Review of "*Marine aerosol properties over the Southern Ocean in relation to the wintertime meteorological conditions*"
by Thomas et al.

I have now read the manuscript of Thomas et al. which concerns (optical) properties of the Southern Ocean marine aerosols.

My overall recommendation is major revision. Indeed, the present manuscript suffers some flaws that need to be addressed. I have also some requirements and points the authors must clarify. Below are some comments.

- The authors should present a geographic map to locate the investigated area(s).
- An important reference (to be quoted) about marine aerosols in the Indian Southern Ocean is Mallet et al. (2018) "*Marine aerosol distribution and variability over the pristine Southern Indian Ocean*" . Atm. Env. 182.

l. 41 : interference → influence
l. 104 : Indicate what is $u_10^{3.5}$. It is clear but explicit indication would help some readers
l. 143 "their contribution was little and did not make much difference to" : This must be quantified
l. 146: "The profiles over the sea-ice and within 50 km from the ice-edge are removed to avoid potential contamination and misclassification due to the wind-blown snow/ice crystals." : This sentence is unclear and must be rephrased.
l. 167: How did the authors obtain the backscatter coefficient? And, later, the extinction coefficient? Data? Model? Is it a coefficient or a cross section? Particles are supposed to be spherical in the calculation?
P25 and P75 must be explicitely specified. 25%ile and 75%ile are not standard notations.
l.170: Is it the optical index or the refractive index (real part of the optical index)?
l. 175 and around: The considerations given must be quantified.
Fig.1 and others: It is quite strange to have a decreasing axis of RH. I assume it is because absolute humidity is higher close to the sea. It is not necessary the same for RH since it depends on temperature.
l.190-195: The authors talk about the aerosol extinction, so do they really include absorption in their discussion? Sea salt are not very absorbing. They say that ‘similar conclusions can be derived for the aerosol extinction’. This must be detailed a bit more. Also, what is the relation with RH and aerosol size?
l. 212: water absorption → water uptake
Legend of Fig.4: symbol for steradian is sr and not Sr
Section 3.3: The authors use a vertical distribution. They must either include an altitude axis z (km or m) to help readers understanding or convert pressure into altitude.

It is not surprising that total backscattering increases with RH and p: RH provides water vapour and high pressure and low temperature (low thermal agitation) favor water uptake. Figure 6 is not at all unexpected. So, do the results obtained really original? This is a serious flaw of the paper: the authors present scatterplots and make only general comments on what they see on their figures. Also, it would be a clear advance in the field to propose a parameterization of the relations between RH and basckscattering and extinction (and/or absorption). Moreover, since RH contains both absolute humidity (AH) and T, the same study using AH would be useful. Such considerations are important then for modeling.
Finally, since the authors recall in their introduction that marine aerosols are important for climate change, they should estimate radiative forcing due to water uptake by marine aerosol (it is not enough for me to focus only (qualitatively) on data about RH and optical properties as done here).
l.278: cubes → polyhedrons

l.285: depolarization is interesting to discriminate dry sea salts from wet ones. Can the authors use their data of depolarization to perform such a discrimination? It is essential since both kind of aerosols do not have the same radiative (direct/indirect) effects. This would be an added value to the study presented.

In the conclusion, joint histograms are mentioned. I did not see any histogram in the manuscript.

For all of the points listed above, I do not recommend a publication before major revisions have been done.