

Interactive comment on “Shipborne measurements of ClNO₂ in the Mediterranean Sea and around the Arabian Peninsula during summer” by Philipp G. Eger et al.

Anonymous Referee #1

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Eger et al. present measurements of ClNO₂, HCl, SO₂, O₃, and NO₂ mixing ratios in the Eastern Mediterranean Sea around the Arabian Peninsula during summer 2017. The data inform about the conversion of N₂O₅ to ClNO₂ in this environment and are a welcome addition to the literature as there are few such measurements outside North America, China and Europe. The analysis is thorough and shows that the ClNO₂ production from NO₃ (epsilon) is small, which is not surprising considering the warm temperatures that shift the equilibrium away from N₂O₅ chemistry.

The data set is broken up into sections by region (Gulf of Oman, Gulf of Aden, Red Sea, etc.) and presented as (nocturnal) averages, median, and maxima (Table 1 + Figure 2).

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I felt that this wasn't the most accessible way to present the data and added confusion. Examples are the averaged nocturnal locations shown in Figs 2 and 5. It would have been more transparent to present a continuous trace of ship locations color-coded by CINO₂ mixing ratios (and split the figure up into part a = leg 1 and part b = leg 2). After all, averages can be skewed by plumes. I also felt that the data were over-interpreted since changes are interpreted as regional differences rather than temporal ones. Imo, it would have perhaps been more informative to stay put in one or two places for some time for that reason, but that was perhaps outside the control of authors.

Overall, the manuscript is suitable for ACP. However, the manuscript is on the long side and could (and perhaps should) be condensed. Some figures are missing axis labels (Latitude, longitude, day, month, etc.). There are also few typos in equations that need to be corrected. The date formatting does not adhere to ACP standards, e.g., 25 July 2007 (dd month yyyy), not 25.7.

Specific comments:

Page 2, reaction (6). There is also a minor channel producing NO₂.

Page 4 line 29. You would get two time series, one for m/z 208 and another for m/z 210. Please specify how you used two masses (added them, or averaged them) or did you chose one over the other? Note that you can get a IBr⁻ at 208.

Line 31 – here, you give one sensitivity. Shouldn't the sensitivity at m/z 210 be ~1/3 that of m/z 208?

Page 5- line 2. Again, how did you use two m/z values to get one mixing ratio?

Page 5. Please comment if the stack emissions truly interfered with CINO₂ measurement by CIMS, or if the data were filtered simply as a precaution.

Page 6 line 12 “modified” how? Was it equipped with a photolytic converter?

Page 6 lines 18-19. Meusel et al. 2016 state that J values were not corrected for

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up-welling UV radiation. This should also be stated here since it biases the J values low.

Page 9 equation (2). [O3] also changes over time.

Page 10 “Boundary layer height of 1000 m”. That seems high for the marine boundary layer. Is there evidence to corroborate such a high mixing height?

Page 11 equation (8). Please define Keq. Should it be capitalized?

Page 13 Please check equation 11 – does not look right.

Line 2 – “k” does not appear in equation 11; perhaps it should say “B” rather than “Bk”?

Line 10. 14+/-14% and throughout the paper. Since the % operator applies to both 14 and 14, please add brackets (14+/14)%. Otherwise, it reads as a percent error, i.e., is interpreted as 14+/-2. This is repeated throughout the paper (e.g., line 21, 13+/10% could be 13+/-1.3). Same goes for units when uncertainties are given (e.g., line 31, 25-35 °C)

Page 15 line 15 – punctuation error. Note that there are others like this throughout the paper.

Equation 15 is incorrect.

Page 19. Many references are missing doi's.

Figure 2 and Figure 3. Please label all axes for clarity.

Figure 4 A lot of the variability may be due to not having enough data. Consider longer averages (1 hr, 90 min, or 2 hr) for the ClNO2 data.

Figure 7. Are you sure if this analysis is valid? It is possible that changes in NOz and ClNO2 are due to shifting air mass.

Figure 8. Are these total chloride and total sodium concentrations, or from certain size fractions only? (state in caption)

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Figures 9 and 10. I think it's important to point out here that the authors only consider selected sources of radicals (ClNO₂ photolysis and O₁D+H₂O). Some important ones are omitted (such as Cl₂ and HONO photolysis and HO₂+NO).

Figure 10 I wouldn't lump HCl+OH→H₂O+Cl (a conversion of one radical to another) in with OH and Cl production from O₃ and ClNO₂ photolysis (which generate radicals from stable molecules).

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