Atmos. Chem. Phys. Discuss., 9, C10672–C10680, 2010
www.atmos-chem-phys-discuss.net/9/C10672/2010/

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.

G. Dufour et al.

dufour@lisa.univ-paris12.fr

Received and published: 9 February 2010

The authors thank the referee for his/her interest in the article and his/her suggestions for improvements. The comments are addressed below.

General comment

"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."

Reply to the general comment:

Concerning the doubts of the referee on the capabilities of IASI to measure tropospheric ozone: several papers (Eremenko et al., 2008, Boynard et al., 2009 and Keim et al., 2009) compare ozone retrievals from different algorithms (not only the one used in this paper) with sondes data for all the seasons and show the performance of IASI to retrieve tropospheric ozone (0-6km columns). In this paper, the authors limited the study only to the 0-6 km columns that have been validated and do not claim that one can retrieved ozone in the boundary layer (that is mentioned nowhere in the text). The authors regret such a misunderstanding and efforts have been done in the revised version of the manuscript to cope with this problem. However, on can note that during large ozone pollution episode, the boundary layer height can be very high (more than 3 km) and reaches altitudes to which IR instruments are sensitive. In some cases, the 0-6km can then be strongly influenced by large ozone contained in the PBL. In more usual case, the sensitivity to the lowest layers is less but not zero, contribution from the PBL is then integrated in the 0-6 km columns. Moreover, the monsoon affects the lowest layers of the troposphere (below 3-4 km) and its effects is then integrated in the 0-6 km columns.

The authors would like also to point out that all along the text, they provided hypotheses for the enhancement of the observed ozone paying careful attention to the conclusions.

Note that the determination of the sources of the large ozone amounts (that would ask dedicated model studies) was not the aim of the paper and is not fully addressed in the revised version of the paper. Nevertheless, additional information from external sources (meteorological analyses, backtrajectories, pollution indicators, etc) is provided to discuss and support the different hypotheses in the revised version of the manuscript.

Comment #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"

Reply #1

The comments of the referee on the size of the processed domains are right. The external information added in the revised version (see previous answer to the referee's comments) helps to address this issue. In the future extended regions will certainly be considered.

Comment #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."

Reply #2

To answer this comment backtrajectory calculations have been performed. They allow us to evaluate which 10° latitude band is the most relevant for the comparison in Fig. 4 and only the relevant latitude band has been kept in the figure. Especially, the Beijing area is mostly influenced by air masses from the 40-50°N, the Shanghai area by air masses from 20-30°N and the Hong Kong area by air masses from both 10-20°N and 20-30°N. The fact that the ozone amounts over Beijing, Shanghai and Hong Kong are larger than the climatology is now more highlighted. This enhancement occurs only during late spring for Beijing and during the second maximum for Shanghai and Hong

Kong.

Comparisons with other cities (for example Madrid and Istanbul) have started and show that broader summer maxima are observed in these European regions. Since the results are preliminary (and behind the scope of the current paper) and since the paper refers the reader to several studies discussing about the seasonal cycles over other cities in the world, especially in Europe we don't think reasonable to add them in the paper.

Comment #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."

Reply #3

3a – The authors apologize. They forgot to precise that only the morning overpasses of IASI have been used for this study. This has been added in the revised version of the manuscript.

3b – The authors agree that the discussion about the performance of the retrieval was not deeply conducted in the ACPD version of the paper. They thank the referee for his/her suggestion. A more detailed discussion has been added in the revised version of the manuscript with examples of averaging kernels measured in unfavourable cases (low surface temperature and thermal contrast), especially during winter. The discussion about the possibility to discriminate 2 partial columns in the troposphere has been extended and we hope makes clearer the performances and limitations of IASI in retrieving tropospheric ozone to the reader. This extended discussion also leads us to modify Figs. 6, 8 and 10 in order to present only partial columns and not individual levels (that are less meaningful due to the poor vertical resolution of IASI).

3c – The authors agree that neglecting the emissivity can be an issue. However, in the 3 regions considered here, they checked with the UW/CIMSS Global IR land Surface Emissvity Database (http://cimss.ssec.wisc.edu/iremis) that the emissivity variations in the different domains were small and homogeneous in a given domain. The authors are aware that the surface temperature retrieved assuming an emissivity equal to the unity is wrong. In this case one can only talk about effective surface temperature and thermal contrast. In the description of the data processing, this is written like this. Note that during the retrievals, the effective surface temperature is retrieved first but that during the ozone retrievals the baseline of the spectrum is finely adjusted for each

microwindow.

Comment #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."

Reply #4

Due to computational time, the error budget, that requires several derivative calculations, was not explicitly calculated for all the pixels analysed. However, estimations of the errors have been performed for different conditions, typical of each month. The errors bars are added when necessary.

Comment #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?"

Reply #5

As mentioned previously, external information has been added in the revised version of the paper. Especially discussion about the meteorological situation is now done with wind fields shown and helps to support the hypothesis on the tropospheric origin of the large ozone amounts observed. As far as we know, no surface ozone measurements are publicly distributed for the different regions. However, the ministry of the environment of China publish every day an Air Pollution Index (API) for the different Chinese cities. This index is a combination of 5 pollutants including O3, CO, NO2, SO2 and

PM10. If the index is larger than 100, the air quality becomes harmful for human beings. We show now in the revised paper that the API in Beijing was larger than 100 for the period discussed in the paper (April). This information has been added in the revised version of the manuscript.

Comment #6

"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."

Reply #6

As already mentioned, external information (winds, backtrajectories, API, etc) have been added to help to support the different hypotheses made. Moreover, an extended discussion concerning the performances of the retrieval has been added. The authors hope these additions give more credit to the data presented. Some sentences maybe too optimistic or somewhat misleading have been modified or suppressed.

Specific comments

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

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

"- 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." This was a typographic mistake. It has been corrected.

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

"- 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." Only one example was given per molecule but all the papers treating about O3 and CO in the IASI special issue have been added in the reference.

"- Section 2.2. The retrieval grid should be stated." This information has been added. The profiles are retrieved on a 1 km grid.

"- Page 23112, line 24: "shows up as" instead of "leaves place"?" This has been corrected. "- 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?" As already mentioned, the authors have chosen to modify Fig. 6, 8, 10 to present partial columns and Fig. 5 has been suppressed.

"- 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)." With the extended discussion about the performances of the retrieval, the different sections have been revised. The discrimination of 2 partial columns is now treated more carefully and better explained (differences between winter and summer 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.” Backtrajectory calculations (that are added as supplementary material) show that the air masses which affect Shanghai from May to October come essentially from the south, from the 20-30° latitude band. Now the comparison is done only with this latitude band where the differences are significant.

"- 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." The retrieval grid is spaced by 1 km and the averaging kernels are presented every km. This has been added.

The technical corrections proposed by the referee have been taken into account.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23103, 2009.