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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Shipborne measurements of $ClNO_2$ in the Mediterranean Sea and around the Arabian Peninsula during summer" *by* Philipp G. Eger et al.

Anonymous Referee #2

Received and published: 6 August 2019

This paper (Shipborne measurements of ClNO2 in the Mediterranean Sea and around the Arabian Peninsula during summer) reports observations of ClNO2, NO3/N2O5, HCl, particle composition and other parameters made during a cruise in the Mediterranean Sea, Red Sea and Persian Gulf. This is a severely understudied region in terms of atmospheric chemistry and, as such, the dataset presented here fills a significant gap. The paper is well laid out, the figures and tables clear, and the analysis of the data is interesting and thorough. I only have a few minor observations, but other than that, I recommend publications on ACP.

Main Points

I find the analysis in Section 3.4 a bit confused. First of all, a little introduction explaining how the factors influencing ClNO2 production efficiency are going to be evaluated in this section would be useful in order to follow the discussion. Second, the values of f calculated with Eq 9 and with Eq 10 are significantly different, but this discrepancy is not really explained or discussed. It is also not clear if the value for the Gulf of Oman is 0.6 (page 11, line 29) or 0.84 (page 12, line 3).

When it comes to f, the main issue is the availability of particulate chloride. In general, it seems (page 12, lines 20-25) that the authors are focusing on fine particles, while I would expect sea salt to be a dominant source of chloride in the open sea. It may be true that the surface area of sea salt is smaller but the ClNO2 yield is higher, as the authors themselves acknowledge on page 13. Therefore neglecting sea salt in the calculation of f may not be appropriate and could possibly lead to a bias in the results of the analysis.

Finally the statement on page 13 line 30 about the importance of kdir, i.e. the direct NO3 loss, seems to be in contrast with the last lines of the section. I am afraid it is not enough to refer to a future publication, given that a significant part of the analysis stands on the assumption that the direct losses of NO3 dominate over the indirect losses. At least a summary of the steady state analysis mentioned here should be given to support the statements about kdir.

Minor Points

page 1, line 30: capitalize "Earth"

page 5, line 2: I am not sure I follow the ion chemistry from HCl to I(CN)Cl-. Where is the CN group coming from? Please provide more information or add the relevant reference.

page 5, line 7 and 12: can you provide more information on the purpose of the IMR bypass? And it is not clear to me how 50 cm of a 1/8 inch tube reduces the pressure

in a 3 m long inlet.

page 6, line 10: do you mean NO3?

page 10: can you specify which of the methods explained in the supplement is being used as default in the paper discussion and in Figure 6? I am guessing method B but it should be stated.

equation 8: I think you need to explain the keq[NO2] part of the equation and how it is related to [N2O5].

At several points in the paper the notation IClNO2- (or similar) is used for the masses measured by CIMS. But IClNO2 is a cluster not a molecule, so it should be more correctly indicated as I.ClNO2-. The same for other ions mentioned throughout the paper.

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