Reply to comment by B. Funke et al. on "Origin of the January–April 2004 increase in stratospheric NO 2 observed in the northern polar latitudes"

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[1] We understand that our hypothesis of the NOx production by ''low'' energy electrons (< 1 Mev) in the lower part of the layer-D of the ionosphere is highly speculative. But our paper [*Renard et al.*, 2006] must be placed in the context of the studies at the end of 2005. The NO_2 enhancements were observed in fall 2003 (Halloween event) and in spring 2004; since no measurements were analyzed in-between, one could postulate that the 2004 enhancement was a persistence of the Halloween event or was coming from a mesospheric descent. It was then necessary to better document the stratospheric NO_2 during the whole polar Arctic winter. At that time, only a first version of MIPAS data and some sparse GOMOS data were available on the web. What was done was to analyze the sudden increase of January 2004. This increase was particular since it occurred at high altitudes, around 65 km (in the lower mesosphere, and not in the upper stratosphere as said by *Funke et al.* [2007]). For the first time accurate satellite instruments (MIPAS and GOMOS) can be used for the detection of NO_2 in the lower mesosphere. It is known that now new MIPAS data are available, but at the time of our paper, only preliminary MIPAS data were available, and were not yet validated. So it was obvious that we could not give an accurate estimation of the amount of NO_2 at each altitude.

[2] It is difficult to reply to the criticisms concerning our work without publishing new materials. We have continued the analysis of NO_2 in the 2003–2004 polar winter using new processed GOMOS data that are now available. We will publish soon how such GOMOS data can be used, and what can be found among them.

[3] Many already published analyses on the origin of the enhancement(s) are based on climatology that are built by averaging out data obtained at different longitudes. Some local strong enhancements could be drowned in such mapping, thus leading to underestimate the maximum value of the enhancement(s). We will discuss this in the upcoming paper.

[4] We agree that it is commonly assumed that increased NOx in the higher mesosphere would be in form of NO, subsequently converted to NO_2 in the lower mesosphere during mesospheric descent. Nevertheless, this assumption needs to be confirmed by accurate simultaneous observations of NO and NO_2 in the mesosphere and in the higher stratosphere after strong solar flares and/or during auroral events. Also, such measurements need to show that the altitude at which the conversion starts is consistent with the altitude where we have detected the NO_2 enhancements in January 2004 (around 65 km). We have now some accurate NO_2 measurements from GOMOS, but to our knowledge, there are no accurate polar NO measurements at such altitudes. When such necessary measurements will be available and will confirm the theoretical predictions, we will accept that what we have proposed is wrong. On other words, even a highly speculative hypothesis cannot be rejected if there are no strong experimental evidences against it.

[5] Also, one problem could occur with the Funke et al. hypothesis on the auroral NOx production downward transported by mesospheric descent. The altitude decrease of the NO_2 enhancement at the end of January is very abrupt and cannot probably be simply regarded as a descent within to a strong polar vortex. While the polar stratosphere is cold in mid-January, the exceptionally cold and strong vortex is rather found in February and March. Then another effect must be invoked, like a mesospheric gravity-wave breaking that could induced a rapid descent of the auroral NOx.

[6] Nevertheless, during the winter 2003–2004, there is no direct evidence from satellite or ground based measurements that the NOx enhancements were produced in the upper mesosphere around 90 km and stayed in this layer three months, from November 2003 to January 2004. Also, there are some problems in the Funke et al. comments concerning the estimation of the total amount of NO. The authors speak about 100 ppb of NO observed by HALOE in the mesosphere only in January 2004 at mid-latitudes, and state that ''there was sufficient mesospheric NO available to produce the observed NO_2 enhancement after its descent to the upper stratosphere. . .'' Nevertheless, we will show with the GOMOS data that the NO_2 enhancements can reach 1 ppmv in the vortex during the mesospheric descent. So, at least 1 ppmv of NO in the upper mesosphere would be necessary to explain such measurements. Even if the NO content is higher in the polar region than at mid-latitudes, its estimation from HALOE data is far from what would be expected in the upper mesosphere. So no conclusion can be derived from the HALOE data of NO. This problem raises the question about the origin of NOx in the higher mesosphere.

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Could auroral sources be able to produce 1 ppmv of NO? It seems that this must be studied in details (using theoretical calculations?) by the authors that defend the auroral production hypothesis.

[7] In our paper, we have presented the ground-based SAOZ data because they were the only other measurements available at this date to confirm the NO_2 enhancement in January 2004. We agree that SAOZ does not measure NO, but it is the same problems with other instruments, so this argument given by Funke et al. is meaningless. We agree that SAOZ measurements are not representative of the polar cap NO_2 distribution and that there is a lack in the data between the 10 January and the 20 January 2004. What we said is that SAOZ has not shown a strong increase in NO_2 total content before 23 January, nothing more. It is obvious that if SAOZ hade shown an enhancement before this date, our hypothesis would have been ruled out. Then the analysis of SAOZ measurements does not give a strong proof of the origin of the NO_2 enhancement, but it is not in contradiction with our speculative hypothesis.

[8] Finally, we agree that some doubts could exist concerning the date of the beginning of the NO_2 enhancement in January 2004. Perhaps MIPAS have observed significant amounts of NO_2 in the lower mesosphere in mid-January, but they are smaller than those detected at the end of January. Also, a careful analysis of the data must be conducted, since the conversion of concentrations to mixing ratios could give artificial enhancements just because of the low signal to noise ratio of the measurements at these altitudes. We will confirm in the upcoming paper that GOMOS data show unambiguously a strong increase of NO_2 content just after the January 21 and 22 electron events.

[9] In conclusion, further analysis of satellite data will be necessary to better document the origin of the enhancements of NOx in the mesosphere and stratosphere during winter 2003–2004 after the "Halloween event". They could allow us to see which hypothesis (local production or auroral production) must be kept.

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