

Interactive comment on “Salinity-induced mixed and barrier layers in the southwestern tropical Atlantic Ocean off the northeast of Brazil” by M. Araujo et al.

Anonymous Referee #1

Received and published: 10 April 2009

This manuscript investigates the vertical stratification of the upper ocean in terms of isothermal and mixed layer depths and barrier layers (BLs). The study is based on the data collected during 2 oceanographic cruises which took place in the southwestern Atlantic off the northeast of Brazil in 1995 and 1997 respectively. The analysis results in some information about BL thickness distribution over the area during winter and summer respectively. The authors also propose a mechanism linked to the intrusion of salty subsurface subtropical waters brought by the south equatorial current into the area.

The topic of the study is doubtless of primary scientific relevance. However, the anal-

C29

ysis that is presented here is weakly documented: no effort is done to test the robustness of the BL distribution against other studies and/or other data set (ARGO for example), the mechanism that is proposed is hardly documented and incomplete, the real findings of the study are not clearly presented and no perspective/comparison with previous studies is proposed. I have the impression much more could be done with the data but at present I have to recommend rejection of the manuscript. However, I add below some comments which I hope can help the authors to improve the analysis and resubmit a manuscript in future.

Major comments:

1. It is claimed that it is the intrusion of subtropical SMW brought by the SEC that is the major process contributing to the seasonal BL formation. I would argue that this is not really demonstrated. Some obscure T/S diagram more or less demonstrate the origin of water masses (see specific comments on these figures below), and only 1 sentence in the text refers to this mechanism using the phrase "... can only be due...". I expect (i) a more convincing argumentation behind this "can only be due" (see suggestions below on the figures etc) and (ii) a discussion of the temperature effect associated to this mechanism: a barrier layer is not only characterized by a marked halocline but also by the lack of temperature gradient associated to this halocline so that the so-called isothermal layer is indeed deeper. What is the temperature of the salinity cores arising from the penetration of the SEC? For what reason would it be similar to that of the surface waters of the region? In other words, why should the isothermal layer be so?

2. de Boyer et al. (2007) and Mignot et al. (2007) recently proposed a global climatology of barrier layer thickness with the corresponding data set online. I expect at least a comparison of the results of this study to theirs. In the southwestern tropical Atlantic ocean off the northeast of Brazil, they reported primarily BLs in austral winter equatorward of the subtropical gyre. Does this correspond to the BLs of 90m deep reported in the "southernmost offshore portion of the area of the study" (p.565 l. 8-10)? What

C30

about the BLs thicker than 30m observed all over the region in austral summer? Is this a new feature that was not in de Boyer & Mignot et al data set because of a lack of specific data?

3. This summer BL is the only one for which a mechanism is approximately proposed (p.564 l.20-end). However, the hydrographic analysis of section 3.1 "concluded" to the influence of SEC transporting SAW south of 6°S. In section 3.2, the same argument is used to justify the fact that "high BL values...not only concentrated in the southernmost part of the study area... but also present from 1° to 10°S". Isn't this inconsistent? Or at least, it should be much more documented and clarified! Abstract and conclusion argue that this mechanism linked to the SEC was demonstrated for both season. I see indeed a discussion of the origin of subsurface salty waters in both seasons in section 3.1 (see general comment 1. above on this point). But I do not see any discussion of the formation mechanism of winter BLs.

4. The language in this paper needs to be improved significantly. In the present state, the reading of the paper is strongly perturbed by the language issue and it is thus difficult to focus on the science. I understand that none of the authors are native english, and I hope that they can find an english speaker around them to help improve the language. Given my overall opinion of the manuscript, I don't think it is necessary at this point that I propose a detailed correction on this point. In general, I advise the authors to use shorter sentences.

Specific points

Fig. 1a: more data are collected in the north and there seems to be focus on topography. Please comment on the reasons and the possible effects of the BLT that are presented.

Fig. 1b & c: Need more information: where do the diagrams for the reference water masses come from? What is the exact location of this "typical" diagram (the black one I guess)? Why not showing all diagrams of easternmost stations between the spec-

C31

ified latitudes? Or is it an average of all easternmost stations between the specified latitudes? Given the topic and the following of the manuscript, some discussion would also be needed on the sensitivity of such diagram to the season. What about NAW waters? Were they not present in the data? Why?

I understand that in Fig. 1a & b and Fig. 2, only easternmost stations are used in order to focus on the entrance of water masses in the study area. If true, this argument should be specified in the text to be more convincing.

p. 561 l. 19-20: It is stated that thresholds to limit the isothermal layer vary from 0.5 to 0.8 degC in the literature. De Boyer Montegut et al. (2004) give a review of studies with criteria varying from 0.1 to 1 degC at least.

There is an inconsistency in the definition of ZT and ZM: they are defined as layers in the abstract and as depths (in m) through equation (1). Please clarify and change the text accordingly (ex. p.564 l. 9, "Zm depths" is not appropriate I think).

p.563 l.10: abbreviation SMW should be specified in the text and not only in the title of the section. How are these waters defined?

p.563 l.14 "this" boundary: what do you refer to? The previous sentences do not refer to any boundary

p.563 l.24-27: This paragraph is crucial for the conclusions of the papers and can not thus simply be based on a "can only be due". See general comment 1. above. At least illustrate the circulation on one of the figures.

p.564 l.6: the word "distribution" is misleading here since the spatial distribution is in fact described in the following paragraph.

p.564 l.6 "lower". Comment that the thickness range is lower but the median higher? Or at least indicate that you will below.

p.564 l.9 "while ZM depths reached 135m in winter". I don't see from the figure where

C32

this depth is reached. So at least use the word "locally". And perhaps give a value that is more representative of Fig. 3b.

p.564 l.10: The comparison of ZT range is not very convincing. I suggest you rather compare maximum values or the median? In addition, an explanation for the depth of the isothermal layer in each season is critically missing (see major comment 2 above). Note that I would also expect an explanation of the depth of ZM in winter. In fact, only ZM in summer is commented l.23-25.

End of the section: the link with the previous section is unclear and confusing. See major comments above.

p.565 l.11: "also": what else? This is the only mechanism that you "discussed" here.

p.565 l.14-19: "these results suggest...". I don't really see the link between this sentence and the following and the results of this paper.

p.565 l. 19 "Nonetheless": there is no opposition with the previous idea. Omit.

Interactive comment on Ocean Sci. Discuss., 6, 557, 2009.