Response to Reviewer 2

We thank the reviewer for the thorough and detailed assessment of our manuscript and for the time invested in providing constructive comments. The recommended clarifications and additional information will be valuable additions to the manuscript.

The paper under review investigates the origin of emissions observed in the poleward boundary dayside aurora that was first reported by Partamies et al. (2025). The authors propose that the emissions arise from highly excited, hot N2. Using spectral modelling and fits to ground-based measurements of high-resolution GHOST spectra, the authors claim that vibrationally and rotationally excited N2 and N2+ can reproduce observed emissions without requiring the presence of NO, as had been suggested in prior published work. Spectral fitting using simulated annealing indicates that GHOST events coincide with extreme ion heating and high neutral temperatures. Background conditions suggest that strong ionospheric flows are typically present and can help provide the necessary environment for producing hot N2 and N2+. The authors conclude that a combination of strong flow, heating, particle precipitation, and cusp conditions produce thermally excited N2 populations that may account for the occurrence of GHOST emissions.

I find the paper to be interesting, well-written, and thorough, in most respects. The figures are certainly interesting and help the reader to understand both the observations and the methodology used to interpret them. The bibliography is mostly complete, though there is prior related work that the authors are either unaware of or may not consider to be relevant. With some additional information and clarifications, I would consider the manuscript suitable for publication. I hope my comments are viewed as helpful and constructive.

Section 1 Introduction

Lines 37-39: The N2 VK and N2+ 1NG (first negative) band systems excited by energetic particle precipitation can produce emission in the wavelength region 502-507 nm. What evidence can the authors provide to support their claim that there is no auroral emission from these band systems for the three events considered in their study?

We have identified the same problem; we note that emissions from the N2 VK, N2 2p, and O2+ 1n band systems are present in that wavelength region (Vallance Jones, 1974). We originally were stating what Spanswick et al. (2024) had presented, but will comment on this directly in the introduction.

Lines 51-55: The language here is imprecise and difficult to interpret. Perhaps the authors mean "…with extreme ion drift velocity and electron temperature in the highest 5% and 3%, respectively, compared with non-STEVE SAID events, corresponding to about 3 km/s and 4000–10000 K, respectively…"

We will update the language used here as you suggest.

Section 4 Spectral identification

This section is quite important as it describes the model used to fit the observed spectra. However, there are missing details that prevent repeatability and, more importantly, are required to support assumptions made in the fitting methodology. For example, the authors state, “We model only the shape of the emission spectrum for each species based on temperature, and do not need to model specific atmospheric conditions since the fit to data leaves the intensities of each molecular vibrational band as free parameters.” First, the atmospheric conditions can only be ignored if such conditions do not affect the shape of the emission spectrum. At lines 213-214 the authors indicate that intensities are corrected to account for water vapour absorption. Presumably this is a wavelength dependent correction. If so, the shape of the emission spectrum is indeed adjusted to account for atmospheric conditions, correct? Second, the authors state they have “checked that the high resolution spectra of NO2…does not show any specific coincidence Hα HITIES spectra…” but they do not show these checks in the manuscript. These comparisons should be included in the manuscript.

Correct, we phrased this badly. We intended to convey that the model we use is purely producing the theoretical spectrum for a set of possible band systems (plus water vapour absorption) and is not modelling for example, the input precipitation, electron transport, or ionospheric chemistry.. We will correct this sentence in the text.

We will mark the positions of the NO2 emission lines on Fig 3 (bottom) to show how the spectrum was checked.

Section 4.1 Synthetic spectra

Line 154: Please explain in the text why the N2 2PG (second positive group) and N2+ 1NG band systems were not included in the spectral modeling.

They were not included as we fit the wavelength range ~728-740nm and N2+1n and N2 2p have no emissions longer than 600 nm.

Section 4.2 Fit to observations

There are some missing details in this section that make repeatability of the methodology difficult, at best. Specifically, the authors use a simulated annealing technique because it can escape local minima to find the global minimum. However, this is not guaranteed. The authors chose limits of 90 iterations or a total of 3000 iterations, and then the whole process is repeated 3 times. What is the justification for these limits (90, 3000, 3)? The authors state that details of the technique are described by Price (2021), but in a different context. How different is the context? Have the authors performed any simulations to demonstrate that they can retrieve the original state of free parameters used to produce the simulated spectra with an appropriate instrument sensitivity (i.e., random variability), along slit calibration differences, atmospheric conditions (i.e., absorption by water vapour), etc.? The authors’ claim that the global minimum can necessarily be found would be more convincing if demonstrated using simulated observations. This is standard practice when using fitting routines to retrieve geophysical parameters.

The simulated annealing technique has been evaluated in detail mathematically (e.g. https://doi.org/10.1126/SCIENCE.220.4598.671) and using auroral spectral data (https://eprints.soton.ac.uk/455066/) in the past, although admittedly not for these specific conditions. We will include a plot of RMS (goodness of fit) against the iteration number to justify the number of iterations used. We do not intend to claim that we find the absolute global minimum, but the final state of the fitting is close enough to the global minimum that we can be confident in our conclusions. Importantly the fitting algorithm does not end stuck in a local minimum far from the true global minimum, in a poorly-fitting state. The fact that the 3 repeats end in similar states adds confidence to the outcome from the fitting.

Line 209: Please include in the text a justification for assuming that the N2+ and O2+ bands have the same rotational temperature. Can the authors cite any laboratory measurements to support their claim?

We are reducing the free parameters by assuming they are the same 'ion temperature.' Typically in the ionosphere (below 500km) it is assumed that ion collisions occur frequently enough that different ion species are driven towards a common, species independent Ti.

Lines 213-214: Are just atomic line intensities corrected to account for water vapour absorption or are all atomic and molecular lines corrected to account for water vapour absorption? Please clarify the text.

Yes, both atomic and molecular, we state in the text; '**All** line intensities are corrected to account for water vapour absorption in the lower atmosphere.'

Lines 217-218: Isn't there a degeneracy between the Gaussian width and the rotational temperature? Did the authors consider determining the width of Gaussians as a preprocessing step to avoid any degeneracy with rotational temperature? Perhaps the simulated annealing process does effectively determine the Gaussian width based on atomic lines, but that would require that the atomic lines are always detectable in the spectra. Please provide additional details that demonstrate there is no degeneracy between Gaussian width and rotational temperature.

HiTIES measures the spectrum at very high resolution such that the rotational temperature is determined from the shape of the spectrum, not a Gaussian width. A Gaussian is applied for each individual rotational line (not a whole vibrational band) and there is no particular degeneracy between rotational temperature and the Gaussian width. There is nothing special about atomic lines compared to molecular lines in this step. We do sometimes measure the Gaussian width by fitting to OH airglow with no aurora, and did originally use that method for the analysis in this paper, but since we realised the aurora may not fill the field of view and there may be slight variation in the width we switched to keeping the width as a free parameter throughout the fitting.

Section 4.3 Results

The quality of the fits to observed spectra reported in this section is difficult to assess since the authors do not provide measurement uncertainties in Figs. 4 & 5. These measurement uncertainties are important since they drive the minimization of the weighted RMS of the residuals. Please add representative error bars to the black plusses in the figures showing observed spectra.

We will add error bars to the spectra plots.

Another issue I have with this section is that the authors do not provide their fitted free parameters for each fitted spectrum. These parameters ultimately provide the basis for the authors' conclusions, but the reader has no access to these values. These values should be provided in tables.

We will provide a table of fitted free parameters in an Appendix.

While the authors briefly discuss the fitted rotational temperatures they obtained, they provide no details on the fitted vibrational populations. These values may provide useful information on the excitation mechanism. For example, how do the fitted vibrational populations compare with theoretical Franck-Condon factors? Are the fitted vibrational populations even physically realistic? Have the authors compared their results with published laboratory measurements of the vibrational populations for any of the important band systems (e.g., N2 1P, N2+ Meinel).

The vibrational populations of N2 are complicated due to cascading between overlapping electronic states. It is possible to solve this cascading in equilibrium conditions, but we are far from certain that that would be a valid assumption in GHOST aurora. In this work, we fit each individual vibrational level as a separate free parameter precisely to avoid making assumptions about the vibrational populations. The free parameters are scaling factors on synthetic spectra and represent intensities of only the lines within our observed wavelength range. It is therefore non-trivial to compare the vibrational populations. For this purpose it is not important, but future work may fit to spectra over a wider wavelength range where the vibrational populations could be investigated.

Lines 252-253: The statement that there is no indication of the presence of NO is dubious since the authors do not include NO2 band systems in their fit. The statement would be more convincing if they included NO2 in their fit and the fit parameters indicate no detectable presence of NO.

We will include the NO2 emission wavelength locations in Figure 3 (bottom), to show clearly the comparison. We have not included NO2 in the modelling as we do not have a NO2 synthetic spectrum model.

Lines 254-257: Why do the authors conclude that the increased contribution from N2 IRA and N2+ Meinel could be "continuum-like" emission when they acknowledge that “normal” N2 aurora is present and N2 IRA and N2+ Meinel band systems are known to be produced in “normal” N2 aurora (Cartwright et al., 1975; doi.org/10.1029/JA080i004p00651; Gattinger and Jones, 1974; doi.org/10.1139/p74-305).

In ‘normal’ aurora we don’t usually see as strong a contribution from N2+ M and N2 IRA within the HiTIES wavelength range - in Fig 5 (bottom) these are unusually enhanced compared to ‘normal’ aurora in Fig 4. We are suggesting that GHOST is not a completely different thing to ‘normal’ aurora, but with enough ‘abnormal’ enhancement of ‘normal’ band systems at high rotational temperatures the spectrum can appear flat and continuum-like.

Lines 258-260: What is the source of the temperature quoted by the authors? Is it from their fits or from uncited published work?

Yes, the temperature comes from the spectral fitting (this will be published as a fit parameter in the Appendix).

Lines 265-267: Are the authors claiming that they observed emission coming from the E-region? If so, how do they know the altitude of the emission?

N2 is typical of E-region aurora, so this is an assumption. The exact altitude of the GHOST aurora is unknown and will be a focus for future work. We will add more discussion of this to the text.

Please provide a figure showing how the structure of the band systems would change with rotational temperature.

We will provide the requested figure as an appendix, one for N2 1p is presented as Fig 4 in Chadney and Whiter, 2018, https://doi.org/10.5194/gi-7-317-2018.

The statement that a high rotational temperature spectrum would be more continuum-like is confusing. A high rotational temperature would indeed make each band appear spectrally wider, but it would not affect the band intensities and therefore the overall “shape” of a band system would be preserved. Changing the rotational temperature is similar to observing a spectrum with two instruments, one with high and one with low spectral resolution.

Yes it’s true that a high rotational temperature won’t change the band intensities, but it does flatten them such that the “low temperature” end is dimmer and the “high temperature tail” is brighter, making them appear wider. Also, since individual vibrational bands overlap, especially for the N2 IRA with many vibrational bands, making them wider does make the overall band system appear more continuum-like.

“The high rotational temperatures generally flatten all band systems, which, together with enhancement of the N2 IRA, would make the spectrum more continuum-like at all wavelengths.”

Section 5 Background conditions

There are several statements in this section relating GHOST event broadness and thickness to favourability of prevailing conditions. How do broadness and thickness relate quantitatively to favourable conditions? What determines the broadness and thickness of the events? Could the broadness and thickness simply be due to observational bias (i.e., geometrical effects)?

We don't know how they relate quantitively, this remains future work and is out of the scope of this paper. The large difference in thickness cannot be explained by geometrical effects.

Section 6 Proposed mechanism

The language used by the authors to describe their proposed mechanism for GHOST events is rather confusing to me. The authors suggest that when precipitation collides with an ionosphere primed with upwelling, heating, and excitation, a continuum made up of electronically and vibrationally excited N2, N+2 and O+2 is produced. Note that precipitation colliding with an ionosphere (primed as described by the authors or not) will produce rovibrational emission if the energy of the particles is higher than the excitation threshold. How is the scenario proposed by the authors truly different from "normal" aurora? Isn't "normal" aurora defined to be a visual manifestation of a continuum made up of electronically and vibrationally excited N2, N2+ and O2+ (i.e., for the wavelength range considered by the authors)? The authors may or may not be aware of results reported by Ellingsen et al. (2021; doi.org/10.5194/angeo-39-849-2021) who analyzed rocket observations of sunlight aurora in the cusp region and emission from N2+ in an environment with ion and neutral upwelling. The relevance and significance of this recent work should be discussed.

GHOST aurora differs from 'normal' aurora in that it produces a continuum spectrum, and the mechanism may require these additional energy sources, but is not completely separate.

We note that Ellingsen et al. (2021) observed similar conditions and will cite this study in our introduction regarding the cusp conditions and upwelling. Based on this single rocket study it is not possible to draw any conclusions about how rare/common these conditions are. Investigating the ASC footage in the supplementary materials, we can see potential white structures, so perhaps it is possible that GHOST emissions appeared during the event, but since the aurora is obscured and the event was before the installation of the MISS instrument it is not possible to confirm either way.

Further studies are required for a complete explanation for the origin of GHOST, comparison of ionospheric conditions for events and non-events will be a significant focus of future work.

Section 7 Conclusions

As noted above, the language used by the authors in their concluding remarks is confusing and needs clarification. It's not entirely clear to me what the authors mean by their statement "The infra-red afterglow and Meinel bands have not been considered in previous Studies…" These emissions (as well as a continuum emission) were certainly considered and discussed in the aforementioned work by Gattinger and Jones (1974). Indeed, I'm surprised that this work was not cited by the authors.

We will clarify this statement to say these bands "have not been considered in previous studies of continuum like emission." We will add a citation to Gattinger and Vallance Jones (1974) in the spectral modelling section.

An additional source of confusion is the authors' statement "We propose that the combination of upwelling, heating, and shear flow provides the energy needed to excite the N2, N2+, and O2+ to produce the observed pseudo-continuum." There is no mention of particle precipitation as a source of energy in this statement. Are the authors suggesting that particle precipitation only plays a role in priming the ionosphere and has no direct role in exciting N2, N2+, and O2+? If this is the argument, and absent the "normal" sources of auroral excitation - energetic particles (possibly with solar illuminations) - how are upwelling, heating, and shear flow able to exceed the excitation threshold of N2, N2+, and O2+ (at least 8 eV) resulting in continuum emission?

"We propose that the combination of upwelling, heating, and shear flow provides the energy needed to prime the N2, N2+, and O2+ to produce the observed pseudo-continuum when hit with indecent precipitation - although further work is required to understand why the N2 IRA is enhanced."

Minor comments:

Lines 93-94, 121: Please provide transitions and wavelengths for the emissions for the benefit of readers who are not familiar with these details.

We will add these details.

Figures 4 & 5: Discrimination of the colors in the figures will be difficult for color blind people. The use of symbols or different line styles would make the figures much easier to interpret.

We have checked the plot with several online tools and a colourblind colleague (who has combined protanopia and tritanopia) and the high contrast of lines allows the plot to be easily read after the N2 VK is removed (as it is close in colour value to N2 1P) . Since the lines are overlapping and closely packed, different line styles and symbols could make the plot more difficult to read, so we would prefer to keep the original.