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Atmospheric Chemistry and Physics Discussions

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

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Response to the referees

We thank the Referees for their instructive comments and the interest towards this paper. Below you can find the answers for the Referee comments. The abstract, introduction and conclusion sections were modified to emphasize our motivation to do this study. For the same reason we added a new section, Discussion, to the paper. After a more detailed analysis of the model results,

we have also revised the discussion (in section 3.3) of the ozone loss in mid-February–early March. In addition to these, only some minor changes have been made in the text without changing the actual point.

Response to Anonymous Referee #1

Major comments

Comment 1. *The introduction provides some necessary back ground knowledge about the NOx 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).*

Answer 1. To make the motivation of this study more clear we have now discussed the papers mentioned by the Referee in comment 2 in section 1 (Introduction).

Comment 2. *Fig. 1 shows that the NOx production rate at the source region (75-85 km) was actually higher in 2007 than in 2009, while the amount of NOx 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-NOx 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-NOx 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-NOx 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 temperature through a change in planetary wave activity. Based on the ERA-40 reanalysis and ECWMF Operational data, Lu et al. (2008) investigated EPP-NOx influences on NH polar stratospheric temperature and zonal wind in spring, during which NOx-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 NOx-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-NOx and ozone interaction. However, as their results showed that the temperature and wind responses to geomagnetic signals are consistenst 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 NOx descending pattern during 2003/2004 and 2006/2007, and concluded that the EPP-NOx 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.

Answer 2. We now discuss the papers mentioned by the Referee in section 1 (Introduction) and also in section 4 (Discussion). However, we want to emphasize that NO_x was not transported low enough into the stratosphere to affect the ozone balance although the descent event in early 2009 was one of the strongest observed. Therefore, based on our study we can conclude that for EPP-NO_x to significantly influence the stratospheric ozone the descent needs to last longer than it did (e.g. in 2004 when the SSW occurred already in December) or local NO_x production has to take place.

Comment 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.*

Answer 3. We used Manney at al. (2009) as a point of comparison for the ECMWF operational analyses and the actual model results as an indicator of the usability of the analyses i.e. how the analyses work in a CTM above 50 km. This is now discussed in the last paragraph of section 3.1 (Meteorological conditions) as follows:

"It has been suggested by Manney et al. (2008) that the ECMWF operational analyses underestimate the variations in the stratopause altitude during extreme meteorological conditions. The operational analyses agree, however, well with satellite observations of MLS and SABER (Sounding of the Atmosphere using Broadband Emission Radiometry) in early winter stratopause temperatures. Compared to the MLS measurements Manney et al. (2009) the ECMWF operational analyses underestimate the altitude of the stratopause reformation in 2009 by about 5 km. The zonal wind, instead, resembles MLS measurements also at higher altitudes. Under 50 km altitude the MLS observations and operational analyses are in very good agreement."

Comment 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-NOx took place. So what is the reason to use the Operational data then? 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.*

Answer 4. The FinROSE-CTM uses dynamics (winds, temperature, pressure) that are from external sources. Therefore the model needs some kind of input for the dynamics on every time step. Because we are interested in the NO_x descent occuring at altitudes above $\sim$50 km, the ECMWF data with upper limit at 80 km (0.01 hPa) clearly suit for this study. By using these analyses we can also test how the data work and can it be used in a CTM above 50 km. In addition, the use of ECMWF operational analyses has the advantage that we can compare them and the model results with the corresponding observations. This is not the case e.g. for climate models. We now discuss this matter in the beginning of section 3.1 (Meteorological conditions).

We relocated the part of the text discussing the ECMWF operational analyses to

- the first paragraph of section 3.1 (Meteorological conditions), where we justified the reasons to use the operational data:

  "In this section we use the ECMWF operational data to analyse the meteorological conditions and their differences in early 2007 and 2009. We are aware of the possible shortcomings of the data Manney et al. (2008), but for this study it is important that the ECMWF fields extend up to 80 km, allowing us to model the NO_x descent starting as high as from the upper mesosphere."

- the last paragraph of section 3.1 (Meteorological conditions), where we compared the analyses against MLS observations shown by Manney et al. (2008) (see Answer 3).

- the fifth paragraph of section 3.2 (NO_x descent), where we draw our conclusions of the usability of the analyses in a CTM above 50 km based on the presented model results:

  "Although this is not a direct measure, the NO_x comparison suggest that the ECMWF data are in this case usable in atmospheric modelling also at mesospheric altitudes."

Comment 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 NOx and its effect on the stratospheric ozone rather than comparing the dynamic condition of 2009 winter and spring to that of 2007.*

Answer 5. We combined the figures as suggested, and also made an effort to write the text on the meteorological conditions in a more concise manner. However, the meteorological conditions and their differences between winter/spring 2007 and 2009 are of great importance for understanding the observed effects on NO_x. Therefore, we feel that it is justified to give them appropriate weight in the paper. In addition, NO_x was not transported down to the stratospheric altitudes where it would have contributed to the ozone balance and therefore no effect on ozone was observed. This gives additional value to understanding the meteorology behind the event.

Comment 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-NOx, 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 NOx 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-NOx on the stratospheric and surface temperature during the*

SSW years than during the non-SSW years. However, both Lu et al. (2008) and SeppÏä 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 chemstry and dynamics in FinRose model, or it is simply because the ECMWF Operational data at higher altitude are not so reliable?

Answer 6. The results of Baumgaertner et al. (2010) and other chemistry-dynamic coupled models mentioned by the Referee suggest a decrease in ozone concentrations due to descent of NO_x. According to the authors of these papers the changes in the amount of ozone might even affect the stratospheric and tropospheric dynamics and therefore also the ground-level climate. We simulated the descent event of NO_x in early 2009 with our CTM and came to the same conclusions as Randall et al. (2009) in their work based only on observations that NO_x did not descent low enough to affect the stratospheric ozone. On the other hand, we do not rule out the possibility for ozone loss caused by descending NO_x if the descent would have started earlier and NO_x would have had more time to descend inside the vortex before its split. The differences between our study and the others can therefore be explained with the non-existent NO_x at the altitudes needed for ozone destruction. In general, changes in ozone concentrations and possible feed back in the dynamics are taken into account in the operational analyses i.e. also in FinROSE as they are produced with a model that is constrained with observed ozone values below 50 km.

We now discuss this in section 4 (Discussion).

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

Answer 7. We have now explained more clearly the use of ACE-FTS observations at the model upper boundary in section 2.2 (Observations):

"We first calculated daily medians from the observations northward of 60°N for 2007 and 2009. As a result we got one value per day representing approximately a zonal average at the median latitude. Using these daily values we then calculated two-day means, which we in this study use at the upper boundary of the model. This two-day mean value is used on every time step for two days after which the next two-day mean is used. In case of missing data for both of the days in question, we use the previous two-day mean. The UBC is taken uniform at every grid point between 60°N–90°N."

Comment 8. *It may also be helpful if the authors can estimate and discuss the difference of the amount of descent EPP-NOx 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 NOx 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.

Answer 8. We performed two test runs, setting the UBC during the change in ACE-FTS measuring direction in different ways: we used 1) a constant value and 2) a two-step increment of NO_x at the upper boundary. The results show that the amount of NO_x changes but the descent stops at the same altitude as with the original UBC. This is expected since the same meteorological fields are used. We discuss this in section 3.2 (NO_x descent) as follows:

"To estimate the effect of different UBCs on the NO_x descent we made two additional model runs. In these runs we used the following upper boundaries: 1) a constant value ($\sim$670 ppb) between the 11th of February and the 4th of March and 2) a two-step increment of NO_x so that the first step ($\sim$190 ppb) is located between the 11th and 21st of February and the second one ($\sim$540 ppb) between the 22nd of February and the 4th of March. The results (not shown) for case 2 are in agreement with those obtained with the interpolated upper boundary shown in Fig. 1. In contrast, case 1 produces about 100 ppb higher NO_x mixing ratios between 60 and 80 km during the descent event until early March. At this time the mixing ratios are about 50 ppb higher. However, NO_x descent stops at the same altitude as with the interpolated upper boundary. As expected, the different upper boundaries change only the amount of descending because in all model runs the descent is driven by the same ECMWF meteorological data."

Minor comments

Comment 1. *NOx is not defined in the first place where is used, see the third line of Abstract.*

Answer 1. NO_x is now defined when it is used for the first time in Abstract.

Comment 2. *VLF needs to spell out in full when it is first used in the paper.*

Answer 2. Corrections made as suggested.

Comment 3. *Line 18, Page 9, "normalized quickly". Rephrase it as "normalize" is normally used as a mathematical term.*

Answer 3. We replaced the sentence "After early March the descent stopped and NO_x concentrations normalized quickly" with "After early March the descent stopped and NO_x concentrations decreased back to the level on which they were before the SSW."

Comment 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 NOx by what amount etc.*

Answer 4. Corrections made as suggested.

Comment 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.*

Answer 5. Corrections made as suggested.

Comment 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.*

Answer 6. Corrections made as suggested.

Response to Anonymous Referee #2

Comment 1. *I completely agree with point 2 of referee 1. I think the paper by Funke et al. (2007) also should be added in the references.*

Answer 1. To make the motivation of this study more clear we have now discussed the papers mentioned by the Referee 1 in comment 2 in section 1 (Introduction). Also the reference to Funke et al. (2007) is now added in to the fourth paragraph of section 1 (Introduction).

Comment 2. *The parts treating the ECMWF-data up to 80 km have to be revised. You are writing that the ECMWF data is not consistent with observa-*

tions above 50 km (p. 1433, l. 26) and "all the issues mentioned above might influence the model results"(p. 1434, l. 8). One of your goals was to test the quality of the ECMWF data. Having the statements from before in mind I think you really should do a test, e.g. do the same model run without using ECMWF above 50 km.

Answer 2. The FinROSE-CTM does not calculate the dynamics itself but takes it from an external source. This means that the model needs dynamical input to every grid point on every time step. Therefore we can not test the quality of the ECMWF operational analyses by not using them above 50 km. The model would not work. In addition, the point of this study was not to test the actual analyses but to find out are the analyses suited for this kind of modelling i.e. how does the NO_x descent compare to that observed.

Our sentence "the ECMWF data are not consistent with observations above 50 km" was a bit misleading as Manney et al. (2008) stated that the data underestimates the variations in the stratopause altitude. This is now changed in to section 3.1 (Meteorological conditions) as follows:

"It has been suggested by Manney et al. (2008) that the ECMWF operational analyses underestimate the variations in the stratopause altitude during extreme meteorological conditions."

Comment 3. *You only have a few observations by ACE-FTS. How are the NOx mixing ratios distributed at the model upper boundary? Uniformly or longitude/latitude dependent? Within the polar vortex or northward of 60N? How big is the uncertainty in the results due to the assumptions made for the*

UBC?

Answer 3. The NO_x mixing ratios at the model upper boundary are the same for all grid points northward of 60°N i.e. NO_x is distributed uniformly within the polar cap area. This means that we have not restricted the UBC to be valid only within the polar vortex nor do the UBC depend on longitude or latitude.

We have now explained more carefully the use of ACE-FTS observations at the model upper boundary in section 2.2 (Observations):

"We first calculated daily medians from the observations northward of 60°N for 2007 and 2009. As a result we got one value per day representing approximately a zonal average at the median latitude. Using these daily values we then calculated two-day means, which we in this study use at the upper boundary of the model. This two-day mean value is used on every time step for two days after which the next two-day mean is used. In case of missing data for both of the days in question, we use the previous two-day mean. The UBC is taken uniform at every grid point between 60°N–90°N."

We also ran the model again with two slightly different upper boundaries and the results are presented and discussed in section 3.2 (NO_x descent):

"To estimate the effect of different UBCs on the NO_x descent we made two additional model runs. In these runs we used the following upper boundaries: 1) a constant value (∼670 ppb) between the 11th of February and the 4th of March and 2) a two-step increment of NO_x so that the first step (∼190 ppb) is located between the 11th and 21st of February and the second one (~540 ppb) between the 22nd of February and the 4th of March. The results (not shown) for case 2 are in agreement with those obtained with the interpolated upper boundary shown in Fig. 1. In contrast, case 1 produces about 100 ppb higher NO_x mixing ratios between 60 and 80 km during the descent event until early March. At this time the mixing ratios are about 50 ppb higher. However, NO_x descent stops at the same altitude as with the interpolated upper boundary. As expected, the different upper boundaries change only the amount of descending NO_x because in all model runs the descent is driven by the same ECMWF meteorological data."

Comment 4. *Satellite data always have a limited vertical resolution. Observed mixing ratios therefore can be lower than the real ones. Is this considered in your measurement error? If not: How big is the influence of vertical resolution for NOx observations by ACE-FTS?*

Answer 4. The vertical resolution of ACE-FTS depends on the altitude and the beta angle (i.e. the angle between the orbit and measurement direction). Above 40 km the vertical resolution of ACE-FTS is about 5-6 km. The possible influence of the vertical resolution on NO_x observations is not taken into account in the measuring error of ACE-FTS. On the other hand, the vertical resolution of the instrument is comparable to that of FinROSE (4-7 km) and therefore we did not take the possible effects of the vertical resolution into account when comparing the observations with the model results.

Minor comments

Comment 1. *Figure 6 and figure 7 have different color scales. Please use same color scale for better comparability*

Answer 1. The color scales do look different in the discussion paper although they are exactly the same in the original figures. This is likely a technical problem at ACP editorial office and we have contacted the journal to correct it.

Comment 2. *Figure 6: NOx; Figure 7: NO; I guess both show NOx?*

Answer 2. Yes, both figures show NO_x. We have corrected the caption.

Comment 3. *Figure 9: loacations* $\rightarrow locations$

Answer 3. *Corrections made as suggested.*

Comment 4. *page 1431, line 29: spelling: "enchancement"* $\rightarrow enhancement$

Answer 4. *Corrections made as suggested.*