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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Highly resolved global distribution of tropospheric NO_2 using GOME narrow swath mode data" *by* S. Beirle et al.

Anonymous Referee #2

Received and published: 13 April 2004

The paper by Beirle et al. describes an investigation of special high-resolution GOME pixels with respect to the derivation of tropospheric NO2 vertical columns. The paper is of interest for trace gas retrieval from GOME-like sensors and shows the impact of the spatial resolution if global and regional maps of the NO2 distribtion are derived.

A scientific paper usually identifies open questions and tries to answer them in the article. However, I have difficulties to retrieve the open questions that have been answered by this study. It is obvious (and interesting and worth) to analyse the NSM data but the presented results and the conclusions are not convincing.

I have the following more specific comments:

a) It is trivial that a higher spatial resolution shows more details but what can be learned from the results ?

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b) an assessment of results suffers from unknown effects (impact of clouds)

c) the description of the retrieval method is too short, even taking into account that the method is published elsewhere.

d) it remains unclear who might benefit in which way from these results

e) the results concerning isolated spots (e.g. Istanbul, Mexico) are not convincing since the NSM GOME pixels are still too large to resolve the extent of a city plume.

In order to sharpen the profile you may highlight scientific work such as the seasonal correction and the analysis of the smoothing effect.

A more general remark is to open the view for development outside the local science community, e.g. in your reference list. Try to remove the grey literature (Ph.D. thesis, diploma thesis) wherever possible and replace it by corresponding articles.

Statements should be more focused and more precise (see several detailed comments below). I therefore recommend publication after a major revision of the paper, especially with regard to the scientific goal of the study.

editorial:

As an editorial comment I recommend to give the paper to a native English speaker before publication. There are many phrases where typical German expressions were just translated into English regardless an existing and corresponding idiom in English.

Don't use "much more" but replace it by just "more"

Detailed Comments:

abstract:

p 1666, l4: ...are derived by estimating the stratospheric fraction from measurements over the remote ocean.

replace e.g. by: are derived by estimating the stratospheric fraction from measure-

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ments over the remote ocean and subsequent subtraction from the total column density.

p 1666, l12:...allows to construct a much more detailed picture of the global NO2 distribution, especially if corrected for seasonal effects

The more detailed global picture of NO2 is an effect of the resolution and certainly independent from the season or seasonal effects. I think, I understand what you want to say but you are combining the higher spatial resolution with another correction which has obviously nothing to do with the resolution. Just start another sentence and decouple the statements.

The entire last paragraph of the abstract is somewhat unclear. What you want to say is the following: You can calculate differences of NO2 VCDs derived from large pixels and smaller pixels. Then you will notice a difference which can be further analysed. A quantitative analysis of these differences (are you still comparing apples and apples ?) can be used to study the smearing effect. This is important for e.g. budget studies but it is not per se important for SCIAMACHY and other sensors. There, you will only have the smaller pixels and the GOME results help to interpret but do not influence the SCIAMACHY/GOME-2/OMI retrieval, or do they ? It tells you what you can expect from sensors with better resolution but it has to my knowledge no impact on the retrieval method.

Introduction:

p 1668, l16: These studies clearly showed, that...

Change the relation. It is misleading to speak about different sources of NOx because the reader now expects a description of source processes followed by an explanation of chemical transformations. Instead, you may just say that you are able to identify industrial regions of the world, which is actually a trivial statement. This is already known from standard GOME data. So a more precise formulation of this paragraph is

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required.

p 1668, l19-20: ...is insuffcient to resolve details of the regional NO2 distribution or individual cities.

replace e.g. by:...is insuffcient to draw a detailed picture of the NO2 distribution on the regional scale.

chapter 2 Retrieval:

section 2.1:

p 1668, l6 remove "...much more..."

p 1668, l8 "...of the total column ()."

Add at least a brief description how this is done. Tell the reader about the main (hidden) assumptions !

p 1668, l9 ...due to the degradation of the GOME instrument or the diffuser plate.

Is the diffuser plate not part of the instrument ? Too sloppy. Be more precise in your statements. The entire instrument ages but the diffuser is presumably the most affected part of the instrument and this will have consequences for the retrieval. Studies about the degradation of GOME have been already published (Tanzi, C.P., et al. Performance Degradation of GOME Polarization Monitoring, Adv. Space Res. 23, 1393-1396, 2000).

p 1668, l11 and thus will not affect...

and will therefore not affect...

p 1668, l15ff

Where is the reference sector precisely ? You don't say anything about possible yearly cycles, so the reader has to know in advance that

a) your sector is presumably in the tropics

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b) that NO2 does not have a noticeable seasonal cycle there.

Then you may conclude that you may estimate the trop. fraction by subtracting the strat. amount from the total amount. Otherwise, it would be impossible to do that ! Just give more information on what you are doing. A more precise explanation helps a lot to understand the method.

...towards trop. gases

replace by: with respect to trop. gases.

Are measurements of stratospheric species not affected by the degradation ? Again too sloppy.

p 1668, l19ff

Your correction factor falls from heaven. It is just 2 and nobody knows why. What is the variance of correction factors derived by others ? Why is 2 then a representative value ? Is there a seasonal dependency, a latitudinal dependency ?

Again, the given explanation of the retrieval method needs to be enhanced and more details must be provided. Also the discussion of cloud effects is too short. You say you don't tackle the cloud problem because it is not fully understood. In other words, since clouds are present almost everywhere, your paper is questionable in general because the impact of clouds is not known.

The way out is either

a) to say that you concentrate on cloud-free scenes (or scenes with a cloudiness below a certain threshold) or don't you ? I guess you do because the trop. NO2 below clouds cannot be seen by GOME and all your hot spots are then not visible.

b) to discuss the calculation of VCDs in case of partially cloudy scene here rather than in chapter 5 where the reader is actually surprised to be confronted with a basic discussion how the VCD is calculated. This needs to be done earlier. I therefore

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suggest to add chapter 5 to chapter 2 which is (too) short anyway.

Move section 2.2 up to section 2.1 and vice versa. The technical details about GOME could even be in a chapter of its own, e.g. "2 GOME instrument". Or you may omit section 2.2 here but referring to existing papers where GOME is described in detail, which is acceptable, as long as instrumental details are not vital for your retrieval.

section 2.2

p 1669, first paragraph.

an angular range of plus/minus 8.7 deg is applied...

might be replaced by: Three days a month (7/8 a.s.o) GOME is operated in the narrow scan mode (240 km swath width) that corresponds to a scan angle of plus/minus 8.7 deg and a ground pixel size of 80 x40 kmˆ2. This scan mode is embedded between the sun calibration timeline of the previous and the following day.

Last paragraph:

p 1669, l 12ff

...to reach a global cover...

replace by:...to provide global coverage...

...around the globe.

replace by: Figure 2 shows the spatial distribution of all NSM measurements during the 5 years...on the globe.

section 2.3

p 1669, l 16ff

Regions of industrial activity show...

Significantly enhanced (or higher) VCDs are observed in areas with dense population

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and/or high industrialisation.

...small sources...

replace by: ...weak sources... or ...small targets with comparably low total emission...

Second paragraph:

Your statement about "many more details" in Fig. 3b with respect to Fig. 3a is actually unsustainable. It is simply the limited space for images which makes it difficult to see the advantages of NSM pixels. Both images look very similar (even 3c, which is not mentioned here) and all feature in 3a are in 3b,c but there aren't prominent features in 3b,c which are not in 3a. So these images are not really appropriate to underline your statements. Fig. 5 however provides detailed regional information. So it might be better to use regional images rather than global images to show the reader the advantages of the NSM data.

Why is the scatter of results in the SAA higher for smaller pixels (Fig. 3a versus Fig 3c) ?

You mention biomass burning areas but the given example (Fig 4) is not convincing. I actually expected spots over Central Africa, Brazil, probably also Indonesia but the scatter over Brazil makes it difficult to distinguish between effects from uncorrected level 1 data and "true" biomass burning effects. In your plots (Fig 3a-c, Fig 4) NO2 values over Africa around and below the equator are generally high but including the Namib desert as well. Fig 4 shows a BBE but the example suffers from enhanced NO2 values of the same order over the Atlantic (southwards from your points A and B). It is therefore difficult to assess the results.

Your problem is the limited lifetime of trop. NO2 which makes it difficult to show single events in a 5 years mean image. There are certainly areas where biomass burning is ongoing for weeks and in fact you show this with Fig. 4 but the question remains whether your plots are adequate for the effect to be shown. Why not concentrate on

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events where such a spot is measured in NSM on one day and SSM on the next or the day before ? This could be more impressive and convincing since you don't have the problem of the limited lifetime of trop. NO2 (provided that the source strength and the meteorological conditions are not varying too much from one day to the other).

p1670, l13: Seasonal variations should not be influencing a multi-year average.

replace by: Seasonal variations should not influence a multi-year average.

p1670, l20ff: Your explanation on how the seasonal effect is finally corrected is again very short. It is no luxury to make use of equations. In general, the paper will benefit from at least some basic formulae, also in the chapter about the retrieval method.

P 1671, chapter 3

Again I cannot follow your statement that Fig 3c is providing that more information than Fig. 3a. Colours are changing from orange to light yellow (which is of course an increase or decrease in total columns) but at least on your global images there aren't new spots. Why not using only Fig 5 for underpinning the "new insight" of trop. NO2 ? You may even add more examples.

p 1672, chapter 4

...rich in contrast...

replace by: ...show details...

...tracks directly crossing the source see...

Just say

...is not visible in the next tracks close by.

One may also argue (without a chance to proof it here) that the larger east-west extension can also be due to the prevailing wind direction in this area which is more zonal than meridional. It is however not very likely that the plume extends to 80 km in east-

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west direction in reality and disappers then. A smaller area is very likely but cannot be confirmed by the GOME data. So it is indeed the resolution which is responsible for that result.

p 1673f section 4.2

Your quantitative analysis of SSW and NSM pixels is interesting and may indeed be used for a further evaluation of the NO2 regional or global distribution. Your discussion of Fig 7 could be more elaborated since it provides the impact of small versus large pixels on retrieved NO2 columns.

It does however not contain information about the impact of the "true" NO2 profile shape that is appropriate for larger and smaller ground pixels. Unfortunately, you don't provide any information about the NO2 profile shape and how it is taken into account in the retrieval. I therefore (have to) guess that the same profile shape is assumed for small-scale and large-scale pixels. This is an acceptable first order assumption (e.g. for routinely processed data) but it compromises a quantitative discussion of NO2 VCDs over "hot spots". In fact, you would have to use a profile with higher NO2 loading in the PBL for smaller pixels and vice versa, in order to reflect the enhanced loading of NO2 where it is produced (and its limited lifetime). So I'm coming back to my remark from the beginning: Please give more details about the retrieval in section 2.1 or discuss it here.

Milan is the largest city in the Po valley but your statement about its responsibility for high NO2 values in the entire Po Valley is in contradiction with your previous estimation about the NO2 lifetime. Assuming 17 hours lifetime and having a wind speed of about 1 m/s (which is too low in fact) transports NO2 only ˜60 km away from Milan. The Po Valley is simply Italians industrial heart and industry and especially traffic (and also private combustion in the winter season) are the main sources of NO2 in the densely populated region. Torino and Padua are certainly other spots but with respect to the NO2 burden the Po Valley is more like the German Ruhr bassin and the bordering

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Netherlands.

Torino is obviously responsible for the overestimation of NO2 above the Western Alps (not Milan).

Mexico is certainly a spot with an east-west extent $<$ 80 km but this is again a trivial result since it is just driven by the pixel resolution of GOME.

p 1674, chapter 5

Due to the high cloud albedo, this effect is nonlinear:

What you presumably had in mind to say is about the nonlinear contribution of cloudy/cloud-free subpixels to the total reflected intensity. This is always non-linear provided that the albedo differs for both scenes, but the effect becomes more prominent if one (subpixel) scene is brighter than the other one. Move the equation below Fig. 8 to here and it becomes more clear.

HICRU database: Nobody outside the German community (probably even not outside your local science community) will

a) know this Diploma thesis (presumably in German)

b) may easily have access to it.

Please provide more information about the database and remove this reference. Even more, the given numbers of the fractional cover do not differ that much. What is the standard deviation of the cloud fractional cover (CFC) values and is it a statistically significant difference ?

GOME products are provided with its own cloud product with known limitations. Why is this product not used and what are the differences of the CFC derived from the ESA GOME product and the HICRU cloud product ?

There is another GOME cloud algorithm available from KNMI (FRESCO) which is ac-

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cepted by the international community. Even the cloud data is available for interested users. Why not to make use of this cloud product for your study ? Is it possible to underpin your results even with other CFC results ? This should be at least discussed since the impact of clouds is left open here.

Unfortunately, you end this chapter with the open question about the impact of clouds which in fact needs to be answered. Your results clearly suffer from the unknown cloud effect and I therefore cannot see the large impact of your work on the interpretation of other sensor data. The problem remains and the statement that a better spatial resolution gives a sharpened view of the global NO2 distribtion is trivial because it is obvious.

p 1675, Conclusion

See again general comments at the beginning. In fact, the quasi-linear relation between NO2 VCDs derived from forward and backscan pixels is surprising but remains unexplained. Generally enhanced NO2 values above clouds over polluted areas seem to be unrealistic, even if vertical transport from the PBL to upper levels in cumulus-like clouds and lightning is taken into account. This result needs to be analysed further before it can be accepted as a comprehensible research result.

Fig 8: Remove parts of the description (equation, conclusions) from here and add it to the text.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 1665, 2004.