Comment on "B_y fluctuations in the magnetosheath and azimuthal flow velocity transients in the dayside ionosphere" by Newell and Sibeck

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Comment on "B_y fluctuations in the magnetosheath and azimuthal flow velocity transients in the dayside ionosphere" by Newell and Sibeck

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Newell and Sibeck [1993] (hereafter N&S) list some objections to our interpretation of dayside auroral transients and associated azimuthal flow bursts in terms of pulsed reconnection [e.g. *Lockwood et al.*, 1989; 1993a]. They present what they term an "apparently overlooked" alternative explanation in terms of steady reconnection and fluctuations in the magnitude of the B_y component of the magnetosheath field. The objections of N&S can all be answered by reference to our previous publications and their alternative explanation was only "overlooked" in so far as it fails to explain the observations. Here we discuss just some of the reasons why the objections of N&S are invalid, and then give reasons why the events are not simply due to magnetosheath $|B_y|$ changes.

Objections of N&S to Bursty Reconnection

1. N&S state "If the sheath parameters have not changed since the open field lines just poleward of the NMR merged then those lines too were exposed to precisely the same forces and should move in the same way". This we agree with, given that N&S have renamed what we termed the "newly-opened flux" as a "newly-merged region" (NMR). However, despite a correct statement in their abstract, N&S incorrectly continue their argument in the main text: "There should be no relative motion observed between the new merging and the open lines just poleward....unless either the IMF or the magnetosheath flow has changed in the interval between the two merging epochs". This second statement is in error because it ignores the fact that the motion of an open field line depends on the time elapsed since it was reconnected. Initially, a newly-opened field line moves away from the X-line under magnetic "tension". If the IMF $|B_y|$ is large this will yield a strong azimuthal (east/west) flow in the ionosphere. But as the field line evolves it straightens and hence the tension force decays while the sheath flow speed it experiences increases. Hence N&S are not correct when they state that the newly-opened flux will move with the same velocity as the flux that was opened at some earlier time, such that there is no relative motion between them.

2. N&S discuss the *Cowley and Lockwood* [1992] (C&L) model of ionospheric flow excitation. They state "The idea is that dragging tubes of reconnected field lines through the ionosphere induces flow in the rest of the polar cap" and proceed to demonstrate that the motion of such tubes through an otherwise stagnant ionosphere induces flow only in the immediate vicinity of the tube. This discussion completely misrepresents the C&L model. In fact, C&L describe in detail how (eg.) a pulse of dayside reconnection alters the equilibrium configuration of both the region of open and closed flux, generating large scale flows in the magnetosphere-ionosphere system. The "idea" quoted above is N&S's, is not part of the C&L model and is incorrect.

3. N&S dismiss a large body of evidence that the solar wind electric field does not simply map into the ionosphere, on timescales shorter than several substorm cycles, with the phrase "this idea is said to replace the inapplicable idea of mapping the solar wind electric field to the ionosphere". They later admit to the possibility of "damping of high-frequency changes due to self inductance limitations". This is true but, without an attempt to define "high frequency", meaningless. In this respect, N&S fail to point out that the inductive circuit analogy they cite [*Sanchez et al.*, 1991] predicts that the relevant time constant for ionospheric flow excitation is about 20 min. (Similar values have also been derived from line-tying arguments, from the EISCAT-AMPTE data on ionospheric flow responses and (by C&L) from the time for newly-opened flux to evolve into the tail lobe). Sanchez et al. stress that it is even inappropriate to map the electric field over 3-4 hour time scales.

4. The arguments used by N&S about persistence of the cusp are invalid. N&S state "Observations thus lead us back to the concept that the cusp is always present". We agree, with the caveat that "always" is something more than 80% of the time [*Smith et al.*, 1992]. However, N&S continue "Dayside merging must then occur constantly". This is not so. *Smith et al.* point out that precipitation termed "cusp" persists on a newly-opened field line for over 10 min after it is reconnected. Hence for the cusp to be absent requires that no reconnection take place for a period of greater than this. The persistence of a cusp in the observations N&S cite does not prove the reconnection is even continuous, let alone constant.

5. Despite citing the *Lockwood et al.* [1993a] paper which discusses the differences between observations of 557.7nm-dominant and the associated 630nm-dominant transients, N&S confuse the two. For example, multiple brightenings of a 557.7nm transient are not a problem for the transient reconnection theory - they simply represent changes in the stability of the required upward field aligned current: they are not seen in the more extensive 630 nm-dominant transients.

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Rather, the 630 nm-dominant transient continuously fades as the newly-opened field lines evolve and fewer sheath particles gain access to the ionosphere. Our model describes the 630 nm transient fading at the sunward edge of the polar cap when the newly-opened flux has become appended to the tail lobe: it does not penetrate further into the polar cap [e.g. *Lockwood et al.*, 1993a, figure 1]. N&S state that this is contrary to observation. The 630nm-dominant transients we have studied all faded at the sunward edge of the polar cap [*Sandholt et al.*, 1992].

6. N&S revive the "rigid moving-cloud" concept, despite a full discussion of why that could no longer be considered applicable in the paper by *Smith et al.* [1992] cited by N&S. Given this discussion by Smith et al., N&S need to justify their assumption that the region of newly-opened flux (for enhanced B_y) is elliptical and remains elliptical, despite being dragged past other field lines. The change of shape of the newly-opened flux as the system returns to an equilibrium configuration is an integral part of our theory [*Smith et al.*, 1992] and is a vital part of our prediction of the observed cusp ion energy jumps. Note that *Lockwood et al.* [1993a] use an ellipse only for an order of magnitude estimate of event area: N&S's conclusion that only local flow is excited is wholly dependent on their assumptions of a rigid elliptical tube down which the motional solar wind electric field maps.

7. N&S argue that sunward-moving events away from noon cannot map to the noon magnetosphere where FTEs are observed. In fact, an explanation of such mapping was given by *Cowley et al.* [1991]. Likewise, N&S argue that the large longitudinal extents of some breakup events cannot map to this part of the magnetopause. Explanation of the variability of this mapping factor was given by *Crooker et al.* [1991]. That N&S should raise such objections is curious because, given that FTEs can occur wherever reconnection can occur, they would also apply to the N&S interpretation.

The interpretation of N&S

There is a simple reason why we did not interpret the events as steady reconnection with varying magnetosheath $|B_y|$. This mechanism would produce changes in the azimuthal flow component in the ionosphere; however, newly-opened flux tubes, down which sheath electrons stream and generate 630 nm aurora, would be produced at a constant rate. Hence the region of 630 nm aurora would continuously be present and would "wag" back and forth in the east-west direction with the flow direction changes due to the $|B_y|$ changes. For example, increasing B_y (>0) would cause westward migration of the plume of 630 nm aurora in the northern hemisphere (as the flow becomes more westward), but the subsequent decrease in B_y would cause the plume to return eastward. This is not observed. What is seen is a series of events which move in one direction across the field of view [*Lockwood et al.*, 1993a]. This point is particularly clear for those small events imaged in full by one 630 nm camera [e.g. *Sandholt et al.*, 1992]. In such cases, the events are seen to repetitively form, propagate azimuthally in only one direction and fade. An east-west "wagging" of a cusp plume is not observed.

Neither can the theory of N&S explain the many observations of dayside transients which move poleward, with no azimuthal motion, nor that these are unaccompanied by changes in the flow direction, as is often seen by EISCAT [*Lockwood et al.*, 1993b]. In addition, *Elphic et al.* [1990] report dayside auroral transients and azimuthal flow enhancements seen by EISCAT when the sheath was directly observed by ISEE-2 at the dayside magnetopause: $|B_y|$ was stable, even during the FTE events (defined by bipolar B_N with a rise in *B)* that Elphic et al. associate with the ionospheric transients (see B_M variation in their Figure 2). These events cannot be interpreted as being due to changes in the magnetosheath B_y.

In summary, the objections to our model raised by N&S are incorrect. The observations they cite which fail to detect that the persistent cusp is made up of a series of poleward-moving transients do not prove that this is not the case. The alternative explanation offered by N&S cannot explain the observations of dayside auroral transient/flow burst events which move repetitively in one azimuthal direction, nor those events which move poleward with little azimuthal motion and constant plasma flow direction. Because all our observations fall into one of these two classes, the model of N&S fails to explain any of the events we have observed.

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