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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 #2

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Friederich et al. present a study of the Halloween 2003 solar proton event (SPE) using data of the MIPAS/Envisat instrument from two Southern Hemisphere latitude bands (63S and 73S). They study the NOx changes and describe a method that can be used to calculate NOx lifetimes and "effective" production rates by particle precipitation. The results show that the after-SPE NOx loss is mostly due to dynamical processes while the photochemical loss is significant but has, in most cases, a smaller role. Increasing from 45 to 65 km, the NOx lifetime varies between 100 and 2 days, and smaller variations with respect to latitude also exist. Considering the estimated NOx lifetimes, the effective NOx production rate (scaled with the ionization rate) is shown to vary between about 0.2 and 1, the smallest values seen in the lower altitudes.

The paper is not acceptable for publication as it is now. It may become acceptable if a major revision is done by the authors. The paper is quite short, the abstract and conclusions are poor and do not give enough useful information, the methods and data are not explained and justified with necessary details, and the results are not discussed or explained properly. The authors' attempt to compare their results with previously published results has little meaning. The paper looks unfinished.

However, despite all the shortcomings, the scope of the paper is suitable for ACP and the results could be quite interesting if better presented. Below are a number of detailed comments, which the authors should carefully consider before submitting the paper for another evaluation.

DETAILED COMMENTS:

Abstract, line 2, altitude-dependent instead of altitude dependent

Abstract, line 9 and in other places, –63S refers to Southern Hemisphere with both - and S. Use either - or S (better use the latter).

Page 17705, line 2. Ionization takes place also below 40 km, especially during solar proton events (SPE). However, even during large SPEs ionization rates do not increase much below ~30 km, because at those altitudes there is always ionization caused by galactic cosmic rays which dominates.

Page 17705, line 5. Why focus on altitudes 42-62 km only? I think the answer is that this is the altitude region which is affected by protons AND is covered by MIPAS observations. It would be good to mention the reason. Also, in the introduction the present work and the reasoning behind the study and the approach taken is usually described briefly in the last paragraph. I suggest that the authors do the same, i.e. the "we focus" part could be moved there with added explanations.

Page 17705, line 10, whole paragraph. Are all these reaction details really needed here? Are they used later in the paper? In addition to Porter (1976), there is a paper by Rusch et al. (1981) which concludes a similar number N-atoms per ion pair. Other studies have given a range of possible numbers, see e.g. discussion in Baumgaertner et al. (2010). It would be good to mention these studies, although 1.25 is the generally accepted number, as it would put the results of this paper in wider context.

Page 17706, paragraph starting from line 5. The chemical lifetime of NOx is a few days in sunlit conditions, at night or during polar winter the lifetime is more like months. Some justification should be given: why was this study made and what are the objectives?

Section 2.1. Some more information on the MIPAS data could be given. As I understand it, NO is not one of the standard MIPAS products and is not provided by ESA. Therefore, it would be interesting for the reader to have more information on the NO product. I suppose NO data are available for limited time periods only? How accurate are the NO data in the stratosphere, there is a lot of NO in the 110 km region which would be in the line-of-sight of every measurement? I do not understand the last paragraph. I assume that the authors are taking zonal averages at selected latitude bands. The last sentence needs an explanation: why is the AVD diagonal element an important criterion? For clarity, please use AVK and AVD instead of avk and avd. About calculating the averages: the authors should give some more information. For example, number of data points, standard deviation/error (the error is shown in the figures, but it would be good to also discuss these briefly here).

Section 2.2, line 5. I think the authors can simply remove the IPP here (later it is useful as abbreviation). The ionization rate units are cm-3 s-1.

Fig 1. The authors could add another panel showing the observations and the fit for another altitude, say 45 km. This would help to demonstrate the differences between altitudes.

Page 17709, line 5. X**2 should be explained much better, now it is not clear what it

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exactly is and how it should be interpreted. I assume that X**2 is the residual, but the authors do not say this!

Page 17709, from line 25 on. The result here is that in general the dynamical lifetime is shorter than the photochemical one, i.e. transport and mixing explain most of the observed NOx behavior. Is this a typical situation? The authors could give some more details on the dynamical conditions. Especially, they should explain the longer dynamical lifetimes at 73S, 50-55 km. Is this related to the orientation of the polar vortex (I know that it might be already gone by October).

Page 17710, line 11-19. This text could be already in the introduction, as a part of a paragraph briefly describing this study.

IPP (ion pair production) is defined many times, only do it once when it is first used.

Section 3.2 needs to be rewritten in order to make it more readable and understandable. Clearly not enough details are given. For example, I do not understand how Eq. 4 can be used for the whole time series of 250 days (as shown in Fig. 3). Surely the NOx lifetimes (photochemical, dynamical, and total) will change considerably within the 8-month time period. The authors then fit a line to all the data points (in Fig. 3r), which means that most of the points have little IPP or corresponding NOx production. Would it not be more appropriate to use a smaller set of points from the SPE period? These issues should be carefully discussed and the approach taken should be justified. I(IPP,tau,t0) given in Eq. 5 should be explained better, how to interpret it and how it will vary with time. Why it's useful to plot the difference of NOx with respect to I(IPP,tau,t0)?

Page 17711, from line 20 on, related to the previous comment. If I(IPP,tau,t0) increases but NOx does not, it could also indicate that the NOx lifetime is shorter than estimated. On November 20, if the NOx lifetime was longer than estimated a month before, it would lead to a behavior similar to that seen in Fig. 3. These possibilities should be discussed.

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Fig 3. Standard error of the mean is not visible. If this is because they are so small, then remove them from the plot and give a typical number in the caption or text.

Page 17712, line 10. "Most of the X**2 values are significantly larger than one and so they argue for a non-linear NOx-production." This statement is mystifying, it does not tell anything to the reader. Larger X**2 values mean a poorer fit, right? Line 12, "This is obvious,...". It is obvious, because 1.25 only considers production, while the authors consider also chemical and dynamical loss (thus "effective" NOx production rate).

Fig. 4. The authors have shown (Fig. 2) that the NOx lifetime decreases with increasing altitude. That should mean that their effective NOx production rate should also decrease with altitude. However, in Fig. 4 the authors are showing the opposite: the effective NOx production rate is lower at lower altitudes. It is quite difficult to understand what is going on here. Another figure like Fig. 3, but at 45 km, should be shown. The authors do have a possible explanation, too high ionization rates, but this is only mentioned in the abstract and conclusions, while a real discussion on this matter is missing. It seems to me that the ionization rates should be a factor of 5 (3) too high to explain 0.2 at 45 km (0.3 at 50 km). Does this result agree with the earlier studies?

Section 4 has very little meaning. The authors try to compare their results, NOx lifetime (NOxLT) and "effective" production rate due to particle precipitation (EPR), with previous studies. However, they do not compare anything with Jackman (2005) results, they simply state Jackman's results. A comparison would not be possible anyway, because Jackman's study did not consider NOxLT or EPR. A comparison with Baumgaertner (2010) is possible, because they also presented EPR (but only considering effective production of N and NO, and not, e.g., dynamical losses). However, the authors mostly describe what Baumgaertner did and then take care of the comparison with one sentence. They do not discuss any of the possible reasons behind the differences. Funke (2011) also presented EPR (but did not consider dynamical loss), which is shown to decrease with increasing altitude (above 45 km). In the current paper, the authors show an opposite altitude behavior (see the previous comment), but do not bother to

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properly discuss the possible reasons. This section needs to be completely rewritten.

The authors give a brief and vague conclusion for their study. What is the reason this study was made? What are the questions to be answered? Do the results have any meaning, e.g., for atmospheric modeling? Where is the improvement? Should we change the current parameterization of NOx production? The results are not properly discussed in context.

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