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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Retrieving tropospheric nitrogen dioxide over China from the Ozone Monitoring Instrument: effects of aerosols, surface reflectance anisotropy and vertical profile of nitrogen dioxide" *by* J.-T. Lin et al.

Anonymous Referee #2

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In this manuscript, Lin e t al. present sensitivity studies on retrievals of tropospheric NO2 from OMI observations over 3 MAX-DOAS sites in China. They present a new retrieval set-up which improves on several aspects of the air mass factor calculations used in the operational DOMINO product, evaluate the sensitivity with respect to assumptions made on surface reflectance, aerosols, and NO2 profile and compare their results to validation measurements from the three MAX-DOAS instruments.

The paper reports on an interesting study with relevance for satellite data retrievals

used in many atmospheric studies. From the content, it would have fitted much better to AMT instead of ACP (I would have recommended moving the paper to AMT if asked before publication in ACPD). I'm also not convinced that the special issue on Asian Emissions is the right place to have this paper but this decision is up to the editors of that special issue.

My first reaction to this paper was enthusiasm, as it promised to address a number of important questions in satellite NO2 retrievals which have often been mentioned but never fully been evaluated. This includes surface reflectance impact, aerosols over China, and most importantly the interplay between aerosols and clouds in the retrieval process. I think the right questions have been asked in this manuscript and an important step has been taken to get a better understanding of these processes. Unfortunately, the paper turned out to be much less useful than expected for three reasons:

1. The data used is limited to the three MAX-DOAS stations as they are used for validation. What is a strength of the paper (validation with independent data) turns out to also be a serious weakness, as the relevance of the results found for the 30 independent measurement points over 3 polluted locations in China for all the other OMI observations is limited.

2. The reference algorithm produces NO2 columns showing impressive correlation to the ground-based data but there is a factor of 2 difference in the absolute values. None of the (realistic) sensitivity tests performed makes a large change to either the good correlation or the poor agreement in absolute values with the exception of the attempt to reproduce the DOMINO retrieval. My interpretation of these results is that either the parameters tested all have quite limited impact on the retrievals or that the data set used is not well suited to test the sensitivities of the retrieval. My guess is that the second possibility applies.

3. The presentation of the results is confusing in many places, focusing on corre-

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lations of the results from modified retrievals to the reference case (I do not see what the reader can learn from this number) and failing to addressing the obvious questions. For example, I haven't been able to find out

- what the main reason for the better correlation of the base case with MAX-DOAS is compared to DOMINO (my suspicion is, that there is simply something wrong in the DOMINO product),
- which of the computational expensive steps of the base retrieval are important and which can be omitted in a future operational system, or
- whether or not the assumption made in DOMINO (cloud retrieval compensates for much of the aerosol impact) is reasonable.

Considering these issues and the detailed points listed below, I think that the authors should make a real effort to improve on the description of the study and their results as well as the figures and to focus on the relevant conclusions before re-submitting the manuscript.

Specific Points:

1. MAX-DOAS data. It is very important for the interpretation of the results to know how the data points used are distributed with season. Please include a table of number of data points per month.

2. All of the effects treated here (improved spatial resolution, surface reflectance, aerosols) have been discussed in the literature before, and in particular retrievals using WRF-chem, CMAQ or nested GEOS-chem for high resolution a priori NO2 profiles have been applied in several previous studies. It is therefore important to point out where your retrieval improves not only on DOMINO but also on what is in the literature.

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3. The “improved AMF formulation” doesn’t look new to me at all – I understand that AMFs are calculated without LUT which allows to include the effects of BRDF and aerosols in a detailed way. The reason why that’s currently not being done in the operational processors is a) computational limitations when not limiting the analysis to 127 points and b) lack of reliable global input data, for example for aerosol properties.

4. The aerosol description is not really clear – are phase functions also taken from the model and if so how? – how are values transferred to other wavelengths? – how exactly was the scaling of model aerosol values done and how large were the factors applied?

5. Why can the DOMINO retrievals not be reproduced better? I would be concerned if the use of a LUT would be the main reason and would in general expect better agreement. Please comment.

6. The discussion of geometric AMFs is an interesting one, pointing at an intrinsic problem of this study which tries to evaluate different retrieval settings by looking at the correlation with MAX-DOAS data. If – as the authors say – the majority of the variability resulting in the good correlation is achieved even when using a geometric AMF then this is not a good data set for the sensitivity study! In addition, it would be very interesting to see one more comparison, namely the one between satellite SC and MAX-DOAS VC (the geometric approximation is good for NO2 in the stratosphere but not so good for NO2 in the BL where a constant AMF is more appropriate). It would also be interesting to know the slope for these two simple comparisons.

7. Discussion of difference in absolute values between base retrieval and MAX-DOAS: In my opinion, this is a weak point - either the MAX-DOAS data are representative for the satellite data, then I would expect better agreement. Or they are not representative, then their use as a standard for decision on the quality of

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the retrieval is questionable. Relying on relative changes (correlation) helps but only if changes in NO2 are dominated by large scale features.

8. Figure 4: It would be good to have the same x and y-axis in all three figures in a line for better comparability

9. Figs 6, 7, 12: it is difficult to read these figures. Please add points to make orientation easier and if possible replace case numbers by more meaningful labels such as “reference”, “no aerosol”, etc. If that’s too difficult, at least add a legend giving the translation from case number to scenario for each figure

10. Fig. 6: I think a table would be more appropriate to display these results

11. Fig. 7: As mentioned above, I do not see what I can learn from the R^2 of the NO2 column of a special scenario with the NO2 column from your reference case.

12. Fig. 9: How is “change in xxx” defined – is that relative changes or absolute changes?

13. Fig. 11: Again: What are “changes in xxx”? And why are changes in columns and AMF so different – I thought that VC = SC / AMF and thus any change in AMF leads to a corresponding inverse proportional change in VC?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 21203, 2013.