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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Unusually strong nitric oxide descent in the Arctic middle atmosphere in early 2013 as observed by Odin/SMR" *by* K. Pérot et al.

Anonymous Referee #1

Received and published: 20 March 2014

General comment:

The manuscript presents observational data in terms of zonal means related to the SSW/ES event in early 2013 as derived from Odin/SMR. The data are interpreted in terms of NO enhancements in the stratosphere/lower mesosphere, and compared to a similar event in early 2009. The paper concludes that "the NO descent observed during the Arctic winter 2012/2013 corresponds to the strongest EPP indirect effect available on record."

As a purely observational paper monitoring another SSW/ES/EPP-IE event, the paper

is good in its present form and acceptable for publication in ACP after consideration of a few minor issues. However, the authors attempt to demonstrate that the 2013 event belongs to the strongest EPP indirect effect on record. For this attempt, the argumentation is often not rigorous enough, but rather hand-waving, and, in particular, a sound quantitative assessment is missing. In particular:

- For a sound quantitative assessment the analysis must be done on equivalent latitudes, not geographical latitudes, for all winters under analysis.
- The discussion of vortex displacement vs. vortex split as it is now in the paper leads to nothing.
- The assessment of the amount of subsided NOx lacks the consideration of photochemical and mixing processes.
- Statements on processes beyond the altitudes covered by observations should be made with more care; they need to be proven by references to the literature or should be avoided (e.g. P3571 "... As a result, very little NO was produced in the MLT. ..." or P3574 "...That allowed more magnetospheric electrons to precipitate into the polar MLT, which increased ionization...")
- The authors need to address the SSW/ES/EPP-IE events in early 2004 for their statement that the 3013 events belong to the strongest EPP indirect effect available on record.

I recommend major revisions for the paper before it can be published if the scope of the paper will be kept. The required major revisions are detailed in the specific comments below. As a purely observational paper without any further interpretation of the observations, the paper might require minor revisions regarding its contents, but it needs to be rewritten over wide parts.

Specific comments:

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

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.

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

P3568, l26-30: Could you comment how much increase of H2O 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 H2O?

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.

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

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.

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.

General comment to the vortex discussion: Is it relevant for the subsidence behavior 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.

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.

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).

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).

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.

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.

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.

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

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

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

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.

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

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.

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.

Technical comments:

P3570, l23: typo "immediately"

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