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Reply

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We thank *Kepko and McPherron* [this issue] (hereinafter referred to as KM01) for their comments, inasmuch as the errors they make in interpreting our results are unlikely to be unique. In correcting KM01 we believe that the physical importance of our results will thereby be clearer to the community. The most interesting of these errors (to the broader community) are threefold:

1. The delay between Pi2 "onsets," as currently measured throughout the community, and auroral breakup onset has important physical consequences that do not evaporate even if a better algorithm for identifying onsets from Pi2 is developed. The current belief that, for example, fast flows seen in the magnetotail precede onset is based on an erroneous and systematically delayed evaluation of onset that occurs when using standard Pi2 techniques. Thus any substorm model in which tail fast flows precede auroral breakup cannot be rescued by improving the technique used for Pi2 onset identification. On the contrary, such renormalization would continue to make clear that the fast flows seen in the magnetotail are delayed with respect to onset [e.g., *Lui et al.*, 1998].

2. KM01 state that they will "suggest a more quantitative method of determining onset times...." Unfortunately, they do not, relying instead on a highly subjective approach. Had they actually tried to implement their fragmentary suggestions as an objective algorithm (instead of identifying onsets by hand), they would have discovered severe difficulties, even within the one case they considered (a second case was later added in the second version of their comment). (Of course, an algorithm can always be adjusted to work for a single case, but KM01 would have discovered the following difficulties.) Neither algorithm fragment that was suggested, namely (1) the "amplitude reaches 1/2 the maximum localized amplitude" and (2) "the amplitude becomes larger than ... perhaps 1 standard deviation of the mean," suffices even for the two cases examined. There are six consecutive localized maxima in their Figure 2, all above the two standard deviation level (actually, KM01 should give UT over which the standard deviation is computed). How is it that KM01 propose to identify the second of the six as the true onset? The severe difficulties in actually implementing an objective Pi2 onset identification without an extraordinary number of false positives have led previous workers to apply much stricter criteria, which would ordinarily rule out taking seriously the small rise (the second of six such rises) as "the" onset.

3. Part of the delay between Pi2 and onset has a definite physical reasoning, not technique related. KM01 have managed to utterly avoid mentioning in their original comment the results of Figures 7 and 8 of our original work. They are invalidating to KM01's thesis. The greater the local time distance between the auroral breakup location and the Pi2 observation, the greater the delay. Likewise, the greater the latitudinal separation, the greater the delay. At most, ~1 min of the average onset could be plausibly attributed to instrumental or identification algorithm techniques. The remainder of the delay orders too well in latitude and local time to be thus explicable. Similar results of latitudinal and longitudinal propagation of Pi2 pulsations have also been reported by K. Yumoto at the 1999 Fall AGU Meeting and partially published by *Uozumi et al.* [2000]. This physical effect can also be seen in the bottom panel of Figure 2b by KM01.

In their greatly revised comment, KM01 have improved their work considerably, profiting from our earlier comments. For example they now use "the first Pi2 associated with a global indicator that an expansion is in progress." In doing so, they are implicitly reaching de facto agreement with our conclusions as to the difficulty of timing onsets through Pi2 alone. Thus, on this major topic the revised result of their work is to enter into unacknowledged but precise agreement with a major thrust of our efforts.

Likewise, they still maintain that the delay between Pi2 pulsations and auroral onset is entirely unphysical, while paradoxically stating that "we agree with the results shown in Figure 7 of *Liou et al.* [2000] that the Pi2s are delayed relative to auroral brightening as a function of the relative separation in local time." Of course, standard techniques for calculating onset time from Pi2 pulsations have no such correction. By acknowledging this need, KM01 once again implicitly agree with a major thrust of our work.

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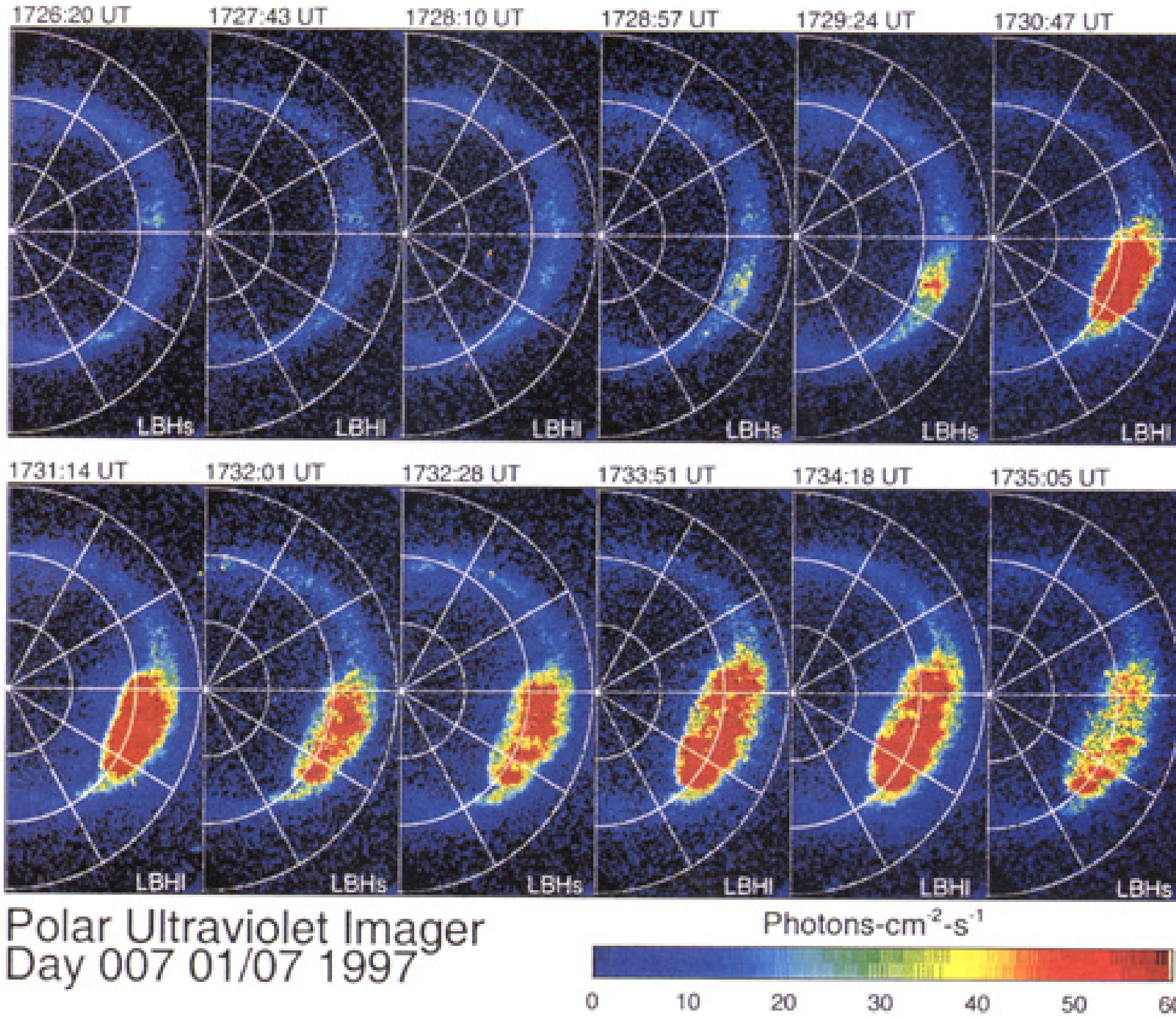

Plate 1. A sequence of nightside N_2 Lyman-Birge-Hopfield (LBH) auroral images from Polar Ultraviolet Imager showing a substorm event on January 7, 1997. The time tag is the center of integration periods (either 18 s or 36 s). Contours of Altitude Adjusted Corrected Geomagnetic Coordinates (AACGM) magnetic latitudes (MLAT) from low to high correspond to 60°, 70°, and 80° MLATs; magnetic midnight is plotted to the right, and dawn is plotted to the top. Two filter images, LBHl (160-180 nm) and LBHs (140-160 nm), are used. A sudden brightening of aurora at 2300 MLT occurred at 1728:10 - 1728:57 UT (1728:29 UT ± 23 s).

The purpose of our paper [*Liou et al.*, 2000] was certainly never to argue that Pi2 pulsations cannot be used to time onsets, or that improvements could not be made in doing so. We did wish to show that certain difficulties arise in using Pi2 pulsations alone to identify onsets and that certain corrections were desirable. If KM01 have developed a technique for identifying onsets that takes into account our results, so much the better.

In terms of the more technical errors that KM01 make, we list only the three most prominent ones below, to suggest caution when analyzing Pi2 pulsations and auroral imagery.

1. *Liou et al.* [2000] defined the onset of Pi2 by the time of peak wave amplitude minus a quarter of the wave period. Admittedly, this definition is ambiguous, because the word "peak" can be either "local" or "global." It is the local peak that was meant to be in the paper, as it can be clearly understood in the case study event. If we had used the "global" peak as falsified by KM01, we would have identified a Pi2 onset 30 or 60 s later than that identified in the paper. Therefore all related comments based on KM01's false implication do not stand. We want to emphasize that the delay between Pi2 "onset" and auroral breakup is not what led us to conclude that Pi2s have significant limitations as substorm identifiers. Obviously, when identifying onsets, it is much more troublesome to have about one third of the instances being false alarms than it would be simply to have a systematic offset. In the second paragraph of their section 2.3, KM01 stated that "it is well known that substorms are accompanied by many Pi2 bursts and ... two bursts are the most probable situation...." Although KM01 do not reference these what they claim to be "well known" results (actually they never provide references for previous results by other authors throughout their comment), we have managed to find a possible source from a nonrefereed proceeding paper by *Hsu and McPherron* [1998]. Obviously, this paper did not use KM01's quantitative method, and we do not think that these cited results would still be true if KM01's onset identification method is used. For example, *Hsu and McPherron* [1998] identified three Pi2s in their Figure 1 event. However, one can easily identify several more Pi2s with KM01's method.

2. KM01 ignore the different wave characteristics that midlatitude Pi2 pulsations have than low-latitude Pi2 pulsations [e.g., *Yumoto*, 1986; *Li et al.*, 1998]. This is why we have carefully chosen the phrase "low-latitude Pi2 pulsations" in our paper title to indicate the validation of our results. In addition to the phase shift, the amplitude of Pi2 pulsations increases with latitude. Therefore it is not physically meaningful to derive an average Pi2 wave envelope from mixing midlatitude and low-latitude Pi2 pulsations as proposed by KM01. Evidently KM01's Figure 2 actually represents wave characteristics from the dominant midlatitude Pi2 pulsations and cannot be used to compare our low-latitude Pi2 results.

Another relevant question is why are not five or nine but seven stations chosen? Had KM01 checked with different numbers of stations from different geographical locations, they would have found inconsistent results. Note that there are, indeed, four small (~0.3 nT peak-to-peak) near-sinusoidal oscillations occurring between the two vertical lines in KM01's Figure 1. However, there is only one wave oscillation in the Pi2 band in KM01's Figure 2 in the same time interval. We feel KM01 owe an explanation about this. Furthermore, the average wave period for the four small oscillations is ~35 s, outside the conventional 40 - 150 s Pi2 band that we adopted and clearly stated in our paper. Scientifically speaking, this small, regular wave train was probably Pc2 or Pi1 pulsations but definitely not Pi2, and therefore it was ruled out from our selection criteria. The small wave power shown in KM01's Figure 2 is a result of using a wider band-pass filter (25 - 150 s) than the conventional Pi2 band (40 - 150 s). Therefore this event may, at most, indicate that Pc2/Pi1 pulsations can time auroral breakups more precisely than Pi2 pulsations can. Of course, a more extensive work needs to be done before making such a conclusion. One more relevant point regarding the characteristics of low-latitude Pi2 pulsation is that the H component of Pi2 dominates at low latitudes; actually, the D component becomes zero at the equator [e.g., *Li et al.*, 1998]. This is the reason why scientists have been using the H component of Pi2 alone to identify substorms.

3. We shall question the reliability of the integrated photon flux shown in KM01's Figure 2 and the validity of their auroral breakup identification method. We believe that their integrated photon flux is subject to severe processing errors, evidently from the appearance of a zigzag structure during the quiet preonset time (flux level is lower for 18-s exposure mode than for 36-s exposure mode for the two LBH filters before ~1330 UT). Consequently, it results in the derivatives of the integrated flux alternatively changing sign as seen in their Figure 2a (in the first version of their comment, KM01 did not realize this error until we pointed it out in the first version of our reply). Note that this kind of error does not appear in our results shown in Figures 1 and 2 of *Liou et al.* [1999]. We feel a better cleaning job on the image data should have been done before testing their quantitative method.

Finally, KM01 state that "without reanalyzing all events used in their analysis, we are not able to determine whether errors similar to the ones discussed here with respect to the March 1, 1997 event were made in identifying the onset of other Pi2 events." Our entire auroral onset database was first put in online (at http://sd-www.jhuapl.edu/Aurora) 2 years ago. If KM01 actually do develop a quantitative approach (as opposed to proposing fragmentary rules), it should be an easy matter for them to prove that they can reproduce onsets without delay. In addition, we also published two more events earlier [*Liou et al.*, 1999], which

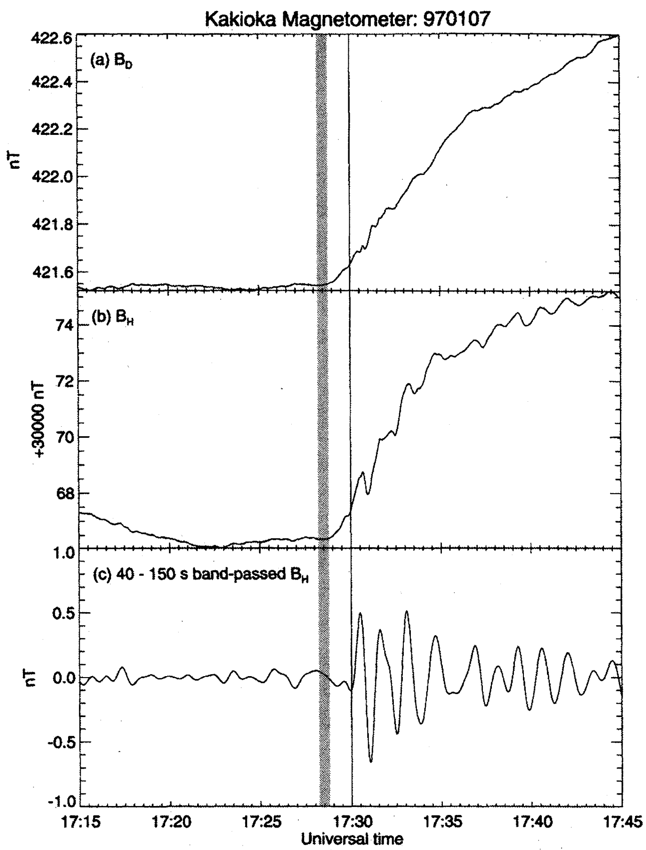

Figure 1. The original geomagnetic field variations from Kakioka magnetometer for a substorm interval between 1715 and 1745 UT: (a) D component, (b) H component, and (c) 40 - 150 s band-pass filtered H component. A vertical line is used to indicate the time of Pi2 onset, and a vertical bar indicates the time of auroral breakup. The width of the bar indicates the uncertainty in determining the auroral breakup.

could have been used as the first check for KM01s undeveloped quantitative algorithm. We made such a suggestion in the first version of our reply, but they instead responded with an event on October 14, 1996, when UVI was out of operation (from August 1996 to November 1996).

So far, we have remarked upon errors made by KM01. Before concluding, we present another event that clearly reveals the delay of Pi2 "onset" relative to auroral breakup. This event occurred at ~1728 UT on January 7, 1997. Nightside auroral images from UVI for this event from 1726 to 1735 UT are plotted in a typical magnetic local time-magnetic latitude format in Plate 1 with midnight to the right and dawn to the top of each image. The auroral breakup can be recognized by a sudden brightening of the aurora at 2300 MLT at 1728:10 - 1728:57 UT (or 1728:29 UT ± 23 s). Magnetic field observations from Kakioka between 1715 and 1745 UT are shown in Figure 1. The magnetic local time of Kakioka at the breakup time was ~0230 UT, ~3.5 hours MLT east of the onset. A Pi2 pulsation of typical peak-to-peak amplitude of 1 nT at this station can be identified with an onset taking place at 1730:00 UT, ~1.5 min delay from the auroral breakup. Subtracting ~0.5 min (LT ~ 4 hours) due to the local time effect, there is still ~1 min delay. Therefore the based line of ~1 min shown in Figure 7 of *Liou et al.* [2000] is real. Note that this event was given in the first version of our reply. However, there is no response from KM01 in the second version of their comment. We would assume that KM00 admit the delay.

The delay of low-latitude Pi2 "onsets" relative to auroral breakups is real. This conclusion is solidly grounded on the basis of many observational facts. We also want to reiterate that Pi2 pulsations are not unique to substorm onsets, so that false onset identifications can jeopardize the validity of Pi2 as a substorm onset identifier.

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