

Review of the manuscript “Retrieval of nitric oxide in the mesosphere and lower thermosphere with SCIAMACHY” submitted to Atmos. Meas. Tech.

The authors present an approach to retrieve nitrogen oxide number densities in the mesosphere and lower thermosphere from limb measurements of the SCIAMACHY instrument on the ENVISAT satellite. First results of their retrieval are presented and compared to retrievals from the MIPAS instrument on the same platform. The authors chose a two-dimensional retrieval approach based on the optimal estimation method, which is innovative. NO in this altitude range had not been retrieved previously from SCIAMACHY and the initial results presented here show promise for interesting science that will be enabled by these data. The submitted manuscript certainly fits within the scope of AMT, and should be publishable after two general comments and a few more detailed comments of mine have been taken into account.

General comments:

- This paper presents a retrieval approach and its application to SCIAMACHY limb measurements. As such, there is surprisingly little detail on the actual algorithm that was developed and adapted to handle this problem. For a technical paper that is to be published in a technical journal, more details on these procedures should be given. For example, in the radiative transfer section, the authors state that “absorption is taken into account by adjusting the emissivity”, without explaining to the reader how this was done. The retrieval section outlines basic principles of the optimal estimation method but falls short with explanations on the a priori profiles and their co-variances or the regularization matrices and their effects on the result. More details on these inputs are necessary to make this paper publishable, and I will give some suggestions in my more detailed comments.
- The authors chose a two-dimensional retrieval approach which gives NO concentrations vs. altitude and latitude. This is innovative as it has not been applied to many experiments that provide sequences of limb measurements. The material provided in the manuscript suggests that mesospheric limb measurements were performed every 6°-7° in latitude. This corresponds to a horizontal distance of the order of 700 km between individual limb measurements. If we assume that the radiance observed by the instrument originates from a 300-400 km long stretch along the line of sight around the tangent point, this suggests that the individual limb measurements should be nearly independent of each other. In that light, it is unclear to me why the authors chose the 2D approach over a simple 1D approach that retrieves individual atmospheric profiles assuming spherical symmetry. This choice is not motivated by the authors but it should be. Because the 2D retrieval is an innovative approach, it also should be quantified what differences (if any) are introduced by the 2D approach compared to a spherically symmetric 1D approach. This would be valuable information not only to retrieval specialists but also to future users of these data. I assume that the authors have performed test retrievals without the latitudinal dependence and its regularization, so a comparison of 1D and 2D outputs for a few example profiles should not be too much effort.

Detailed comments:

Title: Retrievals are not done “with” SCIAMACHY, so I’d suggest something like “Retrieval of nitric oxide in the mesosphere and lower thermosphere from SCIAMACHY limb measurements”, which would also be more precise.

Page 6312, abstract, line 10: ‘compatible’ – ‘comparable’ ?

Page 6312, abstract: I’d also consider including a sentence concerning the comparison with the model and the differences found as it may provide a motivation for future scientific study.

Page 3612, lines 24-25: Sentence structure is somewhat confusing. I’d suggest ‘The SCIAMACHY measurements quantify ... as they are the only measurements...’

Page 3613, line 3 onwards: This paragraph should be shortened. There is a lot of description that’s not relevant. For example, lines 3-8 (until ‘...in phase B’) could be completely removed. Although descopes are always painful, it is sufficient to describe the capabilities as they are in relation to the presented work.

Page 3615, line 17: Remove ‘first’, replace ‘real’ with ‘useful’

Page 3616, line 10: Gamma bands don’t have advantages or disadvantages by themselves, they might have them when used for retrieval, so I’d suggest to add ‘for retrieval’ after ‘...disadvantages’.

Page 3616, line 12: Remove ‘having’

Page 3617, lines 1-7: This would be one of the places where more detail would be adequate. What is the meaning of $N_j - N_j/N_0$ seems to be the population ratio of state j vs. the ground state but this should be stated. Also, ω is given as a dependence of $v'v''$, not as $j'j''$ as it describes a single scattering albedo for a vibrational transition, according to the reference given by the authors. A little more detail would give everybody more confidence that this has been dealt with correctly. As this is a technical paper, I also think that the actual values used for these calculations should be given in a table to give readers a better chance to follow what has been done and build future work on it.

Page 3617, line 24-page 3618, line 4: This is a long and convoluted sentence. I’d suggest to split it up in 2-3 sentences to make it clearer what the authors are trying to communicate.

Page 3618, lines 19-24: This is another place where more detail is necessary. The authors state that the line of sight is optically thin. However, the definition of optically thin is somewhat arbitrary and does not necessarily imply that self-absorption is negligible. This should be quantified. In addition, stating that the ‘ozone and air absorption ... is taken into account by adjusting the emissivity’ is not sufficient. It should be explained in detail what has been done and how the authors arrived at their result.

Page 3619, lines 4-7: I’d remove the explanation of an overdetermined system, this is boilerplate from optimal estimation theory and not relevant to the problem at hand.

Page 3619, line 12 onwards: More detail is needed here, too. How does a typical a priori profile and its co-variances look like? The authors should give an example, maybe Fig.4 would be a good place for it. How do the regularization matrices look like, and what is their effect on the retrieval? Do they introduce a smoothing constraint, like in the operational Mipas algorithm? Why is this needed for the altitude dimension if an a priori profile is used as well? Scharringhausen (2008a) dealt with combined limb/nadir measurements so the problem described in his work is quite different from the pure limb measurements described here and the reference to his work is not sufficient. These questions tie into my two general comments

from above, and the authors should present in detail what the effects of their approaches and constraints are and what motivated their choice.

Page 3620, line 6: Why is the model use questionable below 100 km? This should be explained and/or a reference given.

Page 3620, line 11: Do the authors mean ‘...using the ... model ...’, which has as input parameters...’?

Page 6322, line 2: I wouldn’t call it ‘quite constant’ if the FWHM jumps around between 7 and 12 km. Is this related to the sampling along the orbit? The authors might want to elaborate on this a little, and state what averaging they used to obtain the green line.

Sections 4.3 and 4.4: I’d suggest to swap these two sections so that the comparison with Mipas is presented before the time series. I think this would improve the flow of the paper.

Page 6322, line 10: I find the agreement actually pretty lousy so maybe the authors want to remove this statement, especially as they elaborate on the disagreements in 2008 and 2009 later in the same paragraph.

Page 6322, line 20 onwards: I agree with the statement that direct comparisons with Mipas are desirable. As both instruments retrieve profiles that overlap in a considerable altitude range, I was surprised that the paper only compares averages of vertical columns. Direct comparisons of a few individual profiles of coincident measurements should be presented. I was also surprised about the choice of 70-140 km for the partial column. The reference Bermejo-Pantaleon (2011) shows Mipas profiles between 100-170 km altitude, so I was wondering whether a comparison over, say, 100-150 km would be more suitable. If there is a reason to choose 70 km as a starting altitude, the authors should state it in the paper.

Page 6323, lines 9-14: I don’t understand the meaning of this paragraph. Does ‘compatible’ mean comparable? Does the ‘standard deviation in the bin means’ mean the standard error? I suggest rewriting this paragraph using shorter sentences to make it understandable.

Page 6323, line 22: Remove ‘and’ before ‘using...’

Page 6323, line 26: ‘compatible’ – ‘comparable’ ?

Page 6324, lines 5-6: I suggest mentioning the comparison with the model and pointing out the differences found. This would provide a nice motivation for future scientific study.

Page 6324, lines 10-12: I’m not sure I understand this sentence, does it refer to a new instrument or a new measurement sequence (which would be obsolete now that Envisat is lost)? Does the orbit-to-orbit variation suggest the need for information at different azimuths, what I assume is meant by longitudinal resolution? The paper in the present form shows results only from one orbit and otherwise averages, so this would have to be fleshed out, otherwise this statement should be removed.