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Comment on angeo-2022-16

Anonymous Referee #1

Referee comment on "Modulation of polar mesospheric summer echoes (PMSEs) with high-frequency heating during low solar illumination" by Tinna L. Gunnarsdottir et al., Ann. Geophys. Discuss., <https://doi.org/10.5194/angeo-2022-16-RC1>, 2022

This paper provides an analysis of PMSE modulation by HF heating. It presents data from several days under varying background ionospheric conditions and analyses the response of the different PMSE layers to heating, including a comparison with simulation to explain the size of the overshoot.

There is some nice, thorough analysis and definitely merit in this work. However, there are also some major issues with the submission, mostly in the way that the paper is presented. The comments I provide below are given in the spirit of trying to improve the manuscript.

The main subject:

The first major issue is that it is not at all clear what the main point of the paper is. The title is very generic and gives no clue about what the new/noteworthy and exciting science is. Many papers have been written on HF modulation of PMSE (as the authors show in their introduction), why and what does this one add to the current state of knowledge?

I think that the main takeaway is that this is the first time that analysis has been performed on modulated PMSE under relatively low solar illumination, at late local times and at the end of the traditional PMSE season. The reference to this in the abstract (and how it may affect the observations) is very vague (line 15): "Some individual curves however, show a stronger overshoot than observed in previous studies. A possible explanation for this difference can lie in the dust charging conditions that are different during the night or other conditions might be at play." Essentially you offer a suggestion

and then undermine it by saying it might be something else, but without saying what the something else might be.

I highly recommend changing the title to better reflect the main aim/results of the paper (e.g., "Artificial modulation of PMSE during low solar illumination"). I recommend highlighting the unique aspects of the work in the abstract, introduction and discussion. Make it clear why this is different to previous work, or what it does to build on and support it.

In connection to this, the authors often digress into discussions of sporadic E (it is even mentioned in the abstract). This paper is about PMSE and I recommend removing the discussion and analysis of sporadic E since it is a distraction to the main topic (see line 173, lines 232 onwards). You highlight the E layers as an interval of interest in figure 3c and discuss it as Area 3 in Observations 3 (line 195). Please remove this, and simply state that "the layers above 90 km after 22:45 UT are not PMSE, they are examples of Sporadic E" perhaps with a reference. I understand the temptation to discuss interesting features, but it only detracts from the main focus; if there is something noteworthy about this interval, I recommend writing a separate article on it.

The style:

The manuscript reads often reads more like a student report rather than a journal article, a clear example of which is the inclusion of a table of contents. The submission includes a sizeable appendix, which is effectively the same size as the main body of the article. I leave it to the editor to determine whether the appendix is appropriate.

The submission requires significant proof reading for some spelling but mostly for grammar, with attention on the appropriate use of articles ('the', 'a'). I do recognise that English is not the easiest language when it comes to this issue and that it can be far from intuitive, but I note that the paper has experienced authors and at least one who has English as a first language and so I am sure they can be helpful.

Additional Comments

Line 61 you give a range of temperatures (130 – 150 K) and then say, 'and lower'. Why not say "150 K and lower".

Line 69: You state that "The charges and sizes of the dust particles are largely unknown". Is this really true or is it more nuanced than that? Lots of work has been done by Havnes (for example - Havnes, O. and Kassa, M.: On the sizes and observable effects of dust particles in polar mesospheric winter echoes, *J. Geophys. Res.*, 114, D09209, doi:10.1029/2008JD011276, 2009.) on examining dust sizes, others have also done a lot (e.g., Rapp, Scales, etc)

Line 71: 'shown' instead of 'suggested'

Line 130 onwards: you describe the observations taking place under dusk and night conditions. This gives the initial impression of some of the observations taking place in darkness, but are they really? You go onto discuss the reduction in solar illumination of an order of magnitude compared to midday in mid-Summer, but I am not sure if a reader would have a feel for what this really means. To put it in context, how does the range of illumination vary through the year (lowest to highest) – where on the range do these observations sit? Did you consider including a sentence on the solar zenith angle (just the average if it doesn't vary much) at the times of the observations? This is a measure that I suspect the readers may be more familiar with and are more likely to intuitively understand the difference in degrees between mid-summer and these observations.

Given that the level of illumination is potentially the major selling point of the paper, it would be wise to make sure the reader understood how unique this might be in terms of past observations.

Line 138: Is it possible to give an estimate of the change in the photo-emission current? Or indicate that is something that you will consider in the analysis. Saying it 'should translate...' without providing more detail seems very vague at this stage.

Line 139 – 144: this section seems to describe observations that were mostly irrelevant. Except for the later comment about lack of NLC limiting likelihood of dust size I would have recommended removing this. This could be shortened to: "weather conditions were poor for measuring NLC, though cameras were operating at Kiruna and Nikkaluokta (~200km south of Tromsø) to allow triangulation. During 15/16 August, faint NLC were visible from Kiruna, close to the horizon, but westward of the EISCAT site (closer to Andøya)"

I do have a question about this: how confident are you that the lack of NLC above Tromsø was not due to low clouds in the line of site of the camera? This is quite important since your dismissal of larger particle sizes later on relies on this.

Figure 2 and lines 145 onwards: these measurements have been selected as they are closest to the radar measurements at the time of the PMSE observations. This is unacceptably vague, how far away were they? 1 degree of latitude, 2, 10? It is important to give the right context for the reader to judge.

Lines 153-160: I thought it might be useful to include an example of some of the grammatical issues that occur throughout the paper, along with an indication of some alternate wording and removal of over-precise measurements.

“The radar observations were made in **the** zenith direction with the EISCAT VHF (224 MHz) antennas **radar** near Tromsø (69.59°N, 19.23°E). **The VHF and Heating parameters are given in Table 1. Details on the radar experiments were provided by the EISCAT documentation (Tjulin, 2022*).** EISCAT Heating (Rietveld et al., 1993, 2016) was operated with a vertical beam at 5.423 MHz with a nominal 80 kW per transmitter, which corresponds to Effective Radiated Power (ERP) between 500 and 580 MW. and X-mode polarization was used with **HF pumping in** a sequence of 48 s on and 168 s off. **The beam width of the Heating facility (7°) is much larger than that of the VHF radar (~4°) and given** that the vertical winds and velocity fluctuations of the PMSE are only a few m/s and horizontal winds possibly upto a few 10s m/s (Strelnikova and Rapp, 2011), the radar is always measuring PMSE influenced by the heating.”

*include the documentation as a reference, but make sure you pick the relevant edition for the year of the experiments.

Line 167: remove “we consider the back-scattered power in units...” since you have effectively said this on line 163.

Line 185 onwards: I am curious about the early part of this observation, it is very hard to see because of the scales, but even the weak precipitation from 2000 – 2110 UT seem to shows signs of modulation at higher altitudes. I know that precipitation can show distinct natural modulation that leads to regular Ne patterns, but I wonder if this is a problem with the analysis not fully taking into account that the system power is fluctuating as the heater is turned on and off.

I would also be wary about suggesting the PMSE in (1) ‘seems that it was triggered by the heating’, beyond the coincidence of timing is there a reason for this speculation?

Power modulation might also be visible in Observation 3 around 21 UT. Is it possible that this might also be responsible for the modulation of the sporadic E later in that interval, there are hints the striations continue to higher altitudes? I recommend talking to your co-authors Reitveld and Haggstrom to double check whether this might be the case.

Line 220 onwards: if these figures are important enough to discuss in the main text, why are they not included in the main body of the paper?

Line 223: why was this value selected? You discuss the threshold is perhaps too high, in which case why did you not try the analysis with a different threshold and publish that?

Line 262 and elsewhere: when you refer to particular data that you have presented, you should refer to that figure (i.e. fig3c)

Lines 388 onwards: I found the description a bit confusing. When you say that the 'simulations assume an initial plasma temp...' do you mean that you set that as the initial temperature based on the IRI value for that time? Or did you mean that the simulations are hard wired with that temperature which thankfully happened to match what IRI would expect? I assume not the latter but that is sort of how it reads. Also, how confident are you that the IRI temperature is itself reasonable. Did you look at the EISCAT temperatures for heater-off times around the PMSE region to get an estimate for comparison?

How did you determine that 3nm dust particles were the best fit – did you judge it by eye, or did you fit the model to the data by altering the dust size until some measure of best fit was achieved?

Figure 16. Why is the X-axis scale different between the data and the simulation?

Line 472 onwards: is there a way to explore how much of an effect absorption of the heater beam might play? The way this is written you seem to be suggesting that you have an answer for the large overshoot in interval A, but that other effects may be important and perhaps even fully explain the large overshoot, thus negating your initial statement. It is really good to consider other factors and suggest alternative explanations, however it is better if you can provide an indication of how confident you are in your explanation and why. As it stands it sort of reads as if you are saying: "analysis suggests that X can explain the overshoot effect, but it could also be explained by Y instead". Stating uncertainties and explaining the limits of analysis are of course very good, but at the

same time you can make it clearer how confident you are in the analysis.

“we know that Y potentially contributes to the effect at an unquantified level; however our analysis shows that N% of the effect is explained by X”

I was surprised that there is no reference to:

Havnes, O., Pinedo, H., La Hoz, C., Senior, A., Hartquist, T. W., Rietveld, M. T., and Kosch, M. J.: A comparison of overshoot modelling with observations of polar mesospheric summer echoes at radar frequencies of 56 and 224 MHz, *Ann. Geophys.*, 33, 737–747, <https://doi.org/10.5194/angeo-33-737-2015>, 2015.

who also found varying overshoot for different conditions (albeit with different radar frequencies as well).

Summary

There is some good analysis and interesting data in this submission. Some work is required to bring the level of presentation up to standard and in doing so highlight the important aspects of the work. The title needs rethinking, and the key results need to be better highlighted in the abstract, introduction and discussion to make them stand out more. I think its important to check that the guisdap analysis has properly taken the system power changes into account in each instance.

I am confident that the authors can address these issues and produce a good paper.