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Interactive comment on "Underway seawater and atmospheric measurements of volatile organic compounds in the Southern Ocean" *by* Charel Wohl et al.

Anonymous Referee #2

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This manuscript describes surface ocean concentrations of DMS, isoprene, acetaldehyde, acetone, and methanol and their sea-to-air flux in the Southern Ocean in late summer, early fall. This manuscript is well written and shows an interesting dataset. Especially the discussion of the measurement system and its measurements and technical issues clearly convince the reader of a robust, well analyzed dataset.

Some specific minor questions/comments are: The authors mention that, to their knowledge, these are the first reported seawater measurements for methanol, acetone and acetaldehyde. Even for DMS and isoprene the Southern Ocean is highly undersampled which, for all 5 compounds, increases errors when running global at-

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Discussion paper

mospheric models and using no (or very sparse) data from the Southern Ocean. This fact highlights the importance of these measurements presented in the manuscript, which I think the authors are aware of. However, I suggest to even highlight this importance in the introduction section adding a paragraph about the Southern Ocean and its influence on the atmospheric chemistry, highlighting the importance of this work. 2.1.1: Calibrations. Do the authors have any scientific explanation why isoprene, the most insoluble compound within the 5 compounds presented, is the only compound not achieving fully equilibration using the presented setup? 2.2.3: The authors mention the light driven contamination in the seawater measurements of acetone, acetaldehyde and isoprene and I am confident that they solved this issue. However, for me it is not clear what exactly causes this issue. Perhaps the authors could state clearly if they think it is coming from contamination of the material exposed to high sunlight intensity or from photochemical production in the water flowing through the tube. Both facts seem reasonable, however, if it is the material shouldn't you see these variations also when measuring outside air? Did you experience similar issues in former cruises or tests? Figure 3: Data is shown for 2 weeks before and after dealing with this issue. First, the cruise started only one week before the issue was dealt with, which leads to second, do the two subsets of data have about the same number of measurements? Please check. Additionally, the authors state that daytime values prior to 04/03/19 were not used. However, it seems, that night time data shown in Figure 3 is consistently (for all three compounds) lower than data prior to 04/03/19 shown in Fig. 5a, 7a, and 8a. (i.e. Figure 3, acetone seawater night time values "2 weeks before": $\sim$6 nM; Figure 7, average acetone seawater values shown prior to 04/03/19: $\sim$7-9nM). Please check. l. 309 / Table 2: The authors mention the positive skewness (mean: 0.053, median: 0.045) in the isoprene ambient air mixing ratio which they explain by biology and wind driven emissions as well as the very short atmospheric lifetime. I totally agree. However, this skewness is not at all discussed in the DMS section although the skewness is way higher (mean: 2.6, median: 1.39). DMS has a longer atmospheric lifetime and is more soluble than isoprene.

Technical corrections: l. 11: delete “and” after “compounds” l. 27: missing full stop after “outgassing” l. 288: “As shown in Fig. 1 and Fig. 2 . . .” l. 292: “chlorophyll a” l. 441: “dependent” l. 452: remove “in” l. 518: add “,” after “isoprene” ll. 726-730: check reference typo Figure caption 1: double use of “data” Figure 2a: right y-axis: remove “(PSU)” Figure 2b: left y-axis “μg dm-3” Figure caption 5: “. . .and time series of chlorophyll a.”

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