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Atmospheric Chemistry and Physics Discussions

Interactive comment on "IASI measurements of tropospheric ozone over Chinese megacities: Beijing, Shanghai, and Hong Kong" *by* G. Dufour et al.

Anonymous Referee #2

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The paper presents tropospheric ozone distributions acquired by the IASI nadir sounder during 2008 in small areas above Chinese megacities. The ozone distributions are obtained by retrieving a vertical profile of O3 from the measurements, using forward modeling in the infrared and a Tikhonov-based retrieval method. Seasonal patterns of variations are obtained and discussed. The paper is clearly presented and written but has in my opinion too many limitations to be published in its current form.

The major problem I see is that the paper fails in one of its objective –at least as I understood it from the title, the abstract and the conclusion–, which is to demonstrate the capabilities of IASI to measure tropospheric ozone and in particular boundary layer

ozone pollution above megacities. The distributions above the cities are retrieved and seasonal variations are persuasive (the effect of monsoon in particular) but I couldn't see very convincing evidences that observed enhancements were actually induced by anthropogenic activities. There are several reasons for this:

1. The distributions are obtained in rather small areas around the megacities, which makes it difficult to unambiguously determine if the signal is originating from the city or from e.g. pollution transport in the free troposphere or even stratosphere / troposphere exchanges

2. Most of the variations follow those of a latitudinal climatology which is not really supporting human-induced ozone pollution. If one assumes that the effect of monsoon is indeed seen, what differences with the climatology would be left? An important question that follows is: Would similar seasonal patterns be observed in other places representative of the latitudes studied, such as Southern Europe or North America? I think this has to be addressed in order not to mislead the reader.

3. There are several retrieval aspects that are not tackled at all and that may have a strong influence on the retrievals and the seasonal variations:

a. First, the authors do not clearly say if they use IASI measurements from the morning or the evening orbit. From the introduction, one would assume both. It is very likely that the retrieval sensitivity will be different, especially in the lowest layers. Averaging both would probably hide some interesting patterns; for instance it is not clear what is actually used in Figures 12, 13 and 15.

b. The relation to ground temperature and thermal contrast is not addressed. Figure 2 shows averaging kernels for very favorable scenes: high surface temperatures (probably summer; it should be mentioned) and high positive thermal contrast. It is unlikely that these form the majority of the cases. In particular nighttime measurements (if used) will be characterized by much smaller values of thermal contrast, which will lead to a significant decrease of the vertical sensitivity; then maybe two partial columns

cannot be discriminated (again that should be better discussed). The same holds for wintertime measurements, which will have worse sensitivity also because of the lower surface temperature. Overall, without these important informations on the retrievals, one could argue that at least parts of the seasonal variations (spring-summer maxima) are reflecting differences in the measurements sensitivity. I insist that the authors include these elements in the paper.

c. An emissivity of one is used in the retrieval. This is obviously not the case and could lead to wrong estimates of surface temperature (and thermal contrast). That also comes in neglecting the downward flux. I am not sure this would have an impact for the regions shown here but it would be useful to know.

4. In several Figures, error bars need to be added to clarify what trends are significant. This is the case for the series of Figures 6,8,10 and 12,13,15 but also for Figure 14. Typical errors are given in section 2.2; but again these may vary from one observation to the other depending on local surface temperature, thermal contrast.

5. Except for the very qualitative comparison of the ozone distribution to population density, there are no supporting evidences of pollution episodes. Is there no way of getting information on surface ozone for several days relevant to the study? This would in particular be useful when discussing the daily variations and the individual vertical profiles. Were the days mentioned (e.g. April 16-17 in Beijing, 3-4 June in Shanghai.) particularly polluted in the boundary layer?

For the reasons exposed above, I was uncomfortable reading the paper with its focus on megacities. I recognize that the authors are sometimes very careful in presenting their results and mention the different possible causes of high ozone concentrations in the troposphere (transport from biomass burning plumes, stratospheric intrusions) but the title, abstract and conclusion as well as several sections of the manuscript sound optimistic with regard to air quality monitoring with IASI and some conclusions are in my opinion not well supported. I therefore encourage the authors to carefully check these

parts of the manuscript, which may be misleading unless further discussion and/or new elements demonstrate that photochemical pollution above the Chinese megacities is really monitored.

Specific comments

- Page 23106, last line: I don't think IASI has yet demonstrated "great potential" for air quality monitoring. "potential" is more reasonable.

- Page 23107, line 20: (Clerbaux 2007) should better be replaced with the more recent publication Clerbaux 2009, in the IASI ACP special issue.

- Page 23108, line 2: The values of radiometric noise are quite large. Is this non apodized? Other publications refer to a noise close or below 0.2 K in the ozone band.

- Page 23108, line 10: The spatial resolution of IASI is 12 km at nadir if the pixels are retrieved independently. Is that the case?

- Page 23108, line 17: There are other papers on IASI-ozone published in the IASI special issue that should be cited. Also the paper by George at al. on CO should be cited.

- Section 2.2. The retrieval grid should be stated.

- Page 23112, line 24: "shows up as" instead of "leaves place"?

- I found Figure5 and the discussion around it in the text hard to follow. Is this Figure necessary, in particular considering that Figures 6,8,10 show the evolution of the vmr at two altitudes?

- Page 23113, 23115, 23117 refers to the capability of IASI to discriminate two ozone partial columns in the troposphere. This sounds optimistic and a little contradictory (especially the text page 23117 from line 10 is contradictory). When looking at for instance Figure 2 it seems that the 3km level is well captured but that the information at 8km is originating from the lowest levels (peaking at 5 km and strongly overlapping the 3 km

kernel). Is Figure 6 representative in that respect? Furthermore this entire discussion has to be mitigated again by the specific observation conditions (temperature etc).

- Page 23116, seasonal variations above Shanghai: If it wasn't the point in July, to me the entire trend is more representative of the 30-40°N band.

- Figure 2: I assume the kernels are plotted for levels spaced by 1 km but this is not said. The fact that the black and red colors represent the kernel from 0-6 km and 6-tropopopause should be indicated as well.

Technical corrections:

- Reference Lin et al., 2009: "Richter"

- Figure 13: The title overlaps the day scale on the x-axis.

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