

Anonymous Referee #1

We thank you for your positive reception of the manuscript, and for the helpful and constructive comments. As we understood, major points raised by referees is manuscript structure. In the revised version, we tried to follow referee's recommendations.

The reviewer comments are given in normal typeface, *our responses are italicized and bold*.

General comments:

Since much of the discussion focuses on the geomagnetic forcing and in particular energetic particle precipitation (EPP) impact on the mesosphere chemistry and the link to temperatures, it would be beneficial to have a short paragraph of the now well understood effects of EPP in the introduction going into the proposed temperature impacts via ozone modulation in the mesosphere. In the discussion, the link to temperatures is somewhat difficult to follow. I would recommend clarifying this following along the line of these steps:

1. EPP ionisation leads to production of both HOx and NOx species. This production can be proxied using indices such as Ap. (This is the main link to the Ap-temperature correlations of this study)
2. HOx and NOx contribute to ozone balance in the mesosphere and stratosphere. These effects are well known as demonstrated by the works cited in the existing text.
3. Model simulations have shown that the EPP driven ozone reduction in the polar winter upper mesosphere leads to reduction in long wave (terrestrial outgoing radiation) cooling. This signal is seen as increase of upper mesospheric temperature when comparing simulations with high EPP forcing to those with no, or low EPP forcing.
4. Higher Ap -> more EPP -> more HOx and NOx -> less ozone -> impact on polar winter mesospheric temperatures. This effect on temperatures is focused on polar winter atmosphere, which seems to be in a good agreement with the results presented in this manuscript.

It is not clear from the text presently how sensitive the layer of excited hydroxyl used for the temperature measurements is to changes in HOx concentrations i.e. those related to EPP. Could you please add a comment? This I think is needed to clarify to the readers whether the observed temperature changes are likely linked to changes in ozone or in HOx concentrations.

Response: In the revised article, part of the discussion concerning an indirect effect on the atmosphere from particle penetration was significantly reduced and moved to the introduction. In the introduction and discussion, the main attention is paid to the response of the temperature of the upper mesosphere / lower thermosphere to geomagnetic activity. This particular paper does not process HOx at all. We used the rotational temperature of OH (6-2) band as a proxy of the neutral temperature of the atmosphere. The auroral atomic oxygen line is superimposed on the OH spectrum so that it is impossible to correctly calculate the rotational temperature. Thus, the spectra obtained during the aurora were excluded from the study.

Specific comments and text revisions:

Page 1 L7: "beginning of the 24th" L11-13: "The maximum of the seasonally averaged temperatures is delayed by 2 years relative to the maximum of flux of radio emission from the Sun with a wavelength of 10.7 cm, and correlates with a change in geomagnetic activity. Ap-index as a measure of geomagnetic activity is taken." Change to "The maximum of the seasonally averaged temperatures is delayed by 2 years relative to the maximum of solar radio emission flux (wavelength of 10.7 cm), and correlates with a change in geomagnetic activity (Ap-index)." **The sentence was corrected.**

L19-20: "The review of Beig et al. (2008) lists numerous studies showing that the response: : :". **Corrected.**

L21: Add the abbreviation F10.7 here as it is used later: “solar radio flux at a wavelength of 10.7 cm in 10-22 W M-2 Hz-1 (F10.7)”. **Abbreviation F10.7 was added.**

L22-23: “SABER radiometer onboard the TIMED satellite”. **Corrected.**

L23-24: “100 SFU, in agreement with the”. **Corrected.**

L26-30: You should make it clearer in this paragraph that the first studies only used a very short period of observations. **The sentence “This study only used a very short period of observations which coincided with the maximum of solar activity.” is included.**

L35-36: I recommend revising this to: “ As this is similar in scale to the observed delay of 25 months, it was logical to assume that the long-term temperature fluctuation of the subauroral mesopause correlates with the change in geomagnetic activity.” **Sentence is changed according your recommendation.**

L37: “: :between geomagnetic activity (Ap-index) and: :” **The purpose of paper is described as: “to find geomagnetic signatures in night measurements of OH rotational temperature obtained for the period August 1999 to May 2015”.**

Page 2 L2: “Mesopause (80-100 km) is the atmosphere region where the mesosphere borders on a thermosphere: :” **Sentence is changed according your recommendation.**

L4: Does “activated” here refer to “excited”? **“activated” is replaced by “excited”.**

L4: “: :hydroxyl molecule experiences: :” **“commits” is replaced by “experiences”.**

L8: “optical station Maimaga (63°N, 129.5°E) which is located at a distance of about 120 km to the north of Yakutsk” suggest changing to “optical station of Maimaga (63°N, 129.5°E) located about 120 km north of Yakutsk, Russia.” Could you also give the magnetic latitude of the station? **The sentence is changed. Geomagnetic coordinates of Maimaga station are added in text.**

L10: What is the significance of not having aurora present when the observations are made? This would have an impact on observing the direct EPP effect as particle precipitation can be associated with aurora displays. **The reason is described in the “Instrumentation and measurement technique” as: “The atomic oxygen line which arises at high auroral activity superimposes on OH(6-2) spectrum. To avoid systematic errors in evaluating the temperature because of this, the data obtained in the absence of aurora were selected for the analysis”.**

L17-19: These 2 sentences are presently not clear. **These sentences are replaced by:” The temperature corresponding to that model spectrum, which deviates least from the real spectrum, by not more than the registration noise, is considered as a best fit to the real hydroxyl rotational temperature. The random errors in measuring the temperature are typically 2-10 K, depending on signal-to-noise ratio.”**

L27: “: :Ap-index mean values are shown: :” **“... variations ...” is excluded.**

L30: “The correlation coefficient of TOH and Ap-index is equal 0.51 ± 0.1 at 95% confidence level.” Remove word “equal”. What is the correlation of F10.7 and TOH in your present dataset? **The sentence is changed to text: “The correlation coefficient of TOH and Ap-index is 0.51. The significance of correlation coefficient was tested with 14 degrees of freedom T-test. The critical value of correlation coefficient is 0.46 at the 0.05 level of significance. TOH is not significantly correlate with F10.7, because correlation coefficient 0.36 is less than critical value. The correlation coefficient increases to 0.65 when F10.7 leads the temperature by 2 years.”.**

Paragraph starting at line 31: How were the two Ap groups selected, what is the transition value of 8 based on? **“The average AP in the observation interval of about 8 was chosen as the transition value”.**

Page 3 L1: “: : many papers have been published on the atmosphere response to solar and magnetospheric proton: :” ***The phrase “In the last decade, many papers have been published on the atmosphere response to the proton and electron fluxes with various energies” is shifted to “Introduction”. As was mentioned above, part of the discussion concerning an indirect effect on the atmosphere from particle penetration was significantly reduced and transferred to the Introduction. P3 L1-L5 were moved to Introduction. P3 L6-L20 were deleted.***

L6-7: “Observations from satellites confirm that energetic particle precipitation changes the NO_x amount in the atmosphere.” - ***deleted***

L8: “: : from satellite measurements during the years 1992: :” - ***deleted***

L10: ECHAM5/MESSy is the same as the EMAC model, EMAC stands for - ***deleted***

“ECHAM5/MESSy Atmospheric Chemistry” i.e. 2 of the studies mentioned in the Discussion are from the same model. - ***deleted***

L11-12: “They calculated thermospheric NO_x fluxes to the mesosphere from precipitation of low-energy electrons using the average annual Ap from 1991 to 2005.” - ***deleted***

L12-14: “These average annual NO_x concentrations were based on the UARS/HALOE measurements reported by Randall et al., (2007).” The NO_x model of Baumgaertner et al. was based on the Randall et al. measurements, they were then compared with independent observations by the MIPAS instrument onboard Envisat as reported by Funke et al. (2005) (see reference in Baumgaertner et al., 2009). – ***deleted***

L15-16: I think what you should say is that that the authors of that paper demonstrated that Ap works as a good proxy for low-energy produced NO_x. That particular paper does not handle HO_x at all. But there are others which show the direct impact of electron precipitation on HO_x, for example: Andersson, M. E., P. T. Verronen, S. Wang, C. J. Rodger, M. A. Clilverd, and B. R. Carson (2012), Precipitating radiation belt electrons and enhancements of mesospheric hydroxyl during 2004–2009, J. Geophys. Res., 117, D09304, doi:10.1029/2011JD017246. ***Yes, this particular paper does not process HO_x at all. We used the rotation temperature OH (6-2) as a proxy of the neutral temperature of the atmosphere. The auroral atomic oxygen line is superimposed on the OH spectrum so that it is impossible to correctly calculate the rotational temperature. Thus, the spectra obtained during the aurora were excluded from the study.***

L17: They demonstrated both mesospheric and stratospheric ozone changes. – ***deleted***.

L21: “There is a publication series: :” change to “There are several publications: :” – ***corrected***.

L27-30: A similar downwards descending signal (in the same model) is already demonstrated by Baumgaertner et al (2011) using Geopotential height anomalies. ***This sentence is included in the text of Discussion.***

L30: “moves” - ***corrected***

L32-33: Not all models are limited to this altitude range, but many reanalysis datasets are limited to altitudes below the stratopause. Models have issues in comprehensive inclusion of EPP. ***This sentence is rewritten as: “It should be noted that model and experimental researches of meteorological parameters are limited to a height below ~80 km.”***

L34: “Therefore, warming in our measurements has to be detected earlier.” I don’t understand why this would have to be the case. These temperature signals can be completely independent. That doesn’t mean they would not be linked to geomagnetic activity or EPP. ***The phrase “Nevertheless, it cannot be ruled out that the temperature rise of the upper mesosphere in geomagnetic active years is due to the in situ effect of EPP” is incorporate to the Conclusion.***

Page 3 L16-17: “Warming signal moves down from high altitude to low one.” And most of the last paragraph. This is not necessarily the case and certainly this is not a conclusion you can make based on

the present study. Several of the publications you have cited actually argue that the stratosphere temperature signals are driven by changes in dynamics and are not related to in situ changes in ozone.

Figure 1: It is very difficult to tell the two lines apart, I would suggest making the plot in colour or applying an offset to separate the lines. Additional typos and general language comments - “Energetic particle precipitation” (and the abbreviation EPP) is the generally used term. It is singular, therefore “EPP is: : :”. Be careful not to use “energetic particles precipitation” or “particles precipitation”, or “precipitations”. **Figure 1 and Figure 4 are colored. Abbreviation EPP is used.**

- NO_x and HO_x - the x is a subscript – **corrected**.

- The commonly used terms for both are NO_x = “Odd Nitrogen”, HO_x = “Odd hydrogen” instead of “nitrogen oxides” etc. – **corrected**.

- The author with two papers in the citation list is “Seppälä”, the name is correct in the citation list but incorrect in the text. – **corrected**.

Anonymous Referee #2

We are grateful for the thorough analysis of the manuscript and for the helpful and constructive comments.

The reviewer comments are given in normal typeface, *our responses are italicized and bold*.

Responses:

Major finding: "... The difference is about 10 K (i.e. $10.5\text{K}\pm 1.4\text{K}$, or $9.6\text{K}\pm 1.4\text{K}$, if Feb is included, according to what I have "measured" and calculated based on figure 4), but the text mentions 10 K only in the abstract and in the Conclusion (Page4, Line13), but not when figure 4 is explained...".

Thank you for your "measurement". We include it in figure 4 explanation.

I still find the treatment of the existing literature in the discussion section too long; it is nearly a review, although by no means complete (compared to the additional literature cited in the recent and somewhat related paper by Yi et al., 2017). I think that at least part of this literature overview should go to the introduction, while skipping some of the details of how the literature results were obtained. At any rate, some improvement in structure (like subtitles for the different sub-topics, temperature effect from decrease of ozone radiative cooling - stratospheric warming - direct particle precipitation effects on temperature) would also be helpful. While the focus is on density (with temperature only an auxiliary parameter), it cites many papers about solar activity effects via Joule and particle heating, and about geomagnetic forcing on ozone (none of which are mentioned in the present paper), stating that the expected temperature and density impact has "never been found"

Response: As we understood, the main shortcoming of the article is its structure. In the revised article, we tried to re-write the introduction and discussion according your remarks. Part of the discussion concerning an indirect effect on the atmosphere from particle penetration was significantly reduced and moved to the introduction. In the introduction and discussion, the main attention is paid to the response of the temperature of the upper mesosphere / lower thermosphere to geomagnetic activity. The references describing the direct effect of geomagnetic activity on the temperature of the upper atmosphere have been added to the introduction [Burns et al., 2014, Xu et al., 2013, Chang et al., (2009), and Jiang et al., (2014)].

Minor details:

Page1, Line32: add after "activity", ", the" -> "...measure of geomagnetic activity, the widely available Ap index..." ["index Ap" sounds as if its name were not well-known]. Since "index" is latin, the plural "indices" should be used (same line). ***The proposed changes are made.***

Fig. 1: the overlap between the F10.7 and Ap curves makes it not easy to read. Shifting the zero point for F10.7 upwards would help. ***The figure is changed to the colour plot.***

Page2, Line31: Missing "The" before "first group"; (same issue next sentence). ***Corrections are made.***

Line34: "approximately the similar" -> "approximately similar" (or "approximately the same"); change to read "geomagnetically active years" [an adverb, not an adjective].

Corrections are made.

Fig. 4: the ticks on the time axis seem to be the beginning of each month; to make this easier to see, the labels should be centered between these ticks. On the other hand, the temporal positions of the Aug, Sep, and May data points look as if there was something wrong (not centered near mid-month). ***The figure 4 is corrected.***

Page3, Line32: This reference to the result given in the previous section (temperature enhancement due to

geomagnetic activity) should be formulated so that it does not sound like news, here. Also, the emission height of OH has been mentioned before. Here, only the height difference of 7 km matters, so that the previous sentence could continue "...limited to a height of 80 km, which is 7 km below the hydroxyl emission layer". ***The sentences are rewritten.***

Line34: "has to be detected"? The argument is that stratwarm effects are known to propagate downward, so that the OH temperature effect should be expected to occur earlier than model results obtained for 80 km, and below. ***"has to be detected" is changed to your proposed sentence "stratwarm effects are known to propagate downward, so that the OH temperature effect should be expected to occur earlier than model results obtained for 80 km, and below".***

L35, 36: -> "measurements", delete "also", or start sentence with "Also, most of...". ***We changed to "Also, most of ...".***

L40: it would be better to connect both sentences with ", because in order to separate...", because they are related. ***Sentence changed to: "The data of several solar cycles is necessary because to separate correctly the influence of these components."***

Page4, Line13: missing "is" between "mesopause" and "approximately", missing space between "10" and "K". ***Corrected.***

L16: "onset of warming was noticed", better "the average onset of stratospheric warmings is observed (Seppälä et al., 2013)" to be more explicit, and avoid the impression that the timing of stratwarms was unknown, before 2013. ***"onset of warming was noticed" replaced by "the average onset of stratospheric warmings is observed".***

L37: missing "i" in "Gavrilyeva". ***Corrected.***

The occurrences of "Seppälä" (the 2 in P3L22, and the ones in P3L27, P4L7, P4L16) should be spelled correctly, as in P4L32, P5L10, P5L25, P5L27. ***Corrected.***

Page5, Line13: Mies title has "X dublett-Pi", and pages 150-188. ***Corrected.***

Line14,15: "III" is part of the family name, so -> "Russell III, J.M.". But, I ask myself (and the authors), wouldn't Randall, C.E., Harvey, V.L., Siskind, D.E., France, J., Bernath, P.F., Boone, C.D., and Walker, K.A.: NO_x descent in the Arctic middle atmosphere in early 2009, Geophys. Res. Lett. 36, L18811, doi:10.1029/2009GL039706, 2009. Be more pertinent than the Randall et al. 2007 paper, although it's "only" about special conditions in early 2009. (The "x" in NO_x is subscript). ***"The Randall et al. 2007" paper is more pertinent because it was chronologically the first publication.***

Line29: missing "." after "Kallenrode, M.-B". ***Corrected.***

Anonymous Referee #2

The reviewer comments are given in normal typeface, *our responses are italicized and bold.*

In my opinion, the statement "The correlation coefficient is equal 0.51 ± 0.1 at 95% confidence level." (already in the abstract, Page1, Line16, and also Page2, Line30) is not meaningful. In both cases, the "confidence level" may be interpreted to refer to the meaning of the error bar, i.e., that 0.1 be a 2-sigma error bar, but it seems that the authors interpret more into their numbers than this. Namely, in the Conclusions (Page4, Line12), the authors say "Correlation is statistically significant and is equal to 0.51.", the error bar is not mentioned, and so the reader is expected to believe that the correlation coefficient of 0.51 itself "is meaningful".

However, as Aldrich (Aldrich, J. (1995), Correlations genuine and spurious in Pearson and Yule, Statistical Science 10(4), 364-376) explains, "...there would be a correlation of about 0.4 to 0.5 between these indices had the bones been distributed absolutely at random. (Pearson 1897).

The values of "about 0.4 to 0.5" came from a formula that Pearson developed for the correlation of x_1/x_3 and x_2/x_3 when x_1 , x_2 , and x_3 are independent random variables with equal coefficients of variation."

[The Pearson paper mentioned is K. Pearson (1897), On a form of spurious correlation which may arise when indices are used in the measurements of organs, Proc. Roy. Soc. London Ser. A, 60, 489-498, but according to the Proc.Roy.Soc. website, the correct year is 1896, not 1897]

In my opinion, this means that the level of correlation between A_p and OH temperature is well in the range of what statisticians have called "spurious", and by itself not a clear indication of a "real" effect. Only by geophysical arguments (as those which the authors do bring up) can the concept of a real relationship between geomag activity and mesopause region temperature be based.

Response:

Thank you for noticing the inaccuracy in the description of the correlation and pointing to it. The correlation coefficient of TOH and A_p -index is 0.51. The significance of correlation coefficient was tested with 14 degrees of freedom T-test. The critical value of correlation coefficient is 0.46 at the 0.05 level of significance. TOH is not correlate with F10.7, because correlation coefficient 0.36 is less than critical value. The correlation coefficient increases to 0.65 when F10.7 leads the temperature by 2 years. We agree with you that "... correlation by itself not a clear indication of a "real" effect." Therefore, we attempted to present in the article the results of a study of the effect of geomagnetic activity on the temperature of the upper mesosphere made by other researchers.