

Interactive comment on “An improved NO₂ retrieval for the GOME-2 satellite instrument” by A. Richter et al.

Anonymous Referee #2

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The manuscript presents improvements of the NO₂ algorithm for GOME-2. It focuses primarily on the DOAS fitting part of the algorithm. Claimed improvements are the extension of the fit window and a spike removal procedure. Results are shown on a “soil signature” and the higher sensitivity for ship tracks.

The manuscript should be improved on several aspects before acceptance. The part on the “soil signal” is not convincing. In the part on the spike removal mathematically questionable methods are used, where other procedures are available. In other places of the manuscript substantially more information should be provided to the reader. Main and smaller comments are given below.

Main Comments.

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The status of the algorithm improvements is unclear for the reader. In some parts of the manuscript they are presented as algorithm improvements, whereas the first sentence of the conclusions presents it as a new product. In the discussion on the solar spectrum it is concluded that it is up to the user if a fixed or daily solar spectrum is used, indicating that no clear choices have been made. The authors should state clearly from the beginning and consistently in the manuscript if they are presenting a new product or an improvement to a (part of the) algorithm. If it is a new it should be clear how it relates to other products and where the data is made available for users. Also if this is a new product a clear description on the air mass factor calculations shall be included.

The part on the “soil feature” is not convincing and cannot be published in the current way. I conclude that the soil feature in fact is absorption by O₄. The presented map is very similar to maps of the O₄ column showing highest values for reflective low altitude surfaces. The main reasons that the “soil feature” is not convincing:

-The “soil signal” presented in figure 6 shows very strong correlation with the O₄ absorption in this wavelength region. Note also that an outdated O₄ reference spectrum by Greenblatt is used in the fit.

-The GOME-2 “soil signal” shown in Figure 7 shows no signal over the Tibetan plateau, which does show up in albedo maps and visible imagery as a high reflectance region with a lot of bare soil. The high altitude of this region is the cause for low columns of O₄ and thus for the absence of the signal in Figure 7.

-I tried to find the soil feature in several of the relevant spectra in the Aster database (<http://speclib.jpl.nasa.gov/>) but I could not find any.

The discussion on the fit window selection is incomplete. It describes that several reference spectra have been added to the fit. However, according to Table 1 also the order of the polynomial was reduced from fourth to second order, for reasons not discussed. For new reference spectra it should be shown that the resulting fit parameters give

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meaningful physical results and that the correlation between fit parameters is reasonable to give independent results. In addition, other aspects regarding the length of the fitting window shall be discussed in the manuscript:

-Broader fit windows have larger error due to co-alignment errors (the fact that different wavelengths do not see exactly the same area on ground)

-For broader fit windows the assumption in the AMF calculation that the albedo is independent of wavelength becomes increasingly inaccurate.

-For broader fit windows the assumption of a wavelength independent effective cloud fraction (or radiance cloud fraction) becomes increasingly inaccurate due the variation in Rayleigh scattering.

The spike removal procedure is an ad-hoc method that is mathematically questionable. In the manuscript the authors also conclude that this procedure can bias the data noting that "the spike removal approach always needs to be carefully monitored to avoid biasing the data" (page 224, line 14). (Note that no information is given how the careful monitoring is done in practice). There are other procedures that should be applied to remove the spikes before the data is adjusted to the model, which is what the proposed method does. The duration of the spikes in the SAA is very short, meaning that for one detector pixel they are only present in one measurement and not in the previous or the next. Therefore procedures have been developed that use the variation in time for the detector pixels to filter out these spikes. Such procedures have been successfully applied to other satellite instruments. This method should be used also for GOME-2 (preferably in the Level0-1B processing step), before using the ad-hoc method described in the paper. The authors should investigate the application of the time variation procedure for GOME-2 and present results in the manuscript.

The authors describe investigations using a fixed or daily solar spectrum in their analyses. As they clearly show an impact of the choice, they do not indicate what the probable causes in the solar spectra are. For example are these caused by spectral

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features from the diffuser that interfere with the absorption cross sections, or the fact that the random noise in the solar spectrum turns into systematic errors when it used to normalize all the radiance measurements? Even a speculation of the authors would be valuable.

In the description of the daily vs fixed solar spectrum it seems that everything calls for a fixed spectrum, except for the long-term stability of the instrument. A logical solution would be to use a running mean solar spectrum (e.g. a month or longer), to benefit from more than daily measurements, but to capture the long-term variations. The authors should address this in the manuscript and show results why this will or will not work.

Smaller comments

The focus of the manuscript is on the NO₂ slant column fit. Therefore the authors shall present and discuss relevant details of the fitting procedure, including the fit function.

The OMI NO₂ retrieval algorithms have used a wide fit window since launch for the same reason as it is now introduced for GOME-2. The authors should make a reference to the relevant papers in section 3.

In section 5 the new GOME-2 data are compared to SCIAMACHY and the conclusion is that they compare very well. However, the fitting procedures between the new GOME-2 and SCIAMACHY algorithms differ, due to the presented presented. This contradicts to the approach presented on page 217, line 8. In addition, no information is provided on the air mass factor part of the retrieval. Given the fact that different fit windows are used, the surface albedo's should also differ. How do these differences affect the comparison?

The final statement of section 5 is very weak. The conclusion seems to be that the two approaches (daily or fixed solar spectrum) are both non-ideal. Instead of this statement it should be concluded that more effort is needed to come to better solution.

According to the text figure 6 is the natural logarithm of the ratio of the spectra (p221,

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line 16). In the caption of Figure 6 this is not mentioned. Which one is right, the text or the caption?

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 213, 2011.