Response to Reviews

On our paper acp-2013-1024 ”Unusually strong nitric oxide descent in the Arctic middle atmosphere in early 2013 as observed by Odin/SMR. We thank both reviewers for their attention to our article. Below we present the detailed replies to each referee comment. All the modifications are written in red the revised manuscript.

RC C449

Specific comments

Anonymous referee #3

p3565 l 8-9: I would not agree that the Ap index can be directly used to infer EPP –NOx production. It is rather a proxy for geomagnetic activity which, in turn, drives the auroral NO production.

Authors

Exact. The introduction has been modified in order to clarify that point (l57-68 of the revised manuscript).

Anonymous referee #3

p3566 l7-9: It should be made clear that it is not the SSW which brings down the NOx (rather the opposite effect) but the associated ES event afterwards.

Authors

This point has been clarified in the revised version (l103).

Anonymous referee #3

p3567, section 2.1: It might be worth mentioning that, after communication with the Envisat satellite got lost, SMR/Odin is at present the only satellite instrument capable to measure NO with full global coverage (including the polar winter regions).

Authors

This information has been added to the revised manuscript (l152-156).

Anonymous referee #3

p3568 l18-19: Interestingly, GEOS-5 10 hPa zonal mean zonal winds in Manney et al. 2009 (Fig 1) show a stronger wind reversal in 2009 (easterlies exceeding 20 ms-1) than indicated here by ECMWF data.

Authors

GEOS-5 10hPa zonal mean zonal wind represented by the overlaid solid lines in Fig.1b in Manney et al. (2009) does not correspond to the same latitude than what we show in our Fig.1c. However, the Fig.1d in Manney et al. (2009), which represents the MLS-derived zonal mean zonal winds at 10hPa and 60°N, is comparable to our plot. Indeed, this figure shows a stronger wind reversal. Interestingly, MLS-derived winds are of the same order than ECMWF winds from mid-December until the reversal, but slightly lower in the first half of December, and significantly lower from the reversal until March. However, both datasets lead to the determination of approximately the same date for the wind reversal (24 January 2009 for MLS vs. 23 January 2009 for ECMWF). We do not know the reason for these differences.

Anonymous referee #3

p3569 l14-15: "...this figure shows that the tongue of dry air extended at least 5 km lower in the stratosphere in 2013 than in 2009". Isn't this mainly due to the later onset of the ES event (and also due to the earlier final warming) in 2009 compared to 2013?

Authors

Very good comment, thanks. The earlier onset of the SSW-ES event in 2013 compared to 2009 can indeed partly explain the observed differences between these 2 winters. What we observed with SMR is perfectly consistent with the results presented by Holt et al.(2013), who investigate the link between the timing of SSW-ES events and the amount of NO_x that descends to the stratosphere. We added comments about that in the revised manuscript, as well as a reference to the study by Holt et al. (l238-241 and l427-435).

Anonymous referee #3

p3572 l7: "caused", maybe better: "associated with"

Authors

This has been corrected in the revised version (l368-369).

Anonymous referee #3

p3573 l15: "all of these values" Which values? All altitude x time points in Fig 4?

Authors

Yes exactly: all altitude*time points in Fig.4. This has been clarified in the latest version (l418-419).

Anonymous referee #3

p3573 l19: Isn't it the associated ES event rather than the SSW which had a higher potential to affect the stratospheric composition. Of course, both are related, but since the SSW itself causes a polar NOx depletion (primarily by mixing polar and midlatitude air masses) this sentence might lead to confusion.

Authors

The revised version of the manuscript has been modified in several places in order to clarify that point and to insist on the fact that the observed NO downward transport is due to the elevated stratopause event associated with the stratospheric warming, and not to the SSW event itself.

Anonymous referee #3

p3574 l11-12: "...but these results were inconclusive because of the unavailability of ACE data during the first half of the winter,...". I don't see why the unavailability of ACE data during the first half of the winter prevents conclusive results regarding the EPP-IE related to the 2004 ES event. Randall et al. (2009) showed that the upper stratospheric NOx amounts in Feb/March 2004 were at least 3 times higher than in 2009. López-Puertas et al. (2006, DOI: 10.1007/s11214-006-9073-2), looking at MIPAS NO2 data (covering the complete 2003-2004 winter), concluded that these upper stratospheric NOx enhancements were related to EPP-IE induced by the ES event following the SSW in early 2004.

p3575 l12-13: "...and also because there was a possible influence of the strong solar storms that occurred in late 2003..." . The MIPAS data presented in López-Puertas et al. (2006) clearly indicate that the Halloween SPE contribution in the stratosphere and mesosphere is vertically and temporally separated from the contribution caused by the 2004 ES event. Further, Semeniuk et al. (2005, doi:10.1029/2005GL022392) argued that NOx produced by this SPE in the lower thermosphere would hardly survive until January-February (the onset of the ES event). It is thus very unlikely that the October/November 2003 SPEs had a significant influence on the mesospheric and upper stratospheric NOx increases observed in February-March 2004.

p3575 l14: "EPP-NOx enhancements were larger in 2009 than in 2006 according to Randall et al. (2009)". It is true that Randall et al. reported larger NOx enhancements in 2009 compared to 2006 in terms of maximum VMRs. However, the total amount of NOx (in terms of molecules (in GM) or density) brought into the stratosphere (i.e. ,below 2000 K) was higher in 2006 (see Holt et al., 2012, doi:10.1029/2011JD016663).

p3575 l16-18 "SMR measurements presented in our paper confirm therefore that the NO descent observed during the Arctic winter 2012/2013 corresponds to the strongest EPP indirect effect available on record". I'm not sure if this can be concluded from the presented analysis without a comparison to the extremely efficient (in terms of EPPIE) 2004 NH winter. The EPP-IE strength is not only given by the highest mixing ratios encountered but also by the area covered (vortex size) and the vertical position of the NOx layer (higher densities at lower altitudes). A quantitative comparison would require first to determine the total amount of NOx deposited into the stratosphere in 2013 (in terms of GM) and then compare to existing estimates for other winters (see, e.g., Holt et al., 2012). Why not simply stating that the 2013 winter was among the strongest EPP-IE winters on record?

Authors

We hereby answer to the last four comments from referee #3, which are all associated to the same problem. The interpretation of our results, concerning the comparison with the other observations of the EPP indirect effect, was mainly based on the comparison with the study by Randall et al. (2009). As justly pointed out by referee #3, this was not sufficient to come to the conclusion that the

EPP IE observed in early 2013 by Odin/SMR was the strongest event of this kind available on record. It was simply the strongest observed by Odin/SMR during the 2007 to present period.

In order to be able to carry out such a comparison, we would need to develop a method to determine the total amount of NO deposited in the stratosphere, close to the method described by Holt et al (*Atmospheric effects of energetic particle precipitation in the Arctic winter 1978-1979 revisited*, JGR, 2012), but which could be applied to SMR measurements. That could lead to a comprehensive analysis of the whole SMR NO dataset, not only for the northern hemisphere but globally. That would be a good way to compare the effects of EPP as seen by Odin to the other satellites. We have therefore decided to keep such a project for a later study, and to limit our paper to the case study of the 2013 event and its comparison with the other NH winters observed by SMR. The discussion section has been partly rewritten to take the comments into account, and the conclusions have been changed accordingly (mainly from l461 to l474).

RC C594

Specific comments

Anonymous referee #1

Over the entire manuscript: Make clear if you talk about energetic electron precipitation or solar proton precipitation or both. The term EPP (energetic particle precipitation) usually includes both. If you would like to focus on electrons use EEP (energetic electron precipitation).

P3565 l6: EEP IE instead of EPP IE

Authors

Even if the production of NO in the mesosphere / lower thermosphere is mainly due to regular precipitation of low and medium energy electrons, protons can play a role as well. We should therefore keep the term “EPP IE” to include both types of particles. The first part of the introduction has been rewritten in order to give more details about the particles involved (mainly from l30 to l43 in the revised manuscript).

Anonymous referee #1

P3567, end of section 2.1: Some information about comparison of SMR data (all data used here, i.e. temperature, H2O, NO) to other measurements, available validation etc. would be necessary here. In particular, because the paper states that the NOx downward transport was the most exceptional during the satellite era.

Authors

Lossow et al (2007) presented a detailed retrieval and error analysis of temperature and water vapour retrieved from Odin/SMR measurements at 557 GHz, and a comparison study with ACE-FTS data. This has been clarified in the revised manuscript (l135-140). SMR nitric oxide has been compared to NO measured by OSIRIS and ACE-FTS by Sheese et al. (2013) (already cited in the discussion paper). NO from SMR was generally found to be in between ACE and OSIRIS. However, this study was limited to the Antarctic night-time area. We don't have another published validation study for the moment. A global comparative study for NO in the MLT, over 9 years of data, including Odin/SMR, MIPAS, SCIAMACHY and ACE-FTS is currently in preparation for submission to AMT. It gives good results for SMR NO, but this study is not submitted yet, so cannot be cited for the moment.

Anonymous referee #1

P3567, l17: State where the temperature comes from. Later it becomes clear that it is from SMR, but this should be said here already.

Authors

It is mentioned in the immediately following sentence! Moreover, it was already said previously that we are using temperature measured by Odin/SMR in this study, and the altitude range within which SMR temperature is available was already given in line 4 on the same page. The information in brackets is therefore just a reminder of something already mentioned some lines before. We modified the revised version to make it even clearer, but we are not convinced that so many repetitions are necessary (l161).

Anonymous referee #1

P3568, l26-30: Could you comment how much increase of H_2O over this time you would expect from methane oxidation, and what this means for the balance?

P3569, l5-9: Again, what quantitative impact does methane oxidation have? And other potential (photo-) chemical sources and sinks of H_2O?

Authors

We hereby answer to the two comments above. The photochemical lifetime of methane at the altitudes considered here (40-90 km) is very long: from approximately two weeks to more than one year according to Brasseur and Solomon (*Aeronomy of the Middle Atmosphere*, 2005). The lifetime is shorter at altitudes higher than 70km (but still from two weeks to one month), but, at so high altitude, the methane concentration is very low. Although the oxidation of methane is an important source of H_2O in the stratosphere, we can assume that the impact of the reaction is negligible in the vertical range considered in our study.

A sink of water vapour above 70 km is the photodissociation caused by solar Lyman-α radiation. This effect should be very limited in the case of our study as we mainly look at the polar night, but it could have an impact during spring, after the end of the polar night.

Brasseur and Solomon (2005) show that the photochemical lifetime of water vapour is comparable to vertical transport lifetimes above ~45 km. This species can therefore definitely be considered as a very good tracer of dynamical processes in this region.

Anonymous referee #1

P3569, l13-14: This is, as sort of handwaving argument, certainly quite true. However, a real quantitative estimate on how far down and how important is the influence from subsiding air on the budget of water vapor would require a more quantitative assessment, e.g. with the help of other tracers. In the light of this, the statement here should be weakened.

Authors

This statement has been weakened in the revised manuscript, in order to make clear that it is based only on SMR H_2O observations (l233-237). Water vapour is nevertheless a useful and sufficiently long-lived tracer of transport (not only during polar night).

Anonymous referee #1

P3569, l15-16: Can this statement be expressed in wave number activity, e.g. wave 1, wave 2 etc.?

Authors

Of course, vortex displacements and splits are dynamically distinct. They are both triggered by anomalous wave activity, but the wave number can indeed differ. As described by Charlton and Polvani (2007), in vortex displacements, almost all the heat flux during the period of the warming is due to the zonal wavenumber 1 component, while, in vortex splits, it is mainly due to the zonal wavenumber 2 component. A first heat flux anomaly due to wavenumber 1 component is also generally observed approximately one month before the SSW central date in the case of vortex splitting. This phenomenon is called "preconditioning".

However, we think that going into all these details would not be appropriate here. That would bring nothing more to our study. This paper focuses on the case study of the NO descent observed in early 2013. It is of course important to discuss the stratospheric sudden warming, which plays a very important role in this effect, but our goal is not to present a comprehensive dynamical analysis of this event, which could in itself be the subject of another study.

Anonymous referee #1

P3570, l1: Another, probably earlier date than shown could possibly show the vortex split more clearly. There are many examples in literature that provide a picture of a vortex first elongating, then splitting. The figure presented here looks much more like a vortex shift away from the pole.

Authors

No, there is no clearer figure to show the vortex split at so high altitude. We plotted these PV maps for each day throughout the winter. The 9th of January, date for which the map has been represented in Fig.2, corresponds to the first day when we can observe two different vortices. The splitting occurred on the 6th of January in the lower stratosphere (central date of the warming), but some days later at higher altitude. The numerous examples in literature you are talking about, providing a very clear picture of the splitting phenomenon, correspond generally to a much lower altitude (lower or mid-stratosphere, 20-30km).

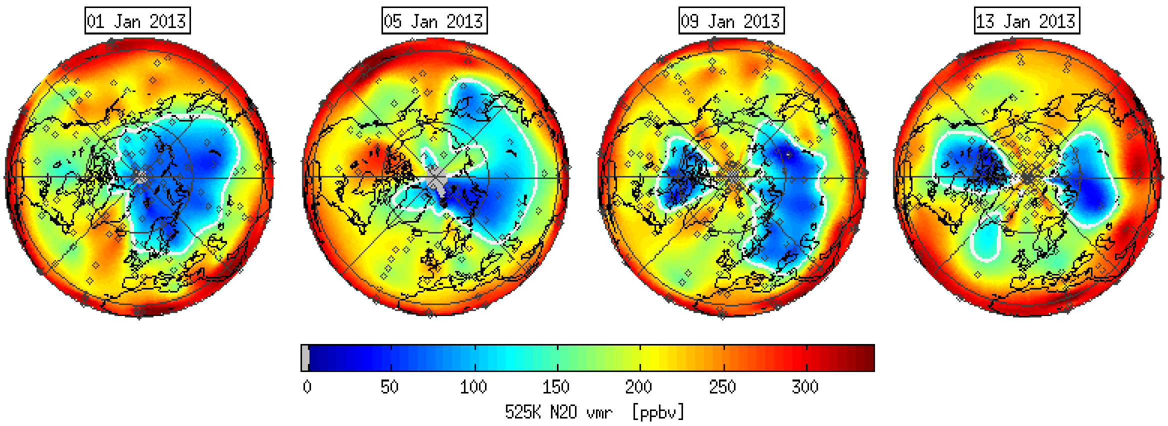
The size and the shape of the polar vortex are extremely variable, not only with time, but also with altitude. In the upper stratosphere, the vortex is much wider and not so well defined than in the lower stratosphere. This is especially because the vortex behaviour is so variable with altitude that we decided to show this picture (rather than the usually shown picture of the lower stratosphere), in order to stay focused on the altitude range under consideration in this study.

Anonymous referee #1

P3570, l18: Why is the N2O figure not shown if the picture becomes clearer with this additional information? And, in addition: do you observe the same behavior of the vortex over all altitude ranges? Again, there are many examples in literature, where the vortex remains one, shifted away from the pole, in certain (usually lower) altitude ranges, while at other altitudes the vortex splits into two sub-vortices.

Authors

Here is the N_2O figure in question, which shows the evolution of the polar vortex around the central date of the SSW, first elongating, and then splitting:



Polar maps of N_2O vmr measured by SMR in the lower stratosphere on four selected observation days during the warming event. White overlays: 150 ppbv contours (help to visualise the vortex edge). Grey diamonds: positions of the measurements.

As explained in the paper, we do not show this figure, which corresponds to an altitude of about 20-25km, because we chose to stay focused on the altitude range under consideration in our study (40-90km). We mention it anyway because, as already explained in the article, the classification of the SSW events is usually established in the lower stratosphere. This is therefore this information from SMR data that allows us to say that the 2013 SSW can be classified as a vortex split event.

As previously said, the vortex is characterized by a very high variability in size and shape with altitude. That explains the very different pictures we obtain if we look at different altitudes. Our main goal in this section is not to determine the category of the event, but to give an idea of what happened dynamically in the altitude range under consideration in this study (mesosphere / lower thermosphere). That is the reason why we chose to show the figure corresponding to the upper stratosphere, rather than the one corresponding to the lower stratosphere.

We modified the text in the revised version in order to insist on the high vertical variability of the vortex shape, and we added a reference to Goncharenko et al. (2013), who present a more detailed dynamical analysis of this event, in term of wave number activity, and confirm that the 2013 SSW was a vortex split event (l267-268 and l279-280).

Anonymous referee #1

General comment to the vortex discussion: Is it relevant for the subsidence behaviour INSIDE the vortex if the vortex is displaced or split? If the authors want to stay with this point, they need to discuss in a more quatitative manner how the wave activities of different wave numbers impact the subsidence in the vortex.

Authors

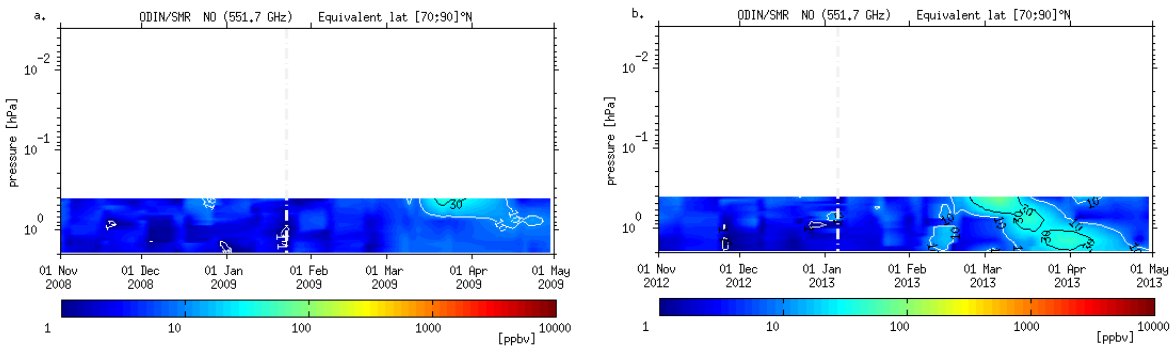
Again, the goal of our study is not to present a comprehensive analysis of the dynamics of the stratospheric sudden warming that occurred in early 2013, but to focus on the downward transport of NO observed by Odin/SMR during the weeks following this event. A basic description of the SSW, as presented in section 2.2, is of course necessary, since this dynamical event plays a very important role in the EPP indirect effect, as it is shown subsequently in the paper. But we strongly disagree with the suggestion according to which we should go into details of all these dynamical considerations. That would completely change the scope of the paper. Moreover, our study is based on the comparison of two SSW events which both led to a vortex split. The impact of the different wave numbers on the subsidence in the vortex is a complex question which could be the subject of another study! We simply add the reference to Goncharenko et al. (2013), which provides a more detailed dynamical analysis.

Anonymous referee #1

On the other hand, the extension of the vortex and its shape is probably relevant for the assessment of downward transport as shown in Figs. 1, 3 and 4 regarding the areas of the vortex which are covered by the [70N,90N] zonal means shown in these Figures. It is obvious from Fig. 3 that this area varies significantly over time, and it is to be expected that considerable differences exist between the years 2008/2009 and 2012/2013 regarding the evolution of the coverage of the vortex by the [70N,90N] zonal mean. Averaging over almost vortex-free areas (as shown for 9 February in Fig.3) will definitely lead to less NOx and less subsidence. This problem also arises when comparing the temporal evolution between the two winters. The way to proceed in order to avoid these uncertainties is an analysis based on equivalent latitudes instead of geographical latitudes.

Authors

Yes, there can be an influence of some mixing with the air from outside the vortex. And it would indeed be much better if the analysis could be based on equivalent latitudes instead of geographical latitudes, in order to avoid the undesirable effect of this mixing. We thought of this problem, and it has been discussed in detail in the paper (see p3570, l28 to p3571, l13 of the discussion manuscript). Equivalent latitudes were calculated from ECMWF PV fields, which are available only up to the lower mesosphere. As a result, it is not possible to cover the whole altitude range under consideration in our study. It is very tricky to clearly define the edge of the vortex in the mesosphere and lower thermosphere. However, as seen in Fig.2, the vortex is particularly wide at these altitudes. Consequently, the area above 70°N should be covered by the vortex throughout the winter season, except during some days around the SSW central date, when the vortex is strongly shifted away from the pole and eventually breaks. This can partly explain the slight decrease in NO vmr observed in the upper stratosphere and lower mesosphere at that time, as discussed in the paper (p3570 of the discussion manuscript). The poleward mixing could more likely have an impact on the [70;90]°N zonal means calculated at lower altitude, where the vortex is more confined (see N_2O figure above). Again, as already stated in the discussion paper, a comparison with the zonal mean NO vmr calculated using equivalent latitudes (in the altitude range where PV is available, see figures below) showed that the descent pattern was similar, slightly more pronounced and more continuous though, as expected. This test has been done for 2009 and 2013, and the conclusions were similar for both winters. It is of course important to keep that problem in mind when interpreting the results. That is the reason why the possible influence of this poleward mixing is mentioned in several places in the paper.



Same plots than Fig.3a and Fig.3b, but using equivalent latitudes instead of geographical latitudes.

<u>Anonymous referee #1</u>

P3570, l17-19: The (photo-)chemical conversion of NO during downward transport needs to be considered here, too, since this depends on illumination conditions.

P3570, l27-29: Careful with absolute numbers here, since what you see is the netto change resulting from several processes (at least downward transport, photochemistry, in-/out-mixing over the 70deg-latitude boundary).

<u>Authors</u>

We hereby answer to the two comments above. The photochemistry can indeed have an impact, as discussed later in the discussion manuscript (p3572, l14-20). But we agree that it is better to discuss that point already here. A comment about that has been added to the revised manuscript, and it has been made clearer that the observed NO vmr can be influenced by several different processes (l305-313).

Anonymous referee #1

P3571, l18-20: How do the Odin/SMR observations compare to earlier published results from, e.g. ACE-FTS observations (see e.g. Randall et al., 2006; 2009).

Authors

Randall et al. (2009) indeed presented a study of the same kind, showing the NO_x volume mixing ratios measured by ACE-FTS for several northern hemisphere winters, in order to look at the NO_x enhancements. Since only the measurements done by SMR after 2007 are considered in our paper (because of a modification in the temporal sampling after this date), there are only two winters in common between these two studies. The results are consistent with each other: both instruments showed that the NH winter 2007/2008 was very quiet with respect to NO, and observed an enhanced NO descent from the MLT down to the upper stratosphere in 2008/2009. The temporal and vertical extent of the observed NO tongues are similar. However, a quantitative comparison between these results is not possible because the measurements presented in Randall et al.'s paper correspond to NO_x vmr, while Odin/SMR measures NO only. Moreover, the limited latitude coverage of ACE-FTS, which changes over the winter season, can affect the results and make the comparison with this instrument a bit tricky.

Anonymous referee #1

P3571, l28-29: This statement cannot be derived from your observation nor anything else presented in this paper. You just assume that this is true! This statement must be marked as pure suspicion, as long as you do not present any proof.

Authors

This sentence has been reworded in order to make clear that this statement is based on the current knowledge we have of the phenomenon, and has not been derived from our observations (l353-355).

Anonymous referee #1

P3572, l12-13: These three winters cannot be considered as a climatological reference state; although no major SSW occurred, the conditions may have been very variable in terms of position, extent and strength of the polar vortex and strength of subsidence. These three winters might be used as reference but it must clearly be stated that this is done only because of lack of a more valid climatological reference.

Authors

Yes, of course, the state of the middle atmosphere can vary a lot from one year to another. This 3-year mean is not a climatological reference, but that is the best reference we have for our study, given the available data. That is precisely why we referred to it as "an estimate of the background levels during quiet conditions" (p3572, l11-12 of the discussion manuscript), and not as a climatological reference. The revised manuscript has been modified to make that point clearer (l375-377).

Anonymous referee #1

P3572, l14-15: The seasonal variation is due to the sampling pattern of the observations, as described afterwards, which covers different photochemical conditions for different times, and not due to the seasonally varying NOx partitioning as such.

Authors

The revised version of the manuscript has been modified to clarify that point (l380-383).

Anonymous referee #1

P3573, l7: stronger in terms of what? Winds? Subsidence rates?

Authors

Right, that point needed to be clarified. The two SSW-ES events are compared in term of subsidence rates here. The paper has been modified accordingly (l406-407).

Anonymous referee #1

P3573, l16: Do you mean excess by a factor of 3 and more? And why have you introduced this threshold?

Authors

We have introduced this threshold because, when we look at the figure 4, we can see that an enhancement of 3 or more is really representative of the NO downward transport that we want to study. It would have worked with a threshold of 2 as well, but we think that the value of 3 is more appropriate if we want to look at the NO tongue only, because some enhancements of 2 to 3 are noticeable throughout the winter. The formulation of this sentence has been clarified in the revised version (l418-419).

Anonymous referee #1

P3573, l19-20: Both tongues do not reach the maximum of the ozone layer, though.

Authors

That is right; this comment has been added to the revised version (l425-427).

Anonymous referee #1

P3574, l1-2: "That allowed more magnetospheric electrons to precipitate into the polar MLT, which increased ionization": This has not been shown in this paper, nor has it be cited from another publication. How do you know? Of course we assume that this is the case, but for a scientific publication we need evidence. And for improving our current understanding, we further need quantification.

Authors

This sentence has been reworded in the revised manuscript, and more details are now given in the introduction about the link between geomagnetic activity and NO production in the MLT (l446-449).

Anonymous referee #1

P3574, l2: ": : : and led to a higher NO production level": Can this statement be quantified?

Authors

The averaged vmr throughout both winters (2008/2009 and 2012/2013) has been calculated in order to quantify the difference in SMR observations with respect to NO measurement in the MLT. We added this information to the revised manuscript (l449-455).

Anonymous referee #1

P3574, l7 and before: This is all consistent with current understanding, but it does not really improve our current state of knowledge. The relationships mentioned here are thought to be known, and the authors refer mainly on this common knowledge, without really improving this knowledge by providing quantitative assessments, correlations etc.. The general message of the paper is: we see with the SMR instrument what we expected to see from earlier observations. As it stands now, the paper documents the monitoring of just another EEP event with observational data which, as such, is good and worth to be published; but based on the observational data and the material presented in the manuscript, I think it could have done better.

Authors

We did not say that this study in itself really improves our current knowledge of the EPP impact on the atmosphere! We presented the observation of a new, particularly strong, case of EPP IE by Odin/SMR, and we said that addressing this case in further studies could help the scientific community to get a better understanding about this mechanism (p3574 l20-24 of the discussion manuscript). Of course, much more can be done with SMR measurements, which constitute a rich data set in that research field. The study presented in our paper is the first one submitted about that topic with this instrument, and we plan to continue working in that direction in the future. Anyway, this part of the discussion and the conclusions have been rewritten in the revised manuscript.

Anonymous referee #1

P3574, l10-18: I fully agree with referee 3 that this manuscript here considerably lacks to pay attention of the SSW in 2004. I don't see why lack of ACE-FTS data should lead to inconclusive results on the 2004 event given the fact that there were more observational platforms available and more papers published on this event than the ones cited here. I do not want to repeat the comments of referee 3 in all detail, I just want to state that I fully agree with those. Similarly, I think the paper has failed to demonstrate that the EPP-IE following a SSW/ES event in 2013 was the strongest on record.

Authors

This part of the discussion section has been modified in the revised manuscript (see our reply to the last comment from referee #3 above).

Technical comments

Anonymous referee #1

P3570, l23: typo “immediately”

Authors

Correction done.