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Re; "Polar Patches and the 'Tongue of Ionization'"
MS# RS-1086

Dear Jan:

I have revised this manuscript in response to the comments of the two referees, which were quite helpful. The resulting manuscript is noticeably shorter, and does not attempt to cover as much ground as did the first version. I will have to postpone discussion of several topics, including the relation between cusp processes and the optical emission (and plasma?) distribution at the pole, to another day and another data set.

Here are my responses to the comments of the two referees. First, referee #1.

The discussion of the Full Correlation Analysis, and the comparison of its results with those of the primary patch drift analysis, has been shortened from nearly two pages to less than one. Figure 5 has been deleted entirely. This was a recommendation of both referees.

For lack of both statistically significant contrary data, and a constructive conclusion, I have removed any discussion of the disagreement between the Sojka et al. (1994a) prediction of patch absence between 0900 and 1100 UT at solstice, and our observations of patches during this time period on Dec. 8, 1993. At this time, others are in a better position than I to explore the relation between Sojka et al.'s predictions and optical observations from Eureka.

I have done a cross-correlation analysis between the IMF B_y and the patch velocity deviation from antisunward, and show the results as Figure 6. The observed 20-min lag between B_y at IMP 8 and drift direction changes at the pole is consistent with observations of Greenwald et al. (1990). Again, a fuller data set with dayside observations would be needed to explore this connection in the depth it deserves.

The relation between the CADI virtual height at 4 MHz ($n_e = 2 \times 10^5$ cm^{-3}) and the optical emission brightness has been explored for a 4.5 hour period with numerous patch transits, and the results have been added to Figure 4 (top panel). There is virtually a 1:1 correspondence between descents of the 4 MHz virtual height and increases in 630 nm zenith brightness at Eureka. Lacking fuller data for this day, such as base heights and full ionograms, I can only set an upper limit of 2×10^5 cm^{-3} on the N_mF_2 in the gaps between the black patches in Figure 4, but the appearance is certainly consistent with the existence of relatively deep plasma voids between the ionosonde (and optical) patches.

Having now completed my response to the comments of Referee #1, I turn to those of Referee #2.

This referee commented generally on the absence of new knowledge regarding polar cap patches of the tongue of ionization. I feel that this criticism was partly deserved, but not entirely. My response to his or her comments has been to tighten up the discussion where positive results were presented, and to remove sections that could not be so improved.

The referee disagreed with my interpretation of the optical emission distribution in terms of the (plasma distribution in) the tongue of ionization. I was unsettled by this, and took the time to review the somewhat limited literature on the TOI. The TOI was originally a purely stistical concept, having been discovered on the basis of monthly medians of hourly ionosonde data (Sato, 1959; Sato & Rourke, 1964), and while there was a recognition that it was formed by convection of plasma from the dayside across the polar cap, no attempt was made to measure plasma drifts. The TOI as originally discovered certainly **did** extend into the middle of the polar cap, although this inference depends on which N_mF_2 contour one chooses to follow. I have no quarrel with the statement that "a time varying mechanism is required to produce the isolated patches", but it seems to me that the referee is mistaken in regarding the TOI as necessarily a smooth, unbroken stream of high density plasma. That this is suggested by the early depictions is, in my mind, a necessary result of the limited resolution of the ionospheric data available.

On the basis of the very favourable comparison between the 4 MHz virtual height and the 630 nm zenith brightness at Eureka, I would argue that the optical emission distribution can indeed be interpreted as representative of the TOI, or at least that part which lies within the field of view from Eureka. The referee expresses doubt about the validity of inferring the spatial distribution of F_2-region plasma from the temporal history of optical emission at Eureka, on the grounds that patches are formed by a time-dependent process acting on the TOI. Implicity, he or she is assuming that this time-dependent process continues to act over the whole extent of the TOI, rather than being restricted to near the dayside auroral oval as current theories indicate, and thereby concluding that the emission (and plasma) distributions at Eureka bear little if any relation to the process or processes acting at the dayside oval. Again, I have no quarrel with the claim that plasma structuring occurs throughout the transit of plasma across the polar cap, but such structuring occurs on 100 m - 10 km spatial scales, much smaller than those evident in the emission distribution, and indeed those resolvable by the Polar Camera with 60-s exposures. Thus, I would reassert that the montage of the optical emission shown (much more economically than before) in Figure 5 of the revised manuscript contains useful information on the scale and 2-d structure of the TOI.

As noted above, the section on the FCA has been tightened up considerably, and I judge that, in this form, it merits retention in the manuscript.

I have addressed the referee's criticism of the presentation of the emission montage to the best of my ability. The montage is now presented as a negative, i.e., with black representing the brightest emissions. The edges of the field of view are now clearly identifiable. I have included a gray scale to aid in interpreting the montage in terms of actual emission intensities. It was not appropriate to add information on magnetic coordinates, as the plot was generated with images mapped to geographic coordinates, and the map from geographic to corrected geomagnetic coordinates does not preserve distances. Therefore, I can only hope that the collection of what was formerly a six-panel figure into three panels on one page, all in close proximity to the distance scale, will aid the reader in getting his or her bearings with respect to the montage.

I have removed the discussion of the possibility that the fine-scale structure evident between 0300 and 0500 UT could results from wave perturbations of the F_2 layer height, as the available ionosonde data do not clarify the question, and a positive conclusion is not supported in this event.

I attempted to strengthen the discussion of the relation between the IMF B_y changes and the patch sequence at Eureka (as distinct from the convection direction), but was unable to establish a clear conclusion. Therefore, this discussion was also removed.

As mentioned above, I also removed any discussion of the relation between the observations on this date and the model predictions of Sojka et al. (1994a). This topic deserves a better treatment than I was able to give it in this manuscript.

In summary, I judge that by removing the weak sections of the manuscript which the referee identified, and adding further, more quantitative discussion of the relation between the IMF B_y and the convection direction at the pole, I have appropriately addressed the referee's criticisms. I trust you will concur, and will find the present manuscript acceptable for publication.

I would like to state my appreciation of the care taken by the referees in reviewing this manuscript. Their comments stimulated useful learning.

I hope this finds you and yours well. We are now nicely settled in Saskatoon, and enjoying it very much. It is good to be back here, working in a very active department.

Thanks again for acting as guest editor for this special issue; I'm sure you'll be very glad to have it off your desk. I look forward to hearing from you soon.

With sincere best regards,

David P. Steele, Ph.D.