

Interactive comment on “NO₂ observations over the western Pacific and Indian Ocean by MAX-DOAS on *Kaiyo*, a Japanese research vessel” by H. Takashima et al.

Anonymous Referee #2

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The study by Takashima et al. reports on “NO₂ observations over the western Pacific and Indian Ocean by MAX-DOAS on *Kaiyo*, a Japanese research vessel.” In addition to the presentation of NO₂ data the paper reports on sensitivity studies for the NO₂ retrieval.

In general, I doubt that the topic of the paper fits into the scope of AMT. In this journal I expect either the description of new measurement and/or retrieval techniques, the comparison of different techniques or at least the description of new concepts for the interpretation/evaluation of existing techniques. All this is obviously not the case here:

C2751

Instrument and algorithms have been described in recent publications, e.g. Irie et al., 2011. All sensitivity studies do not contain any new information. The temperature dependency of the NO₂ cross section is well-known and has been addressed by several authors before (e.g. J. P. Burrows, A. Dehn, B. Deters, S. Himmelmann, A. Richter, S. Voigt, and J. Orphal. Atmospheric remote-sensing reference data from GOME: Part 1. Temperature-dependent absorption cross-sections of NO₂ in the 231–794 nm range. *J. Quant. Spectrosc. Rad. Transfer*, 60:1025–1031, 1998). That water vapour might be an issue in the DOAS fit in particular in regions with high humidity is also not very surprising. E.g. Van Daele et al., JGR, 2005 already discussed the possible impact of interfering species in that wavelength region. To minimize this effect participants of the most recent intercomparison campaign for UV/vis instruments in Cabauw, The Netherlands, agreed on a wavelength window of 425 to 490 nm for the analysis of NO₂. Why the authors did not choose this fitting window?

Further comments: As referee #1 I'm quite sceptical about figure 11, where the authors show the probability function of NO₂ concentrations in the boundary layer reporting a maximum at 0.1 ppb. How meaningful is that, when the detection limit of the system is in the same range? How the authors explain the huge diurnal variation of NO₂ e.g. on July 15, 2008? Dilution within the rising boundary layer? Emission peaks?

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C2752