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Interactive comment on "On the relationship of energetic particle precipitation and mesopause temperature" *by* Florine Enengl et al.

Anonymous Referee #2

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General comments: The paper has attempted to reveal an effect of the energetic particle precipitation (EPP) to polar mesopause region by analyzing large data set obtained from EISCAT radar and collocated OH airglow spectrometer in Longyearbyen. Authors has selected 8 EPP events which reach mesopause region (i.e. OH layer) by setting criteria in a temporal variation of electron density. As a result of a comparison between OH rotational temperature (T_OH) and electron density at mesopause, they show decrease in T_OH during onset of the EPP event in most cases (7 events of 8). They discussed the cause of this decreasing and concluded that EPP eroded upper part of exited OH (OH*) layer and modulated vertical profiles of OH airglow which can change ground observed T_OH since T_OH is an averaged temperature of neutral atmosphere over the OH* layer. This mechanism has been already suggested by Suzuki et al. [2010] based on the single EPP events observed in Antarctica. Suzuki et al. [2010] shows increase in T_OH during EPP event and inconsistent with results of the present study. However, substantial effect on OH airglow layer by EPP is modulation OH* profile. Thus, it is possible to observe either increase, decrease, or no change of T_OH during EPP since it depends on a vertical gradient of atmospheric temperature over OH layer. Authors insist that present study support previous studies but showing new feature in variation of T_OH during EPP in polar mesopause region (decrease in T_OH during EPP).

The reviewer evaluates and agrees with the objective and motivation of this study. Quality of data set and analysis direction is also fine. However, the reviewer does not think the current version of the manuscript merits the publication because of lack of substantial verifications to their results and analysis. Major concerns for the reviewer are described below.

1. Discussion and further analysis focusing on variation in OH intensity are required. The authors mainly show variation in T_OH before, during, and after EPP. However, they do not show quantitative verification for intensity of OH airglow (I_OH) before, during, and after EPP as well. Since modulation of height profile of OH* airglow is an essential phenomenon to explain the observed T_OH, the authors have to show more detail and quantitative verifications for observed I_OH. For example, relative amplitude of decrease in I_OH during EPP is necessary to be quantitatively addressed. And then the amplitude has to be verified whether it is enough to change the T_OH with observed level. Empirically Modeled or observed background atmospheric temperature profile and typical profile of OH* intensity would be necessary for this verification. For background temperature profile, satellite data (MLS/AURA or SABER/TIMED) are best to be hired. If coincide temperature profile data are difficult to collect on event days, empirical model (e.g. CIRA) is another choice to know the typical background temperature profile. Anyway, the reviewer strongly recommends authors to check the typical temperature gradient during events weather observed decrease in I_OH can reproduce

observed T_OH.

2. Insufficient discussions to explain the observed variations in T_OH. As the authors mentioned in the manuscript, atmospheric parameters are highly variable in polar mesopause mainly due to existence of many kinds of atmospheric waves. In particular, small scale ($\sim$10 – 100 km) atmospheric gravity wave is known to be major source causing large fluctuations with a period of hours to minutes. Authors excluded this possibility since the correlation between observed I_OH and T_OH is poor and amplitude of T_OH is greater than 10 K for all cases. Nevertheless, the authors also say that decrease in I_OH is shown in most cases (L260). In addition, authors also say that 'While a positive correlation can be seen between the two parameters in case of the fourth and fifth event, no significant correlation across the entire event set was found (data not shown).' (L243). Thus, it seems little bit inconsistent in their context explaining a relationship between T_OH and I_OH. Thus, the reviewer recommends the authors to re-organize their discussion about relationship between T_OH and I_OH. As the reviewer already pointed in former comments, authors must show more details for observed I_OH during EPP events. The amplitude over 10 K is possible and often seen in T_OH due to atmospheric gravity waves in a polar mesopause region. The phase between I_OH and T_OH is roughly positive but can shift each other depending on a vertical wavelength, damping factor, and a sign of vertical wavenumber of atmospheric gravity waves [Liu and Swenson, 2003]. Thus, authors should discuss more carefully to evaluate and exclude the effect of atmospheric gravity waves. For example, in a first event (29 Dec, 2007), there seems large fluctuation with period of 2-hours over the night in both T_OH and I_OH. In this case, phase of T_OH seems to lead the I_OH. This kind of feature is very common and typically observed in variation of T_OH even on no EPP days [e.g. Suzuki et al. EPS., 2010]. https://earth-planets-space.springeropen.com/articles/10.5047/eps.2010.07.010

3. Lack of verification on auroral contamination to OH spectrum data. During the night with active EPP, bright aurora feature would covers entire sky in typical. Since Minel

OH(6-2) band sits on wavelength between 825 nm and 860 nm, strong contamination from aurora light (including strong OI line at 844.6 nm) can disturb OH spectrum. Since T_OH is very sensitive to relative intensity of P lines, authors should address the how they judge the spectrum data is free from auroral contamination. The authors mentioned about accuracy of T_OH observation in section 2.2 as +/- 2 K. However, data shown in Fig 1 and Fig 3 have much larger error than this. The authors also should clarify about this point.

Minor commnets:

Fig 1. Include a plot of I_OH as well as Fig 3. Fig 1. Include a vertical line to show the onset time in the plot. Table 1. Add uncertainties in each value. L251 Reference Maeda [1967] is old. The reviewer suggests to add a recent paper modeling O3 destruction during EPP events. (e.g. https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016JD025015%4010.1002/%28ISSN%292169-9402.EEL15)

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