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Ocean Science
Discussions

# *Interactive comment on* "Observed response of the marine atmospheric boundary layer to the Southern Ocean fronts during the IPY BGH 2008 cruise" *by* C. Messager et al.

**C. Messager et al.**

christophe.messager@ifremer.fr

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The current comment is a second one. Indeed, the OS editor removed the previous comment from the website after a request of the authors. The previous comment was actually out of a scientific scope and was a collection of arguments against authors rather than the paper itself and the science attached. For instance, the referee #1 mentioned that there was no need to "go into a detailed review" (!?) which is incredible in a scientific review. Despite a new post from the referee #1, with the most irrelevant comments removed, a fair review is questionable (difficult to forget the comments of the first post). The general referee comments with no arguments are not addressed



hereafter by the authors, however, authors answer to some of the most scientific and relevant general comments.

The authors propose a substantial upgrade of the manuscript following the constructive suggestion performed by referees #2 and #3. The presentation and the general organization of the paper will be deeply revised. The paper will be reorganized to present general information as well as a selection of a case of frontal influence with eddy M, a case of diurnal cycle and inference on MABL structure and a case when the changes across fronts are masked by synoptic variability. Note that in his previous comment (the one removed), as well as in its current introduction, referee #1 wrote that absence of diurnal cycle is trivial over the Southern Ocean. In the fourth paragraph of the actual version, he wrote that the absence or presence of diurnal cycle above the Southern ocean could be another paper... so it is not so evident or trivial??... For our point of view, trivial topic or "evidence" must be verified by in-situ measurement, especially over the Southern Ocean where direct observations are rare.

Moreover, the title of the paper will change in order to better fit the actual topic addressed.

The zooms of the lower MABL profiles will be provided for the specific cases discussed (consequently, new figures will be added). However, the figures 3 and 4 will still contain all the vertical profiles. The information about this region are rare and deserved to be communicated to a large community (as mentioned by reviewers 2 and 3), beyond the scope of this paper. For instance, the profile aloft the MABL is interesting for global modeler (analysis) and regional circulation (around Antarctica). However, there is a readability problem with the Figures 3 and 4 as currently stated. Authors will split them to increase their sizes. Additionally the number of curves by frame will be decreased in Figures 2,5,7 (an additional frame will be added for each one).

The referee #1 mentioned that "Meteorological results and air sea interaction derived quantity should be presented as a function of time (and not position because the ship

stops and goes for CTD stations), zooming across the front." ....the question is consequently: is the referee #1 actually read the paper? Indeed all the series figures (2, 5, 7, 8, 11) are presented as a function of time!!

The referee #1 wrote "The authors should complement the cruise data with satellite remote sensing estimates of wind speed to see if indeed there were some homogeneous changes across front when they were measuring said changes. That should be easy for them as the first authors indeed did a comparison between wind estimate from satellite and model during the cruise (Messager and Faure, 2012, mentioned in the paper). Only then they will be able to use the radiosondages info to look and MABL modification, a useful result that would be worth to publish, especially if they can look at the internal boundary layer in detail." ..... Messager et Faure, 2012 only validated the surface parameters, not the MABL profiles. Moreover, it is a strange scientific methodology to use remote sensing estimates of wind speed to see if indeed there were some homogeneous changes across front, and then use the radiosoundings...what is the measurement of the reality for this referee? Remote sensing data or in-situ observations? The use of remote-sensing would be (in the next revised paper version) more developed in a way suggested by referee #2.

"a) A Bowen ration of 0.04 is probably the results of a mistake in the calculation of the turbulent fluxes. This would indicate that the latent heat fluxes are 25 time the value of the sensible heat fluxes." The derived heat fluxes were compared to the Woods Hole OAflux latent and sensible heat fluxes. A high correlation was found between them. Then, it has been shown that the latent heat fluxes are much higher than the sensible ones (c.f. Messager et Faure, 2012 - cited by the referee #1). Anyway, the mention to the Bowen ratio will be removed from the revised version of the paper.

"b) The variation of the net heat fluxes seems to follow the short wave radiation indicating that latent and sensible heat fluxes have little role to play in the net heat budget. How does it fit with the papers main theme that seeks to highlight the role of the turbulent latent heat fluxes?" ?



"c) Nowhere is presented or shown the stability parameters, so it is impossible to know when conditions are unstable or stable, or neutral except when the authors claim it is. In the paper, a plot with the stability parameter is need. It seems that in fact conditions are neutral most of the time during the cruise, a case never mentioned by the authors that also question the veracity of the results." The delightful referee #1 would be happy to know that the stability parameter will be quantitatively presented for the specific cases that will be developed in the revised version.

" "The interactions were mainly investigated in the Northern Hemisphere western boundary current systems" This is not correct; many studies focus on the Agulhas Current system, the Agulhas Current (Chelton et al, O'Neill et al, Song et al, Liu et al, and White et al) " But it is correct! Many studies focus also on the Agulhas Current system and the Agulhas Current (the ones cited by the referee #1 are also cited in the authors paper) but the main part of the papers are dedicated to the Northern Hemisphere western boundary current systems.

" "However, no atmospheric campaign crossed successively the Cape Basin, the S-STF (covering the western subtropical convergence windward the Agulhas Retroflection area). " This is not correct, 3 of the mentioned paper did. Moreover, the authors have chosen not to present radiosondages across the Agulhas Current or in the Cape Basin." Here the referee #1 extracts a part of a sentence which is really unfair. The sentence is -However, no atmospheric campaign crossed successively the Cape Basin, the S-STF (covering the western subtropical convergence windward the Agulhas Retroflection area), and THE ACC FRONTS.- Which is true since the paper mentioned by the referee #1 did not bring information until the ACC fronts.

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Interactive comment on Ocean Sci. Discuss., 9, 1387, 2012.