We thank the referee for the careful and constructive review and for the positive assessment of the novelty and clarity of the manuscript. We have revised the manuscript to clarify the limitations of the wide-field single-station imaging, to quantify the sensitivity of the patch-size statistics to the detection threshold and viewing geometry, and to add broader geophysical context for the event. Below we respond to each comment individually.

Major comment 1:
The line-of-sight and fisheye lens distortions need more careful consideration. While the authors provide reasonable mentioning at a qualitative level, more quantitative estimates of projection effects (particularly far from zenith) would strengthen the conclusions. Points to consider are mapping distortions vs. elevation angle, uncertainties in patch size and cluster boundaries, and systematic biases due to variable emission altitudes.

Response:
We agree. In the revised manuscript, we have clarified that all reported spatial scales are apparent mapped distances at an assumed emission altitude, and that they should not be interpreted as uniquely reconstructed true horizontal scales because the observations were made with a single camera (Lines 93–95). We also expanded the methodological discussion of the keogram-based patch-size estimates, including the limitations caused by one-dimensional slicing and off-zenith line-of-sight mixing (Lines 157–165).

To quantify the possible effect of finite emission thickness, we added a simple geometrical estimate in Appendix C. If an auroral emission layer has an effective vertical thickness H and is viewed at elevation angle e, the apparent horizontal smearing caused by projection is approximately $H / \tan(e)$. For example, for H = 10 km, the projected smearing is 5.77 km at e = 60°, 10.00 km at e = 45°, 17.32 km at e = 30°, and 27.47 km at e = 20°. This shows that apparent patch sizes and cluster boundaries can be substantially overestimated at low elevation angles.

We also examined the elevation angles of the 23,704 detections used in the Figure 6 analysis. Most detections were obtained at relatively high elevation: 22,956 detections (96.8%) were above 45°, 17,075 detections (72.0%) were above 60°, 748 detections (3.2%) were between 30° and 45°, and no detections were below 30°. We repeated the frequency–size analysis using only the higher-elevation subsets. The Pearson correlation coefficients between inverse patch length and peak frequency were 0.002 for all detections, −0.004 for elevation >45°, and −0.020 for elevation >60°, respectively. Figure R1 shows the corresponding occurrence maps for all detections and for the elevation-filtered subsets. The overall frequency distribution remains similar after excluding lower-elevation detections, confirming that the absence of a robust frequency–size relation is not controlled by low-elevation projection effects. Thus, although projection effects affect the absolute apparent patch sizes and cluster boundaries, especially away from zenith, they do not alter the main statistical conclusion.

As an additional conservative test, we repeated the analysis using only high-elevation detections ($e > 60°$) and applied a patch-by-patch correction to the measured length, $L_{corr} = L_{obs} - H/\tan(e)$, assuming $H = 10$ km. Even after this elevation filtering and finite-thickness correction, the mean peak frequency did not show a robust dependence on inverse apparent patch length. These checks have been added to Appendix C (Lines 458–501).

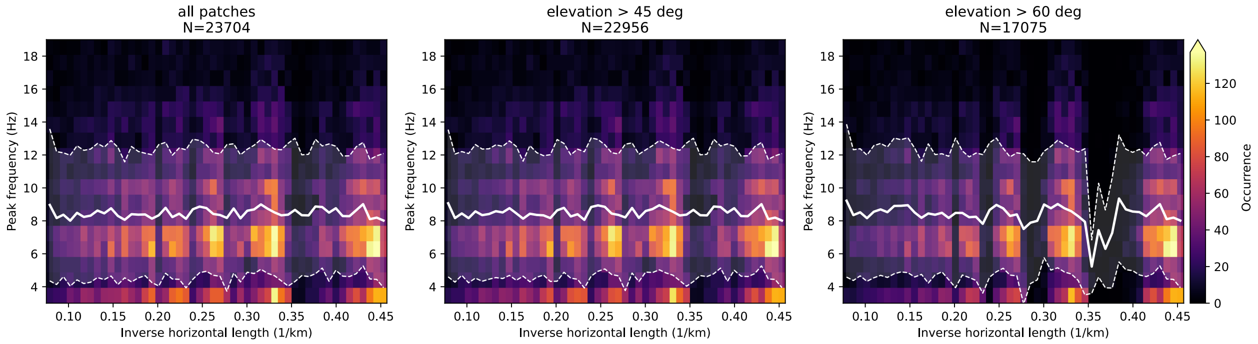

Figure R1: Occurrence maps for all detections and for elevation-filtered subsets.

Major comment 2

As the authors point out, the patch detection via keograms may potentially bias the size statistics. So it remains unclear if the authors have analyzed how their patch detection criteria may influence their results (e.g. with different count thresholds or vertical extent criteria)? How sensitive is the patch-size distribution to the threshold for ΔCount? Did the authors try different values than the threshold of 30, or why was this value chosen? Have the authors looked at how often the keogram slice misses the true center of a patch? A more careful analysis would further strengthen the significance of the conclusions.

Response:

We agree that the keogram-based patch detection introduces important limitations. We chose ΔCount > 30 because a relatively high threshold reduces false detections from background fluctuations and weak non-flickering brightness variations. We have clarified this rationale in the revised manuscript (Lines 176–180).

To test whether the statistical result depends on this choice, we repeated the full patch-detection and frequency–size analysis using ΔCount thresholds of 20, 30, and 40. As expected, the detected patch-size distribution changed with threshold. A lower threshold detected more weakly illuminated and more extended structures, whereas a higher threshold selected fewer and generally shorter structures. The numbers of detected patches were 46,622 for ΔCount > 20, 23,704 for ΔCount > 30, and 10,036 for ΔCount > 40. The corresponding apparent patch lengths were 5.71 ± 3.68 km, 4.35 ± 2.43 km, and 3.93 ± 2.00 km, respectively.

However, the statistical relationship between apparent patch size and peak flickering frequency remained essentially unchanged. The mean peak frequencies were 8.41 ± 3.92 Hz, 8.45 ± 3.92 Hz, and 8.52 ± 3.97 Hz for thresholds of 20, 30, and 40, respectively. The Pearson correlation coefficients between inverse patch length and peak frequency were −0.004, 0.002, and −0.008, respectively. We therefore conclude that the absence of a robust monotonic frequency–size relation is not an artifact of the chosen ΔCount threshold. This threshold-dependence test has been added to Appendix C.

Regarding the possibility that a keogram slice misses the center of a patch, we agree that this is an unavoidable limitation of one-dimensional slicing. A slice that intersects only the edge of a patch can underestimate its apparent size. This means that the derived patch-size distribution should be interpreted as a distribution of apparent cross-slice extents rather than exact two-dimensional patch diameters. We clarified this point in the revised manuscript (Lines 157–165 and Lines 225–235).

We also tested fixed-*y* east–west keogram slices to assess whether the orthogonal direction could provide a complementary patch-size estimate (Figure R2). This test showed that fixed-*y* slices were not suitable for stable individual patch-size statistics in this event. Because the auroral arc was elongated mainly in the east–west direction and moved in the north–south direction, the sampled structure changed strongly with the selected *y* position: some slices sampled only weak or no emissions, whereas others followed bright arc-parallel structures over long distances. In such cases, connected structures in an east–west keogram do not represent individual patch widths, but instead reflect the along-arc extent of the aurora, multiple flickering elements, or the motion of the arc through the fixed slice. We therefore retained the north–south, approximately cross-arc, extent as the more stable one-dimensional measure for the statistical patch-size analysis and clarified this limitation in the manuscript (Lines 225–235).

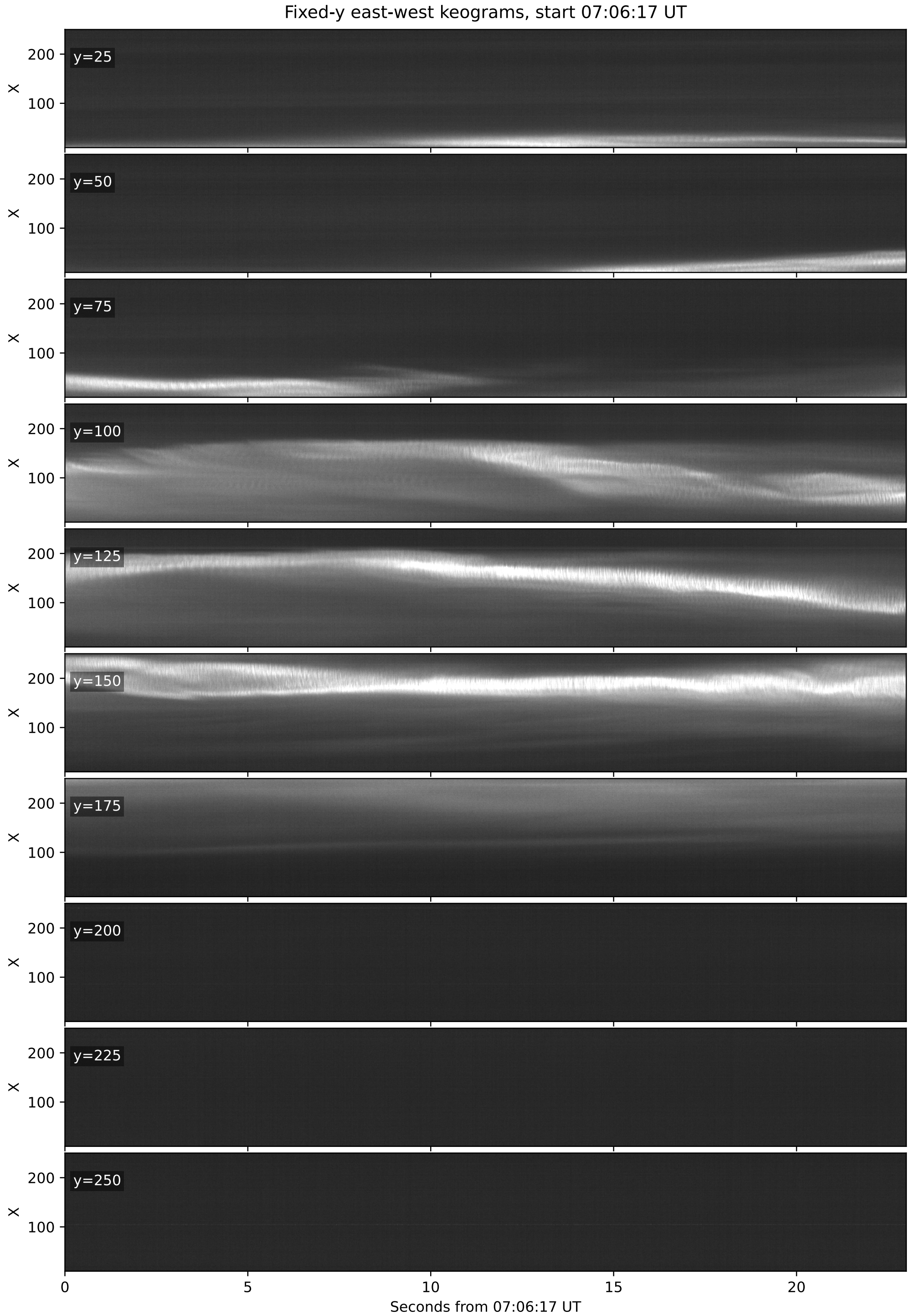

Figure R2. Fixed-*y* east–west keograms.

Major comment 3

The patch size and spatial separation rely on a fixed emission altitude that the authors have chosen (110 km). However, literature shows that there can be significant spread in the peak auroral emission heights (Whiter et al., 2023). Some reports also claim that there can be 557.7 nm emissions below 90 km altitude in the summer mesosphere (Lee et al., 2017), or higher than 120 km (Mende et al., 1993). What is the scientific justification that the authors have used for choosing a fixed emission height of 110 km? How will the spatial estimates be affected if the chosen emission altitude is slightly inaccurate? A more careful analysis of uncertainties, and some quantitative numbers on potential errors, will further strengthen the results.

Response:

We agree that the fixed-altitude mapping should be justified and its limitations should be stated more clearly. We used 110 km as a representative altitude for the green-line-dominated auroral emission in this discrete auroral event. However, because the camera was unfiltered and because the emission altitude can vary with precipitation energy, we do not interpret 110 km as a uniquely determined emission height.

In the revised manuscript, we now state explicitly that all mapped distances are apparent horizontal distances at an assumed emission altitude of 110 km (Lines 93–95). To quantify the effect of this assumption, we repeated the spatial mapping of the detected patches using projection altitudes of 90, 100, 110, and 120 km, while keeping the patch detection and peak-frequency estimates identical to those used in Figure 6. Thus, the same 23,704 detected patches were compared for all assumed altitudes. The mean apparent patch length was 3.56 ± 1.99 km at 90 km, 3.96 ± 2.21 km at 100 km, 4.35 ± 2.43 km at 110 km, and 4.75 ± 2.65 km at 120 km. The corresponding medians were 2.98, 3.31, 3.64, and 3.97 km. Therefore, for an assumed emission-height range of 90–120 km, the mean apparent patch length changes by about 1.2 km.

However, this altitude-dependent change in the absolute spatial scale does not alter the main frequency–size result. The peak frequencies are unchanged because the same detected patches were used, and the Pearson correlation coefficient remained 0.002 for all four altitudes. Thus, the assumed emission altitude affects the absolute apparent patch sizes and separations, but not the conclusion that this event shows no statistical dependence of peak flickering frequency on apparent patch size. This analysis has been added to Appendix C (Lines 493–501). The finite-emission-thickness estimate described in response to Major comment 1 was also added to Appendix C.

Major comment 4

The analysis focuses solely on the auroral observations, with no reference to a broader geophysical context. The paper does not mention the solar wind activity level, geomagnetic activity indices, substorm phase, etc. It may not be necessary with additional figures here, but the broader implications of the results can easily be improved if the authors add a few lines of text to the manuscript about the overall activity level during the observation period. Was this a particularly quiet or active time, in terms of solar wind, magnetic indices, or substorms?

Response:

We agree. We have added a brief description of the broader geophysical and optical context to the revised manuscript. In the main text, we now state that the broader geophysical and optical context is summarized in Appendices A and B, and that the event occurred during enhanced auroral activity rather than under quiet conditions (Lines 107–110).

In Appendix A, we added the 1-min OMNI solar-wind and geomagnetic data (Lines 433–442 and Figure A1). During the optical interval from 06:25 to 07:10 UT, the IMF was predominantly southward, with GSM Bz reaching about −9 nT near the beginning of the optical observations. The solar-wind speed was approximately 400–420 km s^{-1}, and the proton density was about 6–8 cm^{-3}. The geomagnetic indices indicate enhanced auroral activity: AE increased to about 1100 nT and AL decreased to nearly −950 nT, while SYM-H remained only moderately negative and did not indicate a major storm. These conditions show that the flickering event occurred during enhanced geomagnetic activity.

In Appendix B, we also added Poker Flat all-sky keograms to provide a longer optical context for the same night. These keograms show that the analyzed interval was embedded in an active and dynamically evolving auroral arc system rather than being an isolated optical feature (Lines 443–457).

Major comment 5

The paper does not mention any in-situ or ground-based radar/magnetometer data, that might be of relevance. The conclusions might be further strengthened and supported by additional ground magnetometer pulsation data, Poker Flat ISR electron density/temperature measurements, or spaceborne EMIC wave observations (THEMIS, Arase, etc). If unavailable, the authors should at least state this explicitly and discuss the limitations.

Response:

We agree that such data would be valuable for directly testing the EMIC/IAW interpretation. We checked the availability of relevant supporting data for this interval and added a clarification to the revised manuscript (Lines 405–410).

We did not identify suitable conjugate in-situ wave measurements in the magnetosphere or suitable ISR measurements for this interval. We also considered local ground magnetometer data. Such data can provide useful information on the local geomagnetic activity level, but they do not by themselves identify the source-region wave mode responsible for the optical flickering. Therefore, we used OMNI, AE/AL, and all-sky optical observations as supporting context for the event, while avoiding an overinterpretation of ground magnetometer perturbations as direct evidence of source-region EMIC/IAW waves.

We now state that the present study is based primarily on optical imaging, that suitable conjugate in-situ wave and ISR measurements were not identified, and that the source-region wave properties, electron density structure, and acceleration-region altitude cannot be determined directly from the present data set (Lines 405–410). The EMIC/IAW interpretation should therefore be regarded as a consistency argument based on the observed optical frequencies and morphology, rather than as a direct identification of the responsible wave mode. We checked possible magnetospheric spacecraft conjunctions using the Conjunction Event Finder operated by ERG Science Center shown in Figure R3 (from https://ergsc.isee.nagoya-u.ac.jp/cef/orbit.cgi), but did not identify a suitable conjugate satellite near Poker Flat during the analyzed interval.

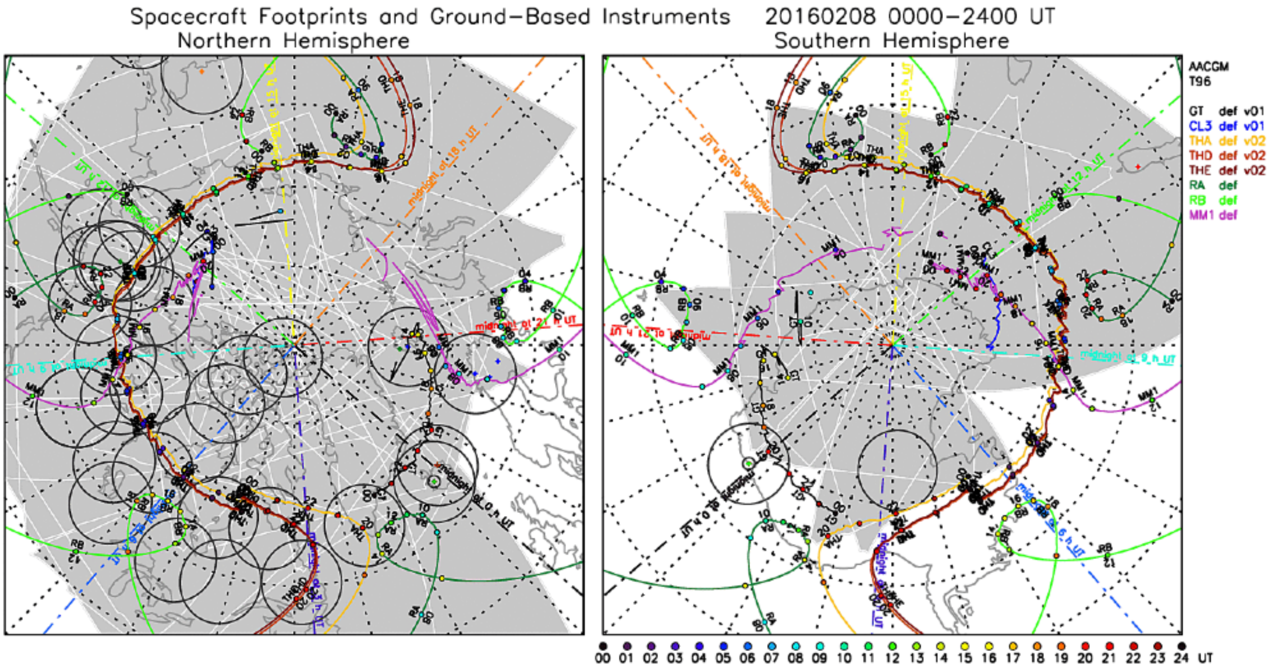

Figure R3. Spacecraft footprints and ground-based instruments on 8 February 2016.

Minor comment 1

Line 202: The video supplement is very helpful. However, please consider referencing a few specific times in the text that show key features.

Response:

We agree. We have added references to specific intervals for Video 1. In the description of Figure 1, we now state that weak flickering can be identified especially in the video interval from 00:00 to about 11:50 (Line 121). We also refer to east–west propagating patches in Video 1, particularly in the interval after about 15:53 (Line 224).

Minor comment 2

The authors may also want to add a few more references to small-scale structures in flickering aurora (Whiter et al., 2008), patch size evolution during pulsating aurora (Partamies et al., 2019), or wave-like structures in the aurora before substorm onset (Wu et al., 2025).

Response:

We agree. We have added relevant references where appropriate. We cite Whiter et al. (2008) in the Introduction when discussing drifting and rotational motions of flickering patches (Line 20). We also cite Wu et al. (2025) when discussing recent observations of pre-onset wave-like auroral forms and their relevance to mesoscale structuring in discrete auroral arcs (Lines 73–75). We did not add Partamies et al. (2019), because that study concerns pulsating aurora. Although pulsating aurora also contains blinking patch-like structures, it differs from flickering aurora in brightness, patch size, modulation period, typical MLT occurrence, and the generation mechanism usually considered. To avoid blurring the distinction between flickering aurora and pulsating aurora, we decided not to include details about pulsating aurora in the revised manuscript.

We thank the referee again for the careful and constructive review. The comments helped us clarify the limitations of the wide-field single-station observations, strengthen the robustness checks for the patch-size statistics, and better place the event in its broader geophysical and optical background.

We thank the referee for the positive assessment of the manuscript and for the helpful comments. We address each point below.

Minor comment 1:

Figure 1 and the FFT spectra. The bright, broadband spectral features, such as the one around 07:05 UT, can be caused by the sudden change in brightness when the bright feature moves into the region where the time series is being extracted from. Care must be taken to not include such "spatial" changes as rapid temporal variations. Perhaps a comment about this could be added. With FFTs a rapid change in brightness can lead to an artificial frequency response at many frequencies.

Response:

We agree. We added a note to explain the spike-like structures in Figure 1f. After examining the data in detail, we found that the narrow vertical structures at approximately 06:48:00, 06:55:44, and 07:03:44 UT were caused by short image-acquisition gaps of about 1 s. We clarified in the revised manuscript that these structures are artifacts and should not be interpreted as broadband rapid temporal variations of the auroral luminosity (Lines 115–117).

Minor comment 2

Line 135: Do you mean panels (b) and (d)? Panels (a) and (c) do not appear to show clear propagation of the auroral structure within the boxed region. Perhaps clarify what is meant here, if I am wrong.

Response:

Yes, the referee is correct. We meant panels (b) and (d). We corrected the text accordingly (Line 147).

Minor comment 3

Figures 4 and 5: It seems this analysis was only done in one direction through the images. Have you done this same analysis through the other direction in the images? For example at Y = 130 and Y = 120 and then remake Figures 4, 5 and 6, for the other direction. If you do not do this in the paper, you should at least explain why it was not done. It seems only natural to examine the size of the patches in both directions, not just one. We cannot assume the patches are symmetric in x and y.

Response:

We agree that the patches should not be assumed to be symmetric. We clarified this point in the revised manuscript. The main reason for using the north–south extent is that the discrete auroral arc was elongated mainly in the east–west direction, so the north–south direction approximately sampled the arc across its narrow dimension. In addition, Video 1 shows that some flickering patches propagated along the arc in the east–west direction. Therefore, an east–west extent would be more sensitive to along-arc motion and would not provide a stable measure of individual patch width. We therefore used the north–south extent as a practical one-dimensional measure of apparent patch size, rather than as a full two-dimensional patch diameter, and explicitly stated this limitation in the revised manuscript (Lines 225–235).

Minor comment 4

Discussion, around line 245 or so. Have you considered adding in an all-sky image or an all-sky keogram from the whole night to show the overall context within which this flickering event is contained? I know all-sky camera data from Poker Flat exists for this day and time, at least in the green line.

Response:

Thank you for the suggestion. We added longer-duration keograms from all-sky cameras as Figure B1 in Appendix B. The keograms show emissions at 557.7, 427.8, 486.1, and 630.0 nm from approximately 03:00 to 16:00 UT and provide the broader context for the sCMOS interval analyzed in the main text. They show that the 06:25–07:10 UT interval occurred during enhanced and dynamically evolving auroral activity rather than under quiet conditions. We also refer to this broader optical context in the main text (Lines 107–110), together with the solar-wind/geomagnetic indices summarized in Appendix A.

Minor comment 5

Line 320 and 345: There is also a comparison to a dispersion relation presented in Michell et al. (2012), Figure 12. They showed that there was some consistency between the theoretical dispersion relation for O+ EMIC waves, for a range of k-parallel and the observed features of the flickering aurora.

Response:

Thank you for pointing this out. We added Michell et al. (2012) to the discussion of the EMIC-wave dispersion relation. The revised text now notes that Michell et al. (2012) also showed consistency between observed flickering aurora features and the theoretical dispersion relation for O^+ EMIC waves over a range of parallel wave numbers (Lines 350–351).

We thank the referee again for the helpful comments. They helped us clarify the interpretation of the FFT spectra, explain the limitations of the one-dimensional patch-size analysis, and place the event in a broader optical context.