Response to Reviewer 1

We thank the reviewer for the detailed and constructive feedback, as well as for the positive assessment of our work. We appreciate the suggestions and questions, which have been highly valuable in improving the clarity and quality of the manuscript.

Overall, this is a very strong and impressive study, and I would like to emphasize the quality of both the dataset and the modelling effort. The manuscript provides a compelling framework to interpret complex auroral spectra, and the clarification that the "continuum" corresponds to a blend of band systems rather than a true blackbody is particularly useful, and offers a strong alternative to the NO continuum.

That said, several aspects of the interpretation would benefit from clearer justification and consolidation, especially regarding the physical conditions required for GHOST emissions.

Section 3 – interpretation of Event 1 and the definition of "continuum"

I compared the discussion of Event 1 from Partamies et al. (2025) with my own reading of their Figure 4, and I think this part would benefit from a more explicit comparison in the manuscript. The spectrum appears to show both: 1. Strong red emissions (630/636 nm), suggesting a significant contribution from low-energy electrons, and 2. clear N_2^+ features (e.g. 427.8 nm, 470.9 nm), indicating that higher-energy electrons are also involved. A straightforward interpretation would therefore be a mixture of low- and high-energy electron precipitation. However, this does not immediately make it obvious that the spectrum should also be understood as a blend of multiple molecular band systems (e.g. N_2 IRA, N_2^+, etc.), as discussed later in the manuscript. From a quick view, the reader may naturally interpret the spectrum in terms of electron energy distributions, and not grasping yet the idea about the role of the different molecular contributions in shaping the continuum-like appearance. It would therefore help to guide the reader more explicitly on this point, for instance by briefly revisiting this event (with the ASC image and spectrum) and clarifying how one moves from the "low/high energy mixture" interpretation to the "continuum as a blend of band systems".

We will add the MISS spectrum for Event 1 and discuss the continuum spectrum more explicitly. We will also introduce the continuum more clearly so it is clear that we are referring to the enhanced broadband background. The GHOST spectrum includes both typical electron precipitation signatures and a raised continuum background; we will clarify in the text that this is the part of the spectrum we are focused on investigating.

Section 4

Lines 144–146: the purpose of this paragraph is not entirely clear. Is Figure 3 intended to show the Hα emission in both the GHOST and background spectra? This could be clarified.

Figure 3 is primarily intended to evidence the GHOST spectrum in Event 3, the Halpha emission itself is not the purpose. We will change the phrasing from ‘Halpha HiTIES spectra’ to ‘HiTIES spectra in the Halpha wavelength range’ to clarify this. We shall also make an updated figure showing the locations of the NO2 emission lines we have checked to evidence the claim in Lines 144-146. We will add text in the introduction to make clear that the chemiluminescent reaction of NO and O produces NO2, so large amounts of NO continuum would necessarily produce high concentrations of NO2 (Harding et al., 2020).

We will edit the paragraph as so;

To assess the NO mechanism proposed by Gillies et al. (2019), Harding et al. (2020) etc. and as discussed in Section 1, we compare our observations with the high resolution spectra of NO2 as shown in Smalley et al. (1975) at very low temperatures. The NO2 spectrum does not match to any features in the HITIES spectra shown in Figure 3, or compared with previous high-resolution spectra of GHOST reported in Partamies et al. (2025). The lack of NO2 indicates that the NO mechanism presented in Section 1 is not responsible for the continuum emission we observe.

Figure 3: the Hα rest wavelength at 656.3 nm is quoted for air. Does this mean that the full spectrometer calibration is performed using the air wavelength convention?

Yes, the measurements are made in air and we do the calibration in air, we calibrate the spectrograph using OH airglow lines. We use the vacuum to air wavelength conversion from Ciddor (1996). We will add this information and reference to the instrumentation section.

Section 4.1

The authors may consider adding, perhaps in an appendix, the complete list of wavelengths/species/band included in the modelling. This would be valuable as a reference for future work.

We will add the list of emission lines as a file in the supplementary materials (due to its large size).

Section 4.3 – modelling results and interpretation

First, I would like to note that the fitting results are excellent, especially given the large number of free parameters involved. That said, I still have several questions regarding the interpretation.

Line 242: it would be helpful to include an ASC image from these events, in order to compare them directly with Figure 1. The manuscript refers here to a “normal aurora”, and a visual comparison would be useful.

We will add four ASC images from these events with a mark showing the location of magnetic zenith and discuss in the text.

Figure 5 (top), GHOST spectrum: I would strongly suggest removing the N_2 VK curve from the plot. It took me some time to distinguish it from the N_2 1P contribution, and since N_2 VK does not seem to contribute significantly here (unless I misunderstood), its inclusion may unnecessarily complicate the figure. By contrast, the authors emphasize that N_2^+ M is important for shaping the spectrum. It would therefore help the reader if some specific spectral examples were pointed out explicitly, for example around 737.0, 739.3, and 740.1 nm.

We will remove the VK line from the plots and add a note in the text explaining that the VK contribution was too minor to include. We will add specific wavelengths to the text as examples so as not to overcomplicate the figure.

Figure 5 (bottom): the N_2 IRA contribution has indeed increased compared with Figure 4 (bottom), especially above 732 nm, although it already seemed to be present at somewhat shorter wavelengths in Figure 4 as well. I am less convinced, however, by the interpretation of the role of N_2^+ M in distinguishing Figure 5 from Figure 4, since this component also appeared important in Figure 4 (bottom).

We remark on the contribution of N2 IRA and N2+M in Fig 5 (bottom) as it is much stronger at the longer wavelengths than in Figure 4 (bottom) though yes, it is present in both.

More generally, I am not entirely convinced that the spectrum in Figure 5 (bottom) represents a clear transition stage between GHOST and “normal” aurora. To my eye, it still looks rather similar to Figure 4 (bottom). Since the N_2 1P bands are less pronounced and likely affected by lower SNR, is it possible that the fitting procedure compensates by assigning a stronger contribution to N_2 IRA and N_2^+ M?

At the shorter wavelengths N2 IRA and N2+ M are improving the fit so we don’t think this is a compensation. In Figure 4 (bottom) there is also some contribution from these band systems so at this stage we cannot be sure how “normal” the observations in Fig 5 bottom are. Future work will investigate weaker potential GHOST conditions in more detail. We will improve the description of Fig 5 (bottom) in line with this.

Line 260: when referring to “the continuum fit”, do the authors mean the spectrum shown in Figure 5 (top), i.e. the GHOST case? This should be stated unambiguously.

Yes, we refer to Figure 5 (top), we will ensure this is clarified in the text.

Lines 264–267: I found this part difficult to follow. My understanding is that the best fit for Figure 5 (top) was obtained with a rotational temperature of 881 K, but the manuscript then states that this is not sufficient to thermally excite N_2 IRA. Yet the model still includes some N_2 IRA signal in both the top and bottom panels of Figure 5. Does this imply that another mechanism is contributing? Please elaborate.

Yes, we think so. We will rewrite these sentences to clarify as so:

These results indicate that the continuum emission is linked to strong heating. As 881 K is not that unusual in the thermosphere, it alone is therefore unlikely to be enough to thermally excite the N2 IRA. However, it is an extreme temperature for the E-region. The high rotational temperatures generally flatten all band systems, which would make the spectrum more continuum-like at all wavelengths.

These results indicate that the continuum emission is linked to strong heating. As 881 K is not that unusual in the cusp thermosphere (Skjæveland et al., 2017), it alone is therefore unlikely to be enough to thermally excite the N2 IRA (or we would expect it to be detected regularly). Assuming the GHOST emission peaks in the E-region, we are seeing an extreme temperature for this altitude, consistent with the extreme heating associated with GHOST, measured in Partamies et al. (2025). The high rotational temperatures generally flatten all band systems, which would make the spectrum more continuum-like at all wavelengths.

"The high rotational temperatures generally flatten all band systems, which would make the spectrum more continuum-like at all wavelengths". I am not sure to follow. Indeed we should expect a more continuum-like at all wavelengths, but this is not what the figure 5 (top and bottom) are showing, is it?

"The high rotational temperatures generally flatten all band systems, which would make the spectrum appear more continuum-like (uniformly enhanced when observed over a broad wavelength range)"

With the HiTIES instrument we are seeing in very high resolution, so although the spectrum is "flattened" (e.g. enhanced away from the band head of individual vibrational bands), we still see rotational structure with HiTIES. High temperatures will more obviously flatten the spectrum when observed with e.g. MISS. We observe that the addition of more vibrational and rotational bands produce a uniformly elevated continuum in Figure 5 (top) when combined.

Since the authors already present a HITIES spectrum around Hα in Figure 3, could the model also be applied to this spectral range? Would this help constrain the N_2 bands, or perhaps provide additional information about N_2 IRA? Even if it is outside the main scope of the paper, a short comment on this would be interesting.

We didn't fit the Halpha panel as it is noisier and adds additional free parameters without giving additional constraints to the O+ panel, as we fit the intensity of each vibrational band separately. In theory though yes it is possible, but that work is outwith the scope of this paper.

We will add a sentence on this in Section 4.2.

Line 295: could the authors specify the typical electron energies discussed here? I would have expected the IMF orientation to influence primarily the flux and access conditions of magnetosheath electrons, not necessarily their characteristic precipitation energy. Some clarification would be useful.

IMF -Bz increases the energy flux (e.g. Brautigan et al 1991 https://doi.org/10.1029%2F91JA00157), while By dominance changes the distribution of number density at different characteristic energies for different MLTs (Laitinen et al., 2024 https://doi.org/10.1029/2023JA032040). So both energy flux and characteristic electron energy can change with IMF orientation for a certain location. We will rephrase the text to discuss only the energy flux as discussion of different reconnection scenarios at the magnetopause is beyond the scope of this paper. We will rephrase as below;

“..an IMF orientation which leads to an increase in energy flux into the cusp ionosphere (e.g. Brautigan et al., 1991).”

Is the code publicly available, or only available upon request?

Can also the code be used for a different part of the spectral range (not only Ha region) ? If so, did the authors try to degrade the spectral resolution of the synthetic spectra to fit eight MISS or T-Rex spectra, or another instrument? This model is particularly valuable and should probably be interesting for the community.

A full description and code for the synthetic spectra is currently being prepared in a separate article. Currently the model is only tested in the O+ region at 728–743 nm, but we plan to extend this and apply it to other instruments in the future.

Sections 5–6 – mechanism and occurrence of GHOST

The proposed mechanism is physically plausible and well motivated, but the different contributing conditions are somewhat dispersed throughout the text. It would therefore help to summarize these conditions more explicitly, for instance in a table or within the schematic/cartoon already presented, distinguishing the main ingredients (ionospheric priming, dayside activity, sunlit conditions, convection/heating).

Table 3 presents a summary of the conditions present for each of the three events, including the physical appearance of the GHOST, the convection direction, IMF direction, whether we are in the cusp, sunlit conditions and dayside activity. We will add whether there is evidence of heating to the table.

In addition to the convection patterns, the manuscript identifies other important conditions, such as an already energized dayside ionosphere (proton and low-energy electron precipitation) and a sunlit atmosphere enhancing both ionization and resonant scattering (notably for N_2^+).

Taken together, these suggest that GHOST emissions require a combination of conditions. However, each of these ingredients seems, at least individually, relatively common in the cusp. This raises a key question of their actual joint occurrence: are these conditions truly rare when combined, or should one expect GHOST-like emissions to occur more frequently? A short discussion on this point would significantly strengthen the interpretation.

This is a tricky question to answer as the ‘typical’ cusp ionospheric conditions are not particularly well constrained in the literature. This question will be a significant focus of future work, it requires quite a wide analysis of cusp data which is outwith the scope of this study.

Line 343: the statement on ion upwelling driven by low-energy precipitation would benefit from clarification and/or references. It may help to better distinguish between convection-driven heating/upwelling and subsequent excitation by soft electrons.

As the ionospheric cusp region is heated with low-energy electron precipitation, pulsed ionospheric flows, and/or convection-driven ionospheric flow (frictional heating), it causes upwelling of lower ionospheric species, which results in these species moving to altitudes higher than the shadow height (Sadler et al., 2012 https://doi.org/10.1016/j.jastp.2012.03.003) (Lockwood et al., 1988 https://doi.org/10.1029/JA093iA06p056410). Many of these mechanisms may occur at the same time and are difficult to separate as they are often correlated with each other (Glocer and Daldorff, 2022 https://doi.org/10.1029/2022JA030635). We will clarify our statement in the manuscript.

Minor comments:

Line 67: NO instead of NO_2?

They compare NO2 brightness as the NO-continuum mechanism is from the production of NO2. We will add more information about the chemistry for this mechanism in the introduction as described above.

Figure 1: please add the date of the event.

We will do this.

Section 3.3: please clarify the HITIES FoV on the ASC image.

As the HiTIES FoV is very small, we will indicate the pointing direction on the ASC image as the FoV would be difficult to see.

Figure 2 (09:24:01 UT): are the whitish features near zenith expected?

As we don’t have the spectral confirmation for the features near zenith, we have not indicated them as GHOST. The viewing geometry can lead to a superposition of green, red, and blue emissions creating an RGB-white rather than a continuum-white (requiring spectra to differentiate the two cases). As we can’t confidently class this as GHOST we have left it out.