

## ***Interactive comment on “Measurements of total and tropospheric ozone from IASI: comparison with correlative satellite and ozonesonde observations” by A. Boynard et al.***

**Anonymous Referee #2**

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Review of “Measurements of total and tropospheric ozone from IASI: comparison with correlative satellite and ozonesonde observations” by Boynard et al.

General:

In this study, the authors compare the ozone data retrieved from the Infrared Atmospheric Sounding Interferometer (IASI) instrument with the ozonesonde and GOME-2 satellite observations. This is a key step for application of IASI ozone data and will have significant implication to ozone chemistry studies. The presentation of the paper is excellent. It is clear, precise, and mostly accurate. For the content, I have following comments. I recommend the paper to be published after the authors address these

C1524

comments.

1. The validation with ozonesonde observations can be conducted in a more meticulous way as we compare this paper with other papers that performed similar comparisons between satellite remote sensing of atmospheric species and sounding data or between satellite instruments. Examples include Nassar et al. (2008) and Dupuy et al. (2009) for ozone. Why do the authors use ozonesonde data from only 14 stations, not all the stations? Can the authors provide a more detailed comparison by region, by altitude, and by season? Can some intercomparison of ozone data also be made between IASI and other satellite instruments?
2. The title of the paper is “Measurement of total and tropospheric ozone from IASI”. In the paper, the troposphere is not explicitly defined. Instead, ozone values from the surface to 6 km or from the surface to 12 km is presented. It should be pointed out that the tropopause is not fixed and ozone from the surface to 12 km may include some stratospheric ozone, especially in high latitudes.
3. In Figure 5, the secondary ozone maximum at 11 km seems to be more than the primary ozone maximum higher up! It would be helpful to show a profile in the mixing ratio. This is an interesting case, only I am not totally convinced that the interpretation of this ozone peak is correct. The authors talk about a low pressure system. However, the potential vorticity (PV) would be a more convincing variable to examine for the stratospheric influence. Also, it would be good to look at the CO/H<sub>2</sub>O profile for this case since IASI gives that too. A low pressure system implies existence of clouds. How does this condition affect the quality of ozone retrieval?

Specific:

Page 2, Line 27: A reference or references are needed for the accuracy and vertical resolution. Also reference(s) are needed for Page 7, Line 21.

Page 5, Line 18-19: more explanations or some references are needed.

C1525

Page 7, Line 3: Can this internal report be cited as a reference?

Table 2: Add sample size to each case. More discussion can be given to Table 2. Why does IASI compare poorly with GOME-2 in northern high latitudes during winter?

Figure 3: Add latitude/longitude to the figure.

Figure 6a: This figure itself is good. However, an overview on behavior of the averaging kernels globally would be helpful to the reader, such as similar plots by latitude, longitude or by land cover types. The same comment applies to Figure 6b.

Figure 11: Add latitude/longitude of the station to the caption.

Technical corrections:

Table 1: Pression should be Pressure.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 10513, 2009.