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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lifetime and production rate of NO_x in the upper stratosphere and lower mesosphere in the polar spring/summer after the solar proton event in October–November 2003" *by* F. Friederich et al.

Anonymous Referee #1

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General comments

The manuscript describes the calculation of the lifetime and effective production rate of NOx using MISPAS measurements of the NOx decay after October-November 2003 solar proton event (SPE). The authors also tried to estimate the contribution of the transport and chemical processes to the NOx decay and compare their results with other attempts. The subject of the manuscript is relevant to the ACP scope and potentially interesting for the community, because it could provide a way to validate a simple parametrization of the N production by precipitating energetic particles applied in the

most of CCMs. However, there are several issues in the manuscript (see below) and I have to suggest moderate revisions before publication.

Specific comments

Section 3.1: The authors mentioned that "vertical advection is small in polar summer", but is this really the case? First of all October-November is not the summer yet, polar night jet can still exist. Even during the summer the polar mesopause is very cold due to enhanced upwelling caused by gravity waves. I guess, this suggestion should be better supported and explained. The authors also excluded "...any important NOx production during..." This assumption should be also explained, because the reaction N2O+O(1D)=NO+NO can be important during the late spring and summer. The calculated life time is separated into transport and photolytic. However, the photolytic life time was not properly introduced and it is not clear what processes are behind this term. It is not described how this quantity is calculated using SLIMCAT model and how accurate this calculations are. The role of reaction R8 is not discussed. This reaction is not related to transport and photolytic (because N(4S) and NO are the products of ionization by particles), so the authors should explain why R8 is neglected. The authors conclude that the transport play a major role in the decay of SPE generated NOx almost everywhere except 73 deg. south between 50 and 55 km. It is interesting feature, but the authors do not even try to explain what could be the reason for the absence of transport processes there. I think it would be interesting to analyze the decay of NOx integrated over entire southern high latitudes. This analysis would exclude the local transport and could show how good the calculation of photolytic (whatever it means) NOx removal is.

Section 3.2: This section is a little bit difficult to read because the explanations are too short. For example the authors say "n(IPP=0, t0) can be determined by means of a polynomial function, fitted to the MIPAS data of the Austral summer 2003/2004", however the analyzed period starts in October and the line in the Figure 3 starts 150 days before 1 January. Does it mean that the summer data were extrapolated to winter

time? The text says “the color code is time dependent”, but there is no explanations at all how to read these colors. The discussion of two branches is hard to follow, I have problem trying to identify which branch has observable gradient. The discussion about another noticeable discrepancy after 20 November is also difficult to understand, because it is too short. The same can be said about the Figure 3 (right) and the explanation how this data were calculated. I do not really understand Equations 4 and 5. In particular, these equations are almost identical (except the coefficient before the integral), but why in the Eq.4 the values depends only on t0, while in eq.5 it is a function of IPP, tau and t0. It confuses a bit when you try to understand Figure 3 (right). I would be very appreciated if this part is explained with more details.

Section 4: Section 4.1 is not instructive at all. It is not clear what the authors would like to convey. It can be easily moved to introductions. In section 4.2 the authors try to compare (I guess) different things: the effective NOx production rate from Fig.4 calculated from the observed decay of NOx after SPE taking into account all processes in the atmosphere and the coefficient of N and NO production associated with the formation of ion pair. I am not sure that this comparison is well justified. In the subsection 4.3 I suggest to introduce better the applied box model. Otherwise, the reader will have to read long Funke et al. (2011) paper where this box model is introduced (in my opinion even in this paper the box model was not properly described). In particular, how the transport is treated in the box model (the authors showed earlier that the transport processes play a major role in NOy decay). It is interesting to note that the results obtained by the authors substantially disagree with the Funke et al. (2011) results for day time conditions. Dashed black curve from Fig.13 of Funke et al. (2011) looks similar to green symbols in Figure 4 and differs from black line. The authors do not try to explain the possible reasons for such a difference.

Conclusions: The conclusions are even shorter the abstract! The authors concluded that “The calculated NOx-production rates do not reproduce the theoretical value of 1.25 . . .”. This conclusion is obvious (as the authors mentioned in the earlier text) and

I do not think it is hopefully not the main conclusion of the paper. The discussion about the comparison with Funke et al. (2011) is vague and not instructive. The discussion about possible overestimation of the IPP by AIMOS model is interesting, but again too short. What is missing here is some discussion/outlook of how the models can be better validated using presented MIPAS data analysis and how to find a way to confirm simple parametrization used in most of CCMs.

Minor comments and technical corrections: 1. Page 17709, line 7: it should be “there” instead of “they”? 2. Page 17714, lines 1-4: Please reformulate. It reads like the electrons are measured at the altitudes around 50 km.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 17703, 2012.