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Interactive comment on "Southern Ocean Cloud and Aerosol data: a compilation of measurements from the 2018 Southern Ocean Ross Sea Marine Ecosystems and Environment voyage" *by* Stefanie Kremser et al.

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1 Response to Gerald Mace

I have reviewed the manuscript describing the data set collected during the TAN1802voyage. The data collected on this voyage to the Ross Sea in January and February 2018 and the manuscript describing the data are quite interesting and very well done. Data sets like this to remote areas are rare and extraordinarily valuable

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because they document unique regions and processes. This data set highlights interactions between the marine biogeochemistry and the atmosphere that are largely unobserved. It is to the authors' credit the they are willing to make this data available to the community.

Community access to this data will certainly maximize the value of the data.

Overall, the manuscript is well done and nicely describes the data set. I think that a bit more detail could be included for not a lot of work that could highlight how the dataset fits into a larger context. I highlight those points below in my comments. Overall, I suggest no major revisions, and I think the manuscript should be published after minor revisions.

We would like to thank Gerald Mace for taking the time to review this paper and for his helpful comments that have led to improvements of the paper. His comments are repeated below in blue with our reply in black.

Specific Comments:

Abstract Line 5: Satellite measurements certainly see the low level clouds without much trouble. The satellite measurements just can't constrain the aerosol-cloud processes very well. Maybe a bit of clarification on this point.

We have rewritten the sentence for clarification following the reviewer's suggestion and would like to further comment that while passive satellite instruments can observe the cloud top, and active satellite instruments whole clouds (if they are not too opaque) active instruments (such as CloudSat, CALIPSO) cannot observe low clouds well. For CloudSat this is because of ground clutter in the first 1 to 2 km from the ground, and for CALIPSO this is because the laser signal cannot generally pass through anything but thin clouds. Passive satellite instruments and CALIPSO cannot observe low clouds if they are obscured by mid- or high-level clouds. Ground-based remote sensing observations can provide cloud base height and make it possible to identify more accurately

cloud types such as stratocumulus and stratus (which are common in the Southern Ocean region) and their horizontal and vertical extent. For a more detailed discussion we refer to McErlich et al. (2021).

Reference

McErlich, C., McDonald, A., Schuddeboom, A., Silber, I. (2021). Comparing satelliteâĂŘ and groundâĂŘbased observations of cloud occurrence over high southern latitude. Journal of Geophysical Research, 126, e2020JD033607. https://doi.org/10.1029/2020JD033607.

Line 55. No argument from me about the need to know INP better, but doesn't the low concentrations of INP suggest that it is very important to understand secondary ice processes much better than we do in this region of low INP?

It is a good point raised by the reviewer, and we added two sentences in the revised manuscript to highlight the need for a better understanding and representation of secondary ice formation processes.

line 76: Do you mean to say that "process model predict that the highest open ocean OCS fluxes are in the Southern Ocean"? Your sentence is missing "that" after "predict".

We reworded the sentence for clarification.

Line 93 (first bullet): "low/medium" is a rather ambiguous term. Do you mean clouds based in the boundary layer or clouds that are not cirrus or...?

The reviewer is correct that this objective is unclear, and we have changed the wording. The voyage objective was to look at low and middle level clouds, using the WMO standard height definitions for polar clouds, i.e. low level clouds below 2 km and middle level clouds between 2 and 4 km. We have revised the research aim statement in the revised manuscript.

Table 2 and Science Objectives: It might be useful to readers to number your science

objective listed in 2.1 and then in Table 2 include a column that connects science objectives with measurements - something like a science traceability matrix.

That is a good suggestion by the reviewer, and we have followed his advice and now provide an additional column in Table 2 that links the instrument to the research objective listed in section 2.1.

Line 104: The heading text ("Meteorological conditions") is not descriptive of what is in the subsection.

We changed the heading from “Meteorological conditions throughout the voyage” to “Meteorological measurements and metadata”.

Line 112: Is 1-minute the highest rate at which motion and position are recorded?

No, sensors record at different time resolutions and raw data are also available at higher frequencies from some sensors, e.g. data on ships attitude (pitch, roll, heave) and heading from a POS/MV Model 320 are recorded at 2 Hz. However, the DAS (The Tangaroa Data Acquisition System) stores the data at one-minute intervals of common variables, which is referred to in the manuscript and which is the temporal resolution of the data set provided. For examining general spatial relationships in the data, 1- or 5-minute resolution is a useful common period available for multiple sensors.

Line 123: Is there a reference describing the gas exchange algorithm?

The reference describing the gas exchange algorithm is mentioned in section 4.3.1 - we have now included the appropriate references upfront as well.

Line 154: Line 154: You don’t say how often the data are recorded from the radiosondes. The temporal or vertical resolution would be useful.

We have followed the suggested by the reviewer and included additional information in the revised manuscript.

Line 184: Oh No! Major Bummer!

Indeed, the loss of the UAV was disappointing but with the measurements from the first flight, we can show the value of UAV measurements in the Southern Ocean and we have learned a lot about the operation of an UAV in the Southern Ocean. The UAV setup was experimental, and the aim was to explore the potential of using an UAV in the SO. For future operations of an UAV in the Southern Ocean region we would recommend that the UAV is first tested in low temperature conditions and admittedly training the person who is going to operate the UAV is important too.

Line 220: Do you mean the maximum detection range? Statement makes it sound like it does not record below the upper troposphere.

Yes, the reviewer is correct that the wording of this sentence is misleading. We reworded the sentence for clarification.

Line 232: How often did the instrument/software fail?

During the six-week voyage, MPL measurements are not available or are unreasonable on 9 days, due to the software failure mentioned in the manuscript. We edited the sentence in the revised manuscript and also added that information to the data set description on Zenodo.

Line 252: would be good to note the temporal and vertical resolutions of these files. Do the files include the attenuated backscatter of the lidars?

We added additional information about the resolution to the revised manuscript. In response to the reviewer comment, the MPL and ceilometer data files include the normalised relative backscatter (NRB) signal, which is the attenuated backscatter after multiple corrections (as explained in l. 23-237 in the original manuscript). The ceilometer backscatter variable is the attenuated backscatter as well.

Section 3.4: Was the MRR on a stabilized platform?

No, the MRR was not on a stabilized platform.

If not the Doppler information will be difficult to interpret due to ship motions. Also, is the MRR calibrated against some independent radar measurement? These are both critical details.

The Doppler information is integrated over a period, which means that variations in the ship motion will be more likely to impact spectral width (i.e. the variance of the Doppler velocity) rather than the value derived. We agree that this will add additional uncertainty to the derived precipitation data, but not necessarily more than other underlying assumptions. Note MRR are generally not calibrated against other instruments apart from disdrometers. However, we did not have one of these instruments onboard and therefore the MRR was not calibrated. This also potentially adds uncertainty into the mix. An extra paragraph was added to the revised manuscript.

Line 277: This is a fairly sophisticated camera. Any chance the RGB imagery could be calibrated?

Unfortunately, this is not possible. The primary use was to use ratios between the color values, with thresholds tuned to the camera itself, so calibration was unnecessary. A color profile could be created and applied, but then the thresholding algorithm would need to be retuned for them to relate to the cloud fraction output.

Line 370: This seem like an important step. Should it be described in more detail? What does "corrected to ambient" mean?

We apologize for the vague language. Correcting to “ambient” means that we corrected the concentrations to standard temperature and pressure. This has been clarified in the revised manuscript. Furthermore, some additional text describing the size-dependent losses has been added.

Line 449: so the the archived data are temperature dependent number concentrations of INP one per filter? Perhaps clarify? Was additional chemistry done on these filters? Could it be if not?

Yes, that's correct. To be clearer, we have amended the following sentence from "Temperature spectra of INP concentrations active via immersion freezing were obtained..." to "Temperature dependent number concentrations of INPs active via immersion freezing (one spectrum per filter) were obtained. . .". No additional chemistry was performed on any filters, but is planned for the subset that coincide with the passage of the ship east of Macquarie Island in mid Feb, 2018 during the MICRE campaign. INPs were being collected on the island at the time, using the same filter system, and samples were later treated with heat and hydrogen peroxide to assess "biological", total organic and mineral INP contributions. It will be valuable to compare not only base INP concentrations but also relative abundances of these three components on the island and 70 east of it during the same period.

Figure 4: It would be useful to plot these over an ocean color or chlorophyll-a retrieval map. I eyeballed the map that can be created at https://oceancolor.gsfc.nasa.gov/l3/and the monthly plot for February 2018 shows some interesting structure in your region that seems to correspond to your data.

We thank the reviewer for this suggestion. However, we would prefer not to include any information about ocean color or chlorophyll-a as going into the biogeochem or biological links to any extent is beyond the scope of this study and will be likely part of future publications. This paper focuses on the description of the data sets obtained during the cruise and by providing information about the times and position coordinates of the ship, future users can easily explore correlations of these data to remotely sensed images of SST, ocean color or chlorophyll-a. As a result, we decided not to include any information about ocean color or chlorophyll-a as we expect some interesting research papers to follow this study.

Line 591: If the MRR is not stabilized the Doppler information is likely not suitable for the Maahn and Kolias algorithm. To my eye, the Doppler velocity looks to include ship motions but it is very hard to discern at the plotting resolution.

The Doppler information is integrated over a period, which means that variations in the ship motion will be more likely to impact spectral width (i.e. the variance of the Doppler velocity) rather than the value derived. We agree that this will add additional uncertainty to the derived precipitation data, but not necessarily more than other underlying assumptions. Note MRR are generally not calibrated against other instruments apart from disdrometers. However, we did not have one of these instruments onboard and therefore the MRR was not calibrated. This also potentially adds uncertainty into the mix. An extra paragraph was added to the revised manuscript for clarification.

Line 618: What fraction of the data are deemed contaminated from exhaust? A day by day graphic showing the contaminated fraction would be useful.

For the measured particle concentration, less than 20% of the measurements were removed due to contamination each day (a new figure has been added to the revised manuscript). Only on three days during the measurement period, more than 20

Line 647:Increases in total number (e) and ccn (d) are pretty obvious on Feb 17. However increases in number in panels a and b are not obvious. Does this air mass change correlate with the onset of strong southerlies in Figure 5? May be worth noting if so. Also, on NASA Worldview It hink maybe I see your ship track in the MODIS Aqua imager approaching CapeAdare https://worldview.earthdata.nasa.gov/?v=258225.19165830087,-2145493.8406539974,501157.02538037306,-1958367.8679662866&p=antarctic&t=2018-02-17-T15%3A23%3A20Z

The air mass definitely changed upon our arrival to Cape Adare, but we should add some further context to this section as it is not just the air mass that is changing. In addition to a changing air mass, fog was also consistently present prior to Feb 17. The occurrence of fog can be inferred from the near 100% RH in Figure 7 from Feb 11 - 17. Cloud base height measurements with the ceilometer also confirmed the presence of fog. The presence of fog accentuated the differences in the number of CCN before and after Feb 17, since any CCN would have been activated within the fog leading up to the

17th. The presence/absence of fog is what is predominantly driving differences in the particle size distribution prior to and after Feb 17, with the changing air mass providing an additional influence.

We agree that it is hard to observe changes in the number of particles in the contour plots of Figures 16 a b since the contours are half an order of magnitude and there was some contamination from ship exhaust in the days immediately preceding Feb 17. However, we did see increases in the number of accumulation and sea spray mode particulate after Feb 17. Look specifically along the 100 nm and 1 um lines: there is a subtle but definite increase in the number of these particles after Feb 17. For a more convincing presentation, the difference in the number of accumulation mode particulate and CCN before and after Feb 17 is shown in Figure 1c of a manuscript submitted to JGR: Atmospheres, "Classification of the Below-Cloud Mixing State Over the Southern Ocean Using In-Situ and Remotely-Sensed Measurements" (https://www.essoar.org/doi/abs/10.1002/essoar.10502904.2). While most of the change to the total number of CCN was due to an increased availability of accumulation mode particulate, sea spray particles were also more abundant after Feb 17 due to the heavy southerlies. The correlation between the abundance of sea spray particles and wind history is further described in Hartery et al (2020).

Accompanying text has been added to the manuscript to highlight this.

Figure 14: Having titles or insets on these panels listing what each is would be very helpful.

We included the instrument name in the titles on each figure panel of Figure 14 (now Fig. 16).

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