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Atmospheric Measurement Techniques Discussions

Interactive comment on "NO_2 observations over the western Pacific and Indian Ocean by MAX-DOAS on *Kaiyo*, a Japanese research vessel" *by* H. Takashima et al.

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We would like to thank the reviewer for carefully assessing our paper. In this study, we developed an instrument for use on ocean vessels by employing an active-type gimbal. We performed the first MAX-DOAS measurements using a compact, low-power/low-cost instrument on a vessel. Because NO2 content is generally low over the remote ocean, and under such condition, H2O contributed to the poor DOAS fitting. To our knowledge, this has not been fully recognized in the literature (in urban areas, the 425–450 nm or 425–490 nm fitting windows are likely to be appropriate because NO2 DSCD

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is much higher than over the ocean; in fact, the 425–490 nm window was selected during the CINDI campaign). Following the reviewer's comment, we also conducted DOAS analysis using the 425–490 nm window, yielding similar results to those obtained with 425–450 nm.

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?

> As noted by the reviewer, a previous study described our instrument and analysis method for measurements on land [i.e., Irie et al., AMT, 2011], but not for measurements from an ocean vessel; consequently, information relevant to measurements from a vessel is presented in this paper. We agree with the reviewer's statement that previous studies have focused on the temperature dependency of the NO2 cross-section. However, these previous studies were based on satellite measurements rather than ground-based measurements.

One of the reasons for selecting the 460–490 nm window was that the difference between the representative wavelengths for NO2 and O4 can be very small, thereby minimizing the wavelength-dependence of air-mass factor information [Irie et al., AMT, 2011]. Following the reviewer's comment, we conducted the analysis by using the 425–

AMTD
4, C2931–C2935, 2012

490 nm fitting window, yielding similar results to those for 425–450 nm. That is, the correlation between NO2 DSCD for 425–490 and H2O DSCD was negative, and the NO2 DSCD was negative on average; however, the value of NO2 DSCD was slightly higher for the 425–490 nm window than for 425–450 nm. See Figure 1 below.

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?

> Please see the reply to the relevant comment by referee #1. Fig. 8 shows good agreement between the two analyses of 460–490 nm and 338–370 nm. At low NO2 concentrations, both analyses show maxima around $\sim$0.1 ppbv, with variability of $\sim$0.05–0.1 ppbv. We may say that the background level over the remote ocean for the 0–1 km layer is $\sim 0.1 \pm 0.1$ ppbv. The variability of $\sim$0.05–0.1 ppbv may correspond to the random error of NO2 concentration over the remote ocean. Because our original description of "detection limit" may have led to misunderstanding, we revised the relevant text.

How the authors explain the huge diurnal variation of NO2 e.g. on July 15, 2008? Dilution within the rising boundary layer? Emission peaks?

> Because Yokosuka is an urban site, we consider that such diurnal variation is reasonable; we compared the MAX-DOAS NO2 for 0–1 km with in situ direct measurements performed near our station, and found a strong correlation between the two. The concentration at the surface was almost double that of MAX-DOAS for 0–1 km, but this finding is also reasonable because the source of NO2 is located near the surface.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/4/C2931/2012/amtd-4-C2931-2012-

AMTD
4, C2931–C2935, 2012

Interactive Comment

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Interactive comment on Atmos. Meas. Tech. Discuss., 4, 6069, 2011.

AMTD

4, C2931–C2935, 2012

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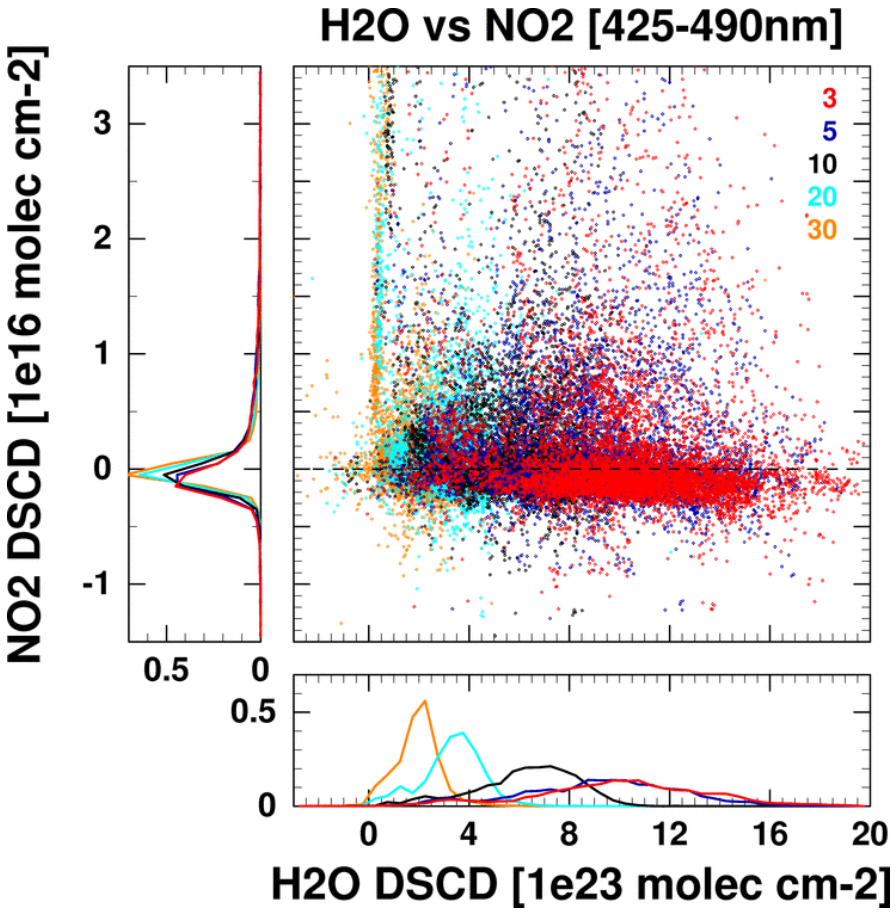

Fig. 1. As for Fig. 7, but for 425–490 nm.

AMTD
4, C2931–C2935, 2012