

Interactive comment on
**“Mesosphere-to-stratosphere descent of odd
nitrogen in February–March 2009 after sudden
stratospheric warming” by S.-M. Salmi et al.**

Anonymous Referee #1

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General comments:

The authors have studied the effect of the odd nitrogen (NO_x) produced by energetic particle precipitation (EPP) on the stratospheric NO_x and ozone in the Northern Hemisphere winter and early spring under two different types of dynamic conditions (i.e. during 2007 and 2009). They compared the descent pattern of NO_x using a chemistry transport model and ACE-FTS observations. They have concluded that the chemical loss of NO_x was insignificant for both winters and the NO_x descended to the lower altitudes was largely controlled by the dynamics. The ozone loss in the stratosphere (30–50 km) is not related to the NO_x descent, but is due to activation of the halogen

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chemistry.

In general, this paper reads well and the results contribute to clear certain confusion that currently exists in the community. I see however a number of weak points which should be adjusted and rewritten before I can recommend publication. In particular the value of this work within the community and the motivation for this study needs to be highlighted more clearly and carefully. My specific comments are given below.

Major comments:

1) The introduction provides some necessary back ground knowledge about the NO_x and its production and descent. However, there is lack of logic and clear statement on what has motivated the author to do their research here, especially given that they seem to reach the same conclusion as Randall et al. (2009).

2) Fig. 1 shows that the NO_x production rate at the source region (75-85 km) was actually higher in 2007 than in 2009, while the amount of NO_x descended to 65 km was the other way around, more in 2009 than in 2007. The authors suggested that it was mostly due to a difference in dynamic conditions between those two years. This comes to an important point whether or not the descent EPP-NO_x to the stratosphere and its in-situ photochemistry reaction with stratospheric ozone play a detectable role on the stratosphere dynamics. GCM studies have suggested that EPP-NO_x effects on ozone at low latitudes may be comparable to the effects of solar UV radiation (Callis et al., 2000; 2001; Langematz et al., 2005; Rozanov et al., 2005) but question remains in terms of the exact mechanism that has caused the temperature and wind changes in the stratosphere or in the troposphere. It has also been suggested recently that the EPP-NO_x caused ozone loss can lead detectable change in stratospheric NAM and dynamics and its effect may reach the surface temperature and pressure through downward transport (Baumgaertner et al., 2011). Instead, Arnold and Robinson (2001) suggested that there may be a dynamic link between geomagnetic Ap induced ionization in the thermosphere which leads to changes in stratospheric wind and tempera-

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ture through a change in planetary wave activity. Based on the ERA-40 reanalysis and ECMWF Operational data, Lu et al. (2008) investigated EPP-NO_x influences on NH polar stratospheric temperature and zonal wind in spring, during which NO_x-ozone photochemistry supposes to be stronger than other seasons. They showed that the temperature and wind variations in relation to the changes of geomagnetic Ap index have a sign that is opposite to that expected from the NO_x-ozone photochemistry mechanism. They therefore concluded that the changes observed in stratospheric zonal wind and temperatures were unlikely to be caused by in-situ EPP-NO_x and ozone interaction. However, as their results showed that the temperature and wind responses to geomagnetic signals are consistent in both northern and southern hemispheres, they speculated the stratospheric signals were more likely to be caused by indirect, dynamic processes. Randall et al. (2009) studied the different NO_x descending pattern during 2003/2004 and 2006/2007, and concluded that the EPP-NO_x descending was largely driven by dynamics; it was particularly true for 2006 winter. It seems that the results here are in line with the conclusion of Arnold and Robinson (2001), Lu et al. (2008) as well as that of Randall (2009). I think that it is important to bring this point out. Some careful discussion is also needed as it helps not only the authors to state their motivation and present their key results better but also helps the community to clear the confusion.

3) The authors stated that “At the same time we can test the quality of ECMWF operational analysis at higher altitudes” (the last paragraph, page 4). However, there is no other observational data to test against the ECMWF used in this paper. If the results of Manney et al. (2009) are the benchmark that the authors used to compare with, say so in the Section 2.1.

4) The second paragraph of Section 2.1 gives the readers an impression that the ECMWF operational data is not the right data set to use here as it compares poorly with Manney et al. (2009) at the pressure levels (i.e. 50-80 km) where the descending of the EPP-NO_x took place. So what is the reason to use the Operational data then?

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In addition, the part of the text starting with “It has, however, ...” should be in the result section as the authors have stated that one of their objectives is to test the quality of ECMWF operational analysis at higher altitudes.

5) I recommend combining fig. 2 and fig.3 into a single figure. So are figs. 4 and 5, so that section 3.1 can be written more concisely. In general, the paper needs to be more focused on results related to the descent of NO_x and its effect on the stratospheric ozone rather than comparing the dynamic condition of 2009 winter and spring to that of 2007.

6) The results from FinRose model revealed that the relative chemical loss is only 3% and the ozone loss or increase in the stratosphere has little to do with the descending EPP-NO_x, even during the year with a strong SSW (i.e. 2009). This is very interesting. Given the 2009 SSW event was one of the strongest events on record (Manney et al. 2009) and according to dynamics, stronger than usual downward movement of the polar air is expected just after the SSW. This further adds support onto the comment #2 above. Indeed, studies have shown that the most significant events of NO_x descent in the NH winter and early spring occurred just after a major SSW (e.g. Randall 2009; Siskind et al. 2007 and this paper). It would be expected that stronger effect of EPP-NO_x on the stratospheric and surface temperature during the SSW years than during the non-SSW years. However, both Lu et al. (2008) and Seppälä et al. (2009) have demonstrated that the stratospheric and tropospheric responses to geomagnetic Ap index were actually enhanced when the SSW-years were excluded from their analyses. Some discussion is needed here to relate the results of this paper to the previous papers. The specific questions which need to be addressed are: How does these results compare with the strong ozone loss in the stratosphere reported by Baumgaertner et al. (2011) and other chemistry-dynamic coupled models (e.g. Rozanov et al. 2005; Baumgaertner et al. 2009; Callis et al., 2000; 2001; Langematz et al., 2005)? Can the difference be explained by the lack of two-way coupling between the chemistry and dynamics in FinRose model, or it is simply because the ECMWF Operational data at

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higher altitude are not so reliable?

7) The paper needs to make it clearer how the observed NO_x based on ACE-FTS observations at 10 grid points were interpreted spatially at the upper boundary of FinRose model.

8) It may also be helpful if the authors can estimate and discuss the difference of the amount of descent EPP-NO_x and its loss if slight different temporal or spatial interpolations of the ACE-FTS observations are used to define the upper boundary condition of FinRose model. It is expected that the difference would depend on how variable the daily NO_x are in space and time. Though I understand that the ACE-FTS observations are not best suited for FinRose model and what has been done by the authors is probably the best they can do. Nevertheless, it is more informative if the uncertainty range can be provided and discussed.

Minor comments:

1. NO_x is not defined in the first place where is used, see the third line of Abstract.
2. VLF needs to spell out in full when it is first used in the paper.
3. Line 18, Page 9, “normalized quickly”. Rephrase it as “normalize” is normally used as a mathematical term.
4. Line 1, page 10, “ had only a slight effect on the model results”. Please be more specific on the “slight effect”, e.g. reduce or increase the NO_x by what amount etc.
5. Line 19-20, page 10. “ It stands out that there are only about 10 measurements per day to observe the northern polar area”. This sentence should be in section 2.2, not here.
6. The first paragraph of section 3.3, page 11. The text is not clear in terms of which year the ozone reduction was observed and modeled.

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