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Comment

## ***Interactive comment on “Intrinsic variability of the Antarctic Circumpolar Current system: low- and high-frequency fluctuations of the Argentine Basin flow” by G. Sgubin et al.***

### **Anonymous Referee #1**

Received and published: 10 December 2013

The scientific content of the manuscript has been clearly improved, in particular regarding the link between high- and low-frequency variabilities. Results are more numerous, better connected mutually and to the existing literature. Despite certain limitations due to the strong idealization (short/periodized basin, apparent limitation of the maximum depth at 4100m?, few vertical levels), the study brings interesting and new scientific results on an important but quite difficult subject.

However, the writing is in contrast with the results : it is often confusing and strongly weakens the scientific content. The writing requires improvement (English quality, accuracy of words and expressions, construction of sentences, clarity, conciseness). I only highlight a few (science-related) wording issues in the following, but a thorough

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rewriting would largely improve the paper (perhaps with an experienced or native English speaker).

I would also suggest a reduction in the number of figures (see below), if possible.

In summary, the scientific content in itself is good and interesting, but I have to request a major revision regarding the expression.

Main remarks =====

- p 1937 lines 20-25. The statement about sigma-coordinate models able to simulate realistic Ekman surface/bottom BL flows is a bit subjective, and too general (in particular these BLs are largely unresolved in this present 12-level implementation). I suggest to drop the part “The POM is a terrain-following. . . steering”

- p 1938 lines 1-13. For clarity, I would suggest to reorder the statements in this part. I would suggest to successively describe the geographical domain, the horizontal and vertical resolution, the boundary conditions, the topography (origin, periodization, smoothing), the parameterizations, and integration strategy (duration, time steps). It is not clear whether the zonal and meridional grid steps are proportional to  $\cos(\text{latitude})$  ; please clarify (the authors may indicate the meridional resolution in km at the northern and southern boundaries). I suggest to replace the expression “Mercator projection” with “Mercator grid”.

- line 14 : replace “drawback” with “limitation”. In fact, the whole sentence deserves rewriting (serious style issue). The pressure gradient error is not rigorously due to “complex” but to “steep” topographies. Please recall the mathematical criterion retained to smooth the topography.

- line 27 : “process studies”.

- line 29 : is the initial stratification “imposed” (this expression suggest temperature is restored to initial condition throughout the integration) ? Please remove “imposed” if not.

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- p 1939, line 4-5 : what is the structure and strength of the surface heat flux? How is it implemented? Please avoid the somewhat ambiguous word “prescribed” here as well. The expressions “total mixing” and “vertical mixing” are not clear (if not erroneous in this argument). Please be more accurate in the physical argument and expression.
- line 10 : I would suggest to replace “is small” with “appears weaker”.
- line 12 : “quite well captured” as compared to which (published) reference?
- p 1940 line 5 : “quite impressive” is not informative. Please remove and modify the sentence accordingly.
- Section 3 : Please present (here or for instance in a subsection 2.2 called “post-processing”) the filtering process, and precisely introduce the frequency bands that will be discussed in the rest of the paper. There seems to be 3 bands of interest (at least in the last figure) : low ( $T > 200$  days), medium (100-150) and high (0-50) frequency. The depiction of interactions between these various frequency bands would be much more informative (and easy to write and read). Please avoid confusing sentences, such as line 1 in page 1945 where the 100-160 day period is called “high-frequency” (this band corresponds to 0-50 day elsewhere). Finally, the interval 0-50 day is not valid : if the output archiving frequency were e.g. 5 days, then the 0-50 band should be referred to as “10-50 days”.
- line 21 (and throughout the paper, including figures/captions if needed). The expression “Quasi-climatological state” is not very clear and may be confusing. I would suggest replacing it with e.g. “active state”. The whole sentence is confusing as well (the word “thus” is not justified) : please remove this word (or justify it), and split at the end of line 21.
- p 1941 line 16 : “shorter than  $T=200$  days”
- lines 19-22 : Why precisely is this correspondence interesting. Wouldn't one expect to find a minimum (i.e. not a maximum) in variance surrounded by local maxima at the

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center of Fu et al.'s dipole ?

- Figs 13 and 14 seem to show the same link between low- and high-frequency signals. Also, it seems that Figs 15 and 16 exhibit similar behaviors. Please consider showing the results either in P2 or in P3 if possible. Also, is it necessary to show 12 (small but crowded) subplots in Figs 9, 15 and 16 ? It seems to me that one sequence of 6 (larger) plots would illustrate the processes discussed in the text.
- page 1942 lines 4-6 : very confusing sentence. Please separate the comments on each period band.
- page 1943, line 7 : “generated resonantly” is not clear ; please explain (or remove the word resonantly which does not seem useful).
- lines 21-22 : please remove “in a unique region in this respect”. This statement is questionable and not really useful.
- page 1944 line 18 : “main”
- page 1946 line 19 : please replace “temporal” with “frequency”.

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Interactive comment on Ocean Sci. Discuss., 10, 1933, 2013.

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