We would like to thank the referees for the effort to critically review this manuscript which has lead to its substantial improvement.

Responses to Anonymous Referee #1:

Comments from Referee:

This work addresses the consequences of proscribed ozone changes on middle atmospheric temperature and wind fields. Specifically, it is considering ozone changes in the context of energetic particle precipitation (EPP) which might cause polar NOx enhancements. It is an interesting and relevant topic for this journal. My main concern, and it is a very serious one, is that the assumed ozone perturbations are demonstrably unrealistically large. Typically, they are well in excess of observations, in some cases of the wrong sign, and thus the effect is to dramatically overstate the importance of EPP to middle atmospheric composition and structure. This work needs to be reconsidered until more realistic assumptions are made.

Despite all the above, the work has potential value because it casts serious doubts on the reality of published correlations between surface temperature and geomagnetic activity comment #4 below). But before that they need to reconsider much of what they have done.

Author's Response:

We thank the reviewer for the honest and constructive criticism. We agree that we use idealized anomalies to trigger a response (which are based on observational evidence), but we feel that testing a mechanism and looking for sensitivities is justifying a certain amount of simplification. In the point-by-point reply, we will make the case for our idealizations and we reflect our explanations in the manuscript. We thank the reviewer for acknowledging the general importance of our results for the detection and attribution discussion and hope that the revised manuscript avoids any possible misunderstanding.

Comments from Referee:

1a) The text states that the ozone perturbations are guided by the Fytterer et al (ACP, 2014) study. However, that study only looked at the Antarctic; the present study applies this to the Arctic which is not valid. Arctic NOx enhancements (and ozone reductions) in the stratosphere are rare- to date only the 2004 spring can be considered a reliable detection (cf. Natarajan et al., 2005; Randall et al., 2005) although the spring of 2013 (Bailey et al., GRL, 2014) could be another candidate. The other year with significant mesospheric descent was 2009 and studies of that year have failed to find significant NOx descent into the stratosphere (Salmi et al., ACP 2011; Siskind et al GRL, 2015). Thus we have, at best, two years out of 10 and nothing for the other 8. At best, their assumed ozone reductions for the Arctic could be characterized as the extreme case.

Author's Response:

To our knowledge, the best observational record of EPP produced NOx (or NOy) is provided by the MIPAS instrument, covering the period of 2002-2012 as summarized in Funke et al., “Mesospheric and stratospheric NOy produced by energetic particle precipitation during 2002-2012”, JGR, 2014. The MIPAS time-series clearly shows EPP-produced NOy at high latitudes in both hemispheres above 60 km in all winters observed, above 50 km in all but two winters (the exceptions being Northern hemisphere winter 2010/2011 and 2009/2010, Figure 2 of Funke et al., 2014). Clearly, production of NOy in the upper mesosphere or lower thermosphere is something that occurs all the time, modulated in strengths by (probably) geomagnetic (auroral) activity, but never completely ceasing. EEP NOy is also observed below 40 km altitude in all of the polar winters observed, though not statistically significant in NH winters 2009/2010. In all other winters, excess NOy can be traced down to ~25 km in both hemispheres, though the signal varies from year to year (probably with geomagnetic activity) and is generally less strong in the Northern hemisphere (Figure 9 of Funke et al., 2014).

It should be noted that in all winters with a strong EEP-NOy signal, a negative signal is observed in the mid-stratosphere below the strong positive signal, maybe comparable to the negative NOy-signal shown in Figure 6 of Fytterer et al., 2015.

MIPAS is better suited than most (not all) other instruments able to observe NOx species because as an Infrared-Instrument the observations are independent of solar light; MIPAS can provide a continuous daily global view of stratospheric and mesospheric Nox (up to ~70 km, the top altitude) even during polar night. Instruments observing scattered or direct solar light, like HALOE, POAM, SBUV, SOFIE, or SCIAMACHY, can't observe during polar night. However, this is the time when the downwelling of EPP NOy from the mesosphere to the stratosphere occurs. To clarify this point, a summary of observations of the EPP indirect effect in NOy has been added in the introduction. The reason the Northern hemisphere is not considered in Fytterer et al., ACP, 2015 (note the year of the reference) is that in the Northern hemisphere, the mid-winter circulation is disturbed by sudden stratospheric warmings (SSW) in about 60% of all winters. In SSW winters, the NOy signal will be mixed with outside-vortex air and diluted, so a continuous downward propagation will not take place. As the time-series available for ozone observations covering polar night was only a few years long at the time of the Fytterer et al study, there were not enough years available without SSWs for study. However, results from early winter before the onset of mid-winter warmings) showed results consistent with the Southern hemisphere (T. Fytterer, personal communication). We argue that in winters without SSWs, the Northern hemisphere will behave similar to the Southern hemisphere, though the stronger downward motion in the Northern hemisphere might lead to lower Noy values as suggested, e.g., by Funke et al., 2014 (note: SSWs occurred in NH winters 2003/2004 (7 January 2004)), 2005/2006 (21 January 2006), and 2008/2009 (24 January 2009)). A relevant question for NH winter therefore is whether potential dynamical changes due to the EPP indirect effect are strong enough to affect the frequency and timing of SSWs.

We also would like to stress that in the idealized time-slice model experiment shown here, we highlight the difference of two extreme cases which might not have happened in the recent past, indeed might never happen – a year with a very strong NOy signal denoting both a strong and stable downward descend, and very high geomagnetic activity compared to a year without any

EEP NOy signal anywhere in the middle atmosphere, which considering the MIPAS results, seems more unlikely. A discussion of this will be implemented in the abstract and in section 2.3.

Comments from Referee:

1b) As far as the Antarctic, there is greater evidence for recurring stratospheric Nox enhancements (and ozone reductions); however, the maximum depletion that Fytterer show is 20%, not the 30% assumed here. Furthermore, the sign of the perturbation reported by Fytterer differs at some altitudes than what is assumed here- in the lower stratosphere they report a positive correlation between Ap and ozone, not the negative effect assumed here.

Author's Response:

Fytterer et al investigated the variation of ozone from year to year in 2002 – 2011, e.g., covering years with a very high (2003, 2005) and very low (2010) excess NOy, see Figure 9 in Funke et al.., However, even in 2010, excess NOy in Southern hemisphere winter was not zero. What is shown in Fytterer et al is the difference of the (mean of years with high Ap)- (mean of years with low Ap), using the 50% percentile as the separator (3-5 years in each bin). Fytterer et al therefore show something like a mean interannual variability; the variation from peak “very high Ap” to “very low Ap” years should be considerably higher, but is not available from such a short period of time. In the idealized model experiment shown here, we highlight the difference between two extreme cases, a year with a very strong NOy signal compared to a year without any EEP NOy signal. As the ozone difference of such years can (at the moment) not be derived from observations, we extrapolated the possible amplitude of the ozone differences of a strong forcing compared to no forcing from model studies investigating a year with strong forcing (Baumgaertner et al., 2009, Reddmann et al., 2010). The shape of the downwelling signal was chosen to be consistent both with the negative ozone anomaly in Fytterer et al., 2015, the modelled shape of the negative ozone anomaly in Baumgaertner et al., 2009; Reddmann et al., 2010; and Rozanov et al., 2012, and the area of observed EPP-NOy as shown by Fytterer et al., 2015, and Funke et al., 2014.

This is clarified now in Section 2.3, which has been rewritten to make the choice, shape, size, and justification of the scenarios more clear. A more detailed discussion of the observed and modelled ozone changes due to EPP-NOy is now included in the introduction.

It is true that a positive ozone anomaly is shown in Figures 2 and 5 of Fytterer et al., 2015, in the stratosphere. This positive ozone anomaly is located always below the negative anomaly, and might be due to self-healing of ozone as pointed out by reviewer #2. Comparison with Figure 6 of Fytterer et al shows that this positive ozone anomaly correlates to a negative anomaly in NOy at least below 30 km, maybe signifying a dynamical feedback. Here, we focus on the direct effect of the downwelling of NOy, which is restricted closely to the areas of negative ozone anomalies, compare Figures 5 and 6 of Fytterer et al. Consideration of indirect effects as, e.g., self-healing or dynamical feedbacks, as also suggested by reviewer #2, would make the model set-up, as well as the interpretation of the results, fairly complicated.

Comments from Referee:

1c) The authors refer to papers such as Rozanov et al 2005 and Baumgaertner et al 2011; however, this reviewer would argue that those papers also overestimate the phenomenon of EPP NOx production. Randall et al (JGR, 2007) discuss how their observations are lower than Rozanov's simulations. For Baumgaertner et al, figure 6a of that paper shows over 30 ppbv of NOx in a deep layer from 40-50 km in January to represent an "exemplary" Northern Hemisphere winter. But reality for an extreme Northern Hemisphere winter is given by Figure 1 of Bailey et al [2014] (i.e. absolutely nothing in January and a narrow layer in March which dissipates in April).

Author's Response:

t is difficult to read 40 ppb from the logarithmic color scale, but Figures 1 and 2 in Funke et al., 2014, clearly shows that at least NOy values of more than 10 ppb are quite common in 40-50 km in polar winters in both hemispheres; in the 17 winters observed by MIPAS, only three do not reach this margin: SH 2010, NH 2009/2010, and NH 2010/2011. In January 2014, a major warming occurred, diluting any NOy below 80 km at high latitudes- this is very clearly seen in Figure 1 of Bailey et al., which shows more than 10 ppb of NO (not the same as Noy) above 60 km in early January, which than vanish completely during the warming. After the warming, more than 100 ppb of NO (not the same as NOy!) are transported quickly down to altitudes of about 50 km in early March, about 10 ppb are observed just above 40 km around day 80-100 (end of March). The same event is also observed by SMR/ODIN, a microwave instrument which can also observe in polar night; SMR data show more than 100 ppb of NO at 7*10 -2 hPa (~60 km) in February North of 70°N, more than 30 ppb of NO above 1 hPa (~45 km) in mid-March, and more than 10 ppb of NO above 5 hPa (~35 km), see Perot et al., 2014. Sinnhuber et al., ACP, 2014, show the downward propagation of NOx (NO and NO2) in one truly extreme polar winter, NH winter 2003/2004, as observed by MIPAS inside the polar vortex during polar night. They show vortex averaged NOx of more than 1000 ppb down to 60 km in early February 2004 after a strong SSW, and still more than 100 ppb of NOx in 40-50 km in mid-March (Figure 3 of Sinnhuber et al., 2014). The 340 ppb of the Baumgaertner et al.paper Rozanov et al therefore appear not unreasonable for a strong forcing, though the timing in January might be questionable. It should be pointed out that the shape of the downwelling signal shown in Baumgaertner et al., 2009; Reddman et al., 2010; and Rozanov et al., 2012, agrees quite well to the observations of Fytterer et al., 2015. The observed amount of mesospheric and stratospheric EPP-NOy and the role of SSWs in Northern hemisphere winters is now discussed in more detail in the Introduction.

Comments from Referee:

1d) Finally, Figure 2 shows a 30% depletion originating in the 0.1 – 0.01 hPa layer and gives the impression that this propagates downward. I think this is misleading. An ozone perturbation at these altitudes is due to HOx chemistry, not NOx and is known to be short lived. The many works of Jackman show that ozone-HOx perturbations dissipate in a few days and do not propagate down into the stratosphere.

Author's Response:

It is true that above ~1 hPa, the ozone loss is due to catalytic cycles involving HOx. However, some of the model studies available on the subject of EEP-NOy extending into the mesosphere (e.g., Rozanov et al., 2005; 2012; Semeniuk et al., ACP, 2011), show a negative response of mesospheric ozone up to at least 0.01 hPa (~80 km). A similar feature is observed in Figures 3 and 5 of Fytterer et al. when looking at MIPAS data only; for the composite, the mesospheric signal is dominated by a very strong, but very noisy signal from SMR. In a very recent paper, model studies with the SIC ion chemistry model indicate that NOy does modulated mesospheric ozone even in polar night, by affecting the partitioning and therefore the lifetime, of HOx (Verronen et al., GRL, 2015). Considering this, we think that a negative ozone signal in the early winter mesosphere is a realistic feature. Because of its potential relation to NOy (see Verronen et al., 20145), it is also realistic to relate it to the downwelling signal, though it might be argued that it should extend longer in the mesosphere because direct production of OH in the mesosphere might have a big impact on ozone throughout polar winter (see Andersson et al., Nature, 2014). A longer extent of a negative mesospheric ozone signal is shown both in Semeniuk et al., 2011, and Rozanov et al., 2012; however, we want to focus on the impact of the downwelling NOy signal here. However, we want to point out that while 30% is in range of the values given by Semeniuk et al., 2011 and Andersson et al., 2014, it exemplifies an extreme value. The mesospheric ozone anomalies are now discussed in more detail in the introduction.

Comments from Referee:

1e) It’s actually not obvious what Figure 2 really means. Do they change the perturbation in a discontinuous fashion from month-to-month? Or are they initial conditions which propagate downward of their own accord.

Author's Response:

Thanks for pointing this out –this is now clarified in section 2.3 of the paper. In the EMAC chemistry-climate model version 2.4.2 setup without interactive chemistry, ozone is prescribed from a climatology as monthly mean values. In the disturbed scenarios, the monthly mean values are reduced by 30%. However, for each day, ozone is interpolated from the 15th of each month to the 15th of the next month. This provides a continuous descending ozone anomaly downwelling signal whose amplitude varies from 0-30%, and is on average, 15%.

Comments from Referee:

I think that this work needs to be reconsidered in light of what actually happens in the upper stratosphere and lower mesosphere. The perturbations between NH and SH are quite different and overall, smaller than what the authors assume (much smaller in the NH, somewhat smaller in the SH). They are also focused on a much narrower altitude range than assumed here (mainly between 1.0 and 20 hPa). I expect the resultant effects to be less (but more realistic). I think it eventually should be publishable, but only if it adheres to what is observed.

Author's Response:

As our assumptions about the shape of the downwelling NOy-EPP signal are based on the very detailed MIPAS timeseries, and the strength of the anomaly on the most detailed model studies available, we feel that we provide a reasonable estimate of what a strong, possibly extreme, particle forcing would look like. However, we stress again that this is an idealized model experiment meant to exemplify the difference between a strong to extreme particle forcing, and no forcing at all. This is now clarified in Section 2.3.

Comments from Referee:

2) The question of “self healing” of ozone is not addressed, but should be. This is the idea that ozone loss at a higher altitude allows for greater ozone production below. This might be the cause of the positive O3-Ap relation that Fytterer observe, and they speculate as much.

Author's Response:

Yes, that is true. However, self-healing would be an indirect impact of the EPP-NOy induced ozone loss, and there might be other indirect effects – changes in the vertical transport speed for example or vortex strength – which might also affect ozone at other altitudes. It is difficult to assess and implement these indirect feedback processes, and we wanted to keep the model experiment as simple as possible. Anyway, Jackmann et al., 2000; 2013, show that even for large SPEs, the self-healing is small, generally less than 1%. This is also discussed now in Section 2.3.

Comments from Referee:

3) Finally there is a question for the rationale for the O3-TS simulation. This presumably is a solar effect from photons, not particles. So why is it included in a paper entitled “EPP-ozone changes”? It seems out of place. But it need not be. I suggest that if they want to keep this simulation (there is nothing fundamentally wrong with it), they should consider these two suggestions.

Author's Response:

Thanks for pointing this out. The title of the paper has been changed to clarify the intentions of the study: “Are confined ozone changes in the middle atmosphere triggering large-scale dynamical anomalies? – An idealized model study on the role of geomagnetic and solar forcing”.

Comments from Referee:

a) Change the title of the paper to something like “On the relative roles of photons and energetic particles to middle atmospheric temperature and dynamics”.

Author's Response:

See responses to 3.

Comments from Referee:

b) They should additionally include the 20 km perturbation to ozone that Soukharev and Hood reported.

Author's Response:

This is a really intriguing point. However, as Hood and Soukharev point out, this ozone anomaly is likely due to a dynamical feedback. In the idealized model experiment shown here, we want to restrict ourselves to the direct radiative (and NOy-related) impact of solar spectral variability (particle forcing). This is now clarified in Section 2.3.

Comments from Referee:

4) Even with the overestimated perturbations, and certainly upon revision downward, the lower tropospheric perturbations fall well short of those reported by Seppala et al (2009). The present paper barely gets a 1K perturbation into the tropopause region (Figures 4-5). To me, this casts serious doubt as to the reality of Seppala's correlations. At a minimum, this discrepancy needs to be discussed here. Ultimately this may be the real value of this paper (i.e. proving that Seppala's results are theoretically difficult/impossible to explain).

Author's Response:

Actually, the authors share some of the uneasiness of reviewer #1 about the Seppälä et al. results, but for a different reason; the small number of years used in the Seppälä et al study after excluding strong solar forcing, volcanoe years, and years with SSWs make the attribution to geomagnetic forcing difficult. A much longer time-series would be needed for a robust attribution of tropospheric changes to geomagnetic (or indeed solar) forcing. We restricted ourselves to analyse mainly the middle atmosphere because a stronger and more robust response might be expected there.

However, our results show different quantites than Seppälä et al., which should not be compared directly. We show that the zonally averaged temperature do not change significantly in the lower troposphere, while Seppälä et al shows changes in surface air temperature on a regional scale. In particular, she shows a dipolar change reminiscent (at least) of the NAO, with high values over Northern Europe and a reversed sign over Greenland – the zonal mean might be fairly low in Seppälä et al as well, so our data are not in disagreement (or, as we don't show surface air temperature maps, are not shown to be in disagreement) with Seppäläs. That a possible solar response of surface air temperatures resembles the NAO, with regionally high values, but low values in the zonal average, has been shown in a few of publications, e.g., Lokwood et al., Sur Geo, 2012 (Figure 4), or Lean and Rind, GRL, 2008 (Figure 3). However, we did not show surface air temperatures for another reason here: the version of the EMAC model we use does not have a coupled interactive ocean module, so ocean surface temperatures cannot be derived by the model. Therefore, ocean air temperatures have to be prescribed, and changes in the lowermost troposphere will be low, at least over the ocean. The same incidentally is true for Baumgaertner et al, 2009/2011, who use an older version of the same CCM. A discussion will be implemented in the text.

Comments from Referee:

5) I have refrained from commenting in detail on their dynamical diagnostics because they will likely change significantly once the initial perturbations are done more accurately. Editorial: The abstract is pretty qualitative, overly so in my opinion. It gives no numbers and as a result is not helpful for someone looking for a quick order of magnitude estimate.

Author's Response:

Thanks for pointing this out. Abstract will be rewritten accordingly.

Comments from Referee:

Grammar: While I expect this sentence to be significantly modified (or deleted) in a revision that more accurately characterizes the ozone perturbations, the sentence on lines 890 should read " . . . at least be comparable to) . . .. . …

Author's Response:

Thanks for pointing this out. The sentence will be rewritten accordingly.

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