature difference. This could be, one may assume this, a constant, but the temperature gradient inside the box may vary between a linear one to a sharp front inside the box. Is the average over the box a good measure for the overall strength of the ABFZ? The maximum gradient, a median of the gradient or the meridional temperature variance may be also an interesting quantity.

259) Please do not mix variables and units. I propose to introduce \Delta t and say this quantity is 1d.

281) The "two different physical processes" are a very important hypothesis. I think that the paper of Mohrholz et al. (1999) shows the action of TILT and CONF in a descriptive manner. In the new paper we see the equations.

300) I cannot see the "mirror image".

302) To see this, a graticule or some "zero line" should be added in Fig. 5 and 6.

309) Which SSH? From the model? How did you calculate OML-depth? Why only the geostrophic component? Some more details in the "data"-section would be of great help. More important, the concept of reasoning should be made clear. So, what is the motivation, to consider geostrophy only? Fig.7 does not show the Ekman transport, which should be an essential part of the surface flow in the trade wind zone. I would expect it as divergent from the pronounced wind stress curl. If not, the model would be fully Sverdrup balanced away from the coast? Please add some remarks on your way of reasoning and comment.

336) A geostrophic balance between a pressure gradient and the Coriolis force of a current does not imply any information on the driving mechanisms. The Angola Gyre may be related to the wind stress curl in that area but the doming structure and the currents develop together. For me it seems to be more reasonable that the dome is result of a flow, but for sure not of the geostrophically balanced one. It is not intension of this paper to explain the dynamics of the dome. Just, please avoid oversimplified unphysical wording. Proposal: replace "to induce" by "related to".

349) what is negative stratification? In Eqs. 3.4 and 3.5 the sign convention of \Delta \theta is not explained. I tried to understand 349/350. Does it mean negative dw/dy and negative stratification (whatever this is) from January to August and positive dw/dy and positive stratification otherwise? Also 354 to 357 leaves me confused. Aug-Sep.

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dw/dy is negative but is positive from August to December. Not quite clear. What is Fig. S1a/b? It cannot be Fig. 1 a/b since this does not show w.

360) What about wind driven mixing?

367) Where are the Figures?

372) This was a chapter on vertical velocity. So, I would have upwelling in my mind, but the chapter closes without even mentioning "wind". So, as a discussion, it remains very technical and less physical. The discussion is mostly on the kinematics of the front and not on the dynamics (in terms of forces).

380) higher order with respect to which parameter?

381) Do you mean "vertical mixing at the bottom of the OML"?

391/2) two maxima of what? Maximum gradient?

414) The variability of the frontal position is not discussed in this paper. However, the common view on the ABFZ variability is that of a seasonally varying meridional movement of the fronts. The stripe between 17°S and 15°S is relatively narrow. Please show that you cover the major part of the ABFZ during all model years.

Fig. 2) Averaging from 10°E to 12°E excludes the coastal areas north and south of 17°S. Fig. 4 shows that this may be a poor approximation. The same applies for other figures using the same approximation. I propose to select a stripe along the coast, probably with a width of some Rossby radii?

Fig. 4) I propose to add a figure showing the time dependent term. It should become small for a long averaging period. But this should be shown. Somewhere in the text the strong interseasonal variability was mentioned. So it could be that this term is large. If the interannual variability is larger than the seasonal cycle, how significant is this figure? For me it is not really satisfying to see that RESD is the largest contribution. I would suggest to write down the details on how the averaged terms (as well as the

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monthly climatologies) are calculated from the daily model output. I think the only term, which cannot be calculated is that related to vertical diffusion at the base of the OML. Even here at least the order of magnitude could be estimated.

Fig 5b) I am missing the other contributions. A zero line would be of some help. The same applies for the following figures.

In summary the paper is of great interest. The message is that just "normal" linear advection and mixing even with coarse resolution may generate a frontal system. However, the word "dynamics" is to promising. The paper considers kinematiks of the fronts, the consideration of dynamics, i.e., a discussion of the relation of the development of the front in relation to atmospheric or other driving forces is still outstanding. Unfortunately the typical wind pattern are neither mentioned nor included in the discussion. However, we should not expect that the authors do in one paper what a whole community could not accomplish within decades. So my proposal would be, not to include an analysis of the winds here and just to consider the paper as a step into this direction.

More details on the data processing are needed, especially on the time averaging to gain the climatology. Sometimes I suspected that contributions to the OFGF are calculated from climatological quantities. Please make a clear statement on the method.

The residual term is to large to be called residual. The remaining terms collected here should get a name and deserve more detailed discussion. What about a \theta/D dw/dx term? The reasoning that the left hand side of Eq. 3.5 is small, which defines RESD (Eq. 5.1) is not convincing. This problem should be reconsidered, Eq. 5.1, if it holds, requires more justification.

The fact that the confluence of the geostrophic flow is the major contribution to the CONF-term is important and should be discussed more explicitly. Fig. 7C shows a strong divergence of the geostrophic flow. The water must stay somewhere. Here it continues most probably as the zonal component of the Ekman transport. Since this term is not in the OFGF, it does not become visible here. So one may conclude that a

consideration of geostrophic currents may be sufficient to understand the time evolution of the frontal system. I am not convinced that this is true. Please consider this issue.