General comments

I recommend against publication of that paper in the present form. The radiosondages done during the cruise, especially across fronts, represent an original dataset that should be better exploited. It seemed to be the focus of that paper according to the introduction. At the moment, the figures do not allow assessing the veracity of the author's allegations on the origin and mechanisms of changes, if any, in the marine atmospheric boundary layer and atmospheric surface characteristics across sea surface temperature fronts. Many side results are trivial and do not need to be mentioned (i.e. the absence of a diurnal cycle in the Southern Ocean, storms mix the marine boundary layer, shear dominate buoyancy when the wind is strong, diurnal cycle of net heat fluxes is dominated by short wave radiation). A substantial upgrade is needed, especially concerning air sea interaction terminology and grammar and the physics of air sea interaction. As a result, the paper is sometimes difficult to follow. The authors should seek an air sea interaction specialist to help with the air sea interaction and marine meteorology terminology and the underpinning physics.

The figures look like they come from the cruise data report. There is no need to present the all cruise dataset or the radiosondages at their all heights from top to bottom of the troposphere. 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. A vertical low level zoom is also needed to assess results concerning marine atmospheric boundary layer modification. Only then One can assess if indeed wind increase or decrease across front and if the MABL is changed and how. However, to study those changes with a slow moving ship is a difficult exercise to do and this is why previous study mentioned in the introduction did not use ship data for that purpose but satellite estimates. When the weather changes very quickly and is often at the origin of changes, it is difficult to use ship measurements; hence the data should be presented as a function of time.

The in situ paper on the topics mentioned in the introduction using radiosondages pave the way on what need to be calculated, how relevant parameters should be done and what terminology to use.

One of the results discussed by the authors concerns the absence or presence of a diurnal cycle above the Southern Ocean. That could be another paper altogether and this not related to the main theme of the paper

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. In fact the only original and novel aspect of the measurements taken during the cruise is related to the radiosondages done across

fronts. It would be very interesting to focus on that point and present relevant radiosondages only, zooming on the first thousand meters only.

There are some other fundamental mistakes that also question the results and calculations done here.

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.

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. Also this property applied to the constant flux layer and not the entire boundary layer. This is one of the numerous utterances that call for an air sea interaction specialists to intervene in that paper.

Specific comments

Abstracts

"The MABL is stabilized (destabilized)".

This is not the proper wording. One should write "the MABL is neutral, stable or unstable. This is the first of many utterances that are plaguing the text that show that the paper needs a major revamping. Better grammar and ad hoc air sea interaction and marine meteorology are needed.

Introduction

"All these papers described the SST fronts interaction with the atmosphere and the wind changes near the surface and throughout of the Marine Atmospheric Boundary Layer (MABL)".

This is not correct, although some papers present MABL characteristics; few mentioned wind changes because a vessel is too slow when compared with synoptic weather changes. Please read those papers more carefully. Also only a few of the mention papers have results across fronts

"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)

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.

Also puzzling is that the first radiosondages is at 38 S and only 3 radiosondages were launched north of 40 S. It is said that the cruise started the 13 of February while the first radiosondages has a 23 of February stamp. I supposed that the vessel was in the Agulhas Current system previous to that date. The radiosondages and meteorological measurement as well as air sea interaction quantities should be presented in that paper.

"The global warm exchanges between the Indian and the Atlantic oceans as well as with the atmosphere"

No clear what is meant here, it needs to be rephrased

2 Ocean dynamics and fronts

Figure 1: A microwave SST images superimpose with SSH is needed here to assess the SST gradient and the location of the front.

A lot of the writing for that section is quite general and belongs to the introduction. This should be incorporated in the result section.

3 Instruments and data

It is not clear how the long wave radiation was calculated as there is no mention of the outgoing longwave component

The all section can be shortened. Although is it good to know that quality control and comparison was done, there are no graphs to show they were successful. Also a lot of the instruments are not used in the paper. (I.e. turbulence statistics using the sonic anemometer) by the authors who end up using a bulk formula rather than the inertial dissipation method

4 Ocean fronts and MABL

The atmospheric vertical potential temperature profiles is expected to be stabilized (or neutrally stabilized)

This is an oxymoron and the wording is not correct. This is now the start of a long list of problems and allegation that cannot be sustained by the figures. The general comments apply from that section all the way to the conclusion and a major revamp of the is needed before one can review the results. I stop the review here and hope for a major revision that will follow my recommendations.

My last advice is to get an air sea interaction specialist on board to help with the terminology and the underpinning physics.