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

Anonymous Referee #3

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The paper by Messenger et al. presents meteorological and in particular radiosonde data from a cruise through the Southern Ocean south of South Africa. In principal, the data from the cruise bears sufficient material worth being published. However, I can recommend a publication of the manuscript only after major revision.

I have several major concerns: I agree with the other referees that it reads more as a cruise report. It is difficult to derive, as a reader, from the presented graphs the information for the statements the authors are making. It would be worth to zoom in into the altitudes important for the stability and boundary layer discussions and not to show the complete altitude range (Figs. 3, 4). Also, for the reader it is not easy to

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follow all the statements made referring to the time series graphs (Figs. 2, 5, 7). Again, zooming in into special cases would be better.

The presentation and discussion of the turbulent fluxes have deficiencies. In section 3, the authors mention that they have derived the surface turbulent fluxes using a bulk parameterization. In the same section, they mention that a sonic anemometer was installed during the cruise. It is not really clear, if the turbulent fluxes of sensible and latent heat were derived from the sonic's data or from the bulk parameterization. If the fluxes are derived via the parameterizations, then the authors should explain why not the sonic anemometer measurements (commonly the reference method) have been used. Also, a parameterization of turbulent fluxes has to be accompanied by careful quality control and is not always applicable (e.g., stability of the boundary layer as criterion). If the fluxes have been derived with the sonic's data, then it is necessary to describe how sensible and latent heat flux were separated. Have there been specific high frequency temperature and humidity sensors? If not, than one derives from a sonic anemometer (and its acoustic temperature which can be approximated as the virtual temperature) the buoyancy flux including both fluxes. A separation is then only possible in regions of high specific humidity. Therefore, the author's discussion of the results for the sensible and latent heat flux is difficult to follow. E.g., on page 1402, line 25, the sensible and latent heat fluxes are not turbulent fluxes of momentum (line 25). The turbulent momentum flux is derived from the fluctuations of vertical and horizontal wind speed. In the conclusion, the authors mention also a mean Bowen Ratio. If it is not clear how sensible and latent heat fluxes were exactly determined, it is not possible to interpret the Bowen Ratio values as a reader.

The authors are often discussing the stability of the boundary layer. Why do they not use the sonic anemometer (which is a reference instrument for it) and the parameters retrievable from it (Obukhov length L , stability parameter $\zeta=z/L$)? E.g., on page 1409 they are using instead the bulk Richardson number. It is also often used, but there was a sonic, so why not using this? For all discussions of the stability of the MABL in the

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Interactive
Comment

various cases the authors describe, they could, in addition to the potential temperature profile, also use the stability parameter $\zeta=z/L$. The many radiosonde profiles and the sonic anemometer measurements together bear sufficient interesting material to investigate the structure and evolution of the MABL. But unfortunately, it is not accordingly exploited.

I have also concerns regarding the Instruments and data section. The authors state that ‘attention was paid to limit the effect of the ship on each instrument . . .’. Has the effect of flow distortion caused by the ship on the measurements, in particular the wind speed measurements, been taken into account? This is particularly important, if the turbulent fluxes have been determined via a bulk parameterization. In addition, there are no specifications or references for the METEO-FRANCE ‘BATOS’ station. The authors mention quality control and assurance. They should be more precise and give exact criteria for excluding or accepting data. They also mention Weatherpack and M-AERI temperature comparisons. They should again elaborate more on, e.g., exact correlation numbers, biases.

The manuscript should be proofread by a native English speaker. There are many phrases with grammatical errors, which make the reading of the manuscript difficult.

The first part of section 4 (all the part before subsection 4.1 begins) could be included in the introduction.

Technical comments: Mentioned reference Branellec et al., 2010 (page 1393, line 11) not in reference section. Some abbreviations, acronyms are not explained (not all readers are common with them): XBT, LLJ, TKE Improvement of graphs: generally, increase letter size. Fig. 3: denote clearly which x-axis for what, units of altitude; Fig. 4: same; Figs. 6, 8, 9, 10: crosses and ‘O’ hardly to be seen

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