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Comment on "Origins of energetic ions in the cusp"

Abstract

The paper by *Trattner et al.,[2001]* henceforth TETAL, reanalyzes the data of *Chen et al. [1998]* (CFS) and attempts to prove two statements: first, that the energetic ions observed in the cusp by CFS were actually accelerated at the bow shock and then transported to the cusp; and second, that the detected ions are not accelerated locally. Quoting TETAL's abstract, "An alternative explanation for the energetic particle events is that they are accelerated at the quasi-parallel bowshock, then transported downstream and enter the cusp along newly reconnected field lines or some other solar wind entry mechanism....No local acceleration is required to explain the observed CEP events up to 150 keV/e." We object that neither assertion of TETAL is defensible on theoretical or experimental grounds. Indeed we find the conclusions of the paper are flawed by both statistical and logical errors. In section 1, we outline the logical flaws, in section 2 the misuse of the scientific method, in section 3 the improper use of statistics, and in section 4, the mishandling of spacecraft data. Finally, in section 5 we apply the scientific method of hypothesis testing to the two theories, arguing that given complete ignorance of the data, the hypotheses of CFS are preferable to TETAL.

Logic

The first problem we encounter in TETAL is a matter of logic. In order to prove the existence of a new space physics phenomenon, one example is sufficient, though of course, with the vagaries of experimental science, several examples demonstrating consistency are even better. In order to disprove the existence of a purported phenomenon empirically, however, every example must be reanalyzed for some deficiency that disqualifies said phenomenon for existence. Now CFS argue for the existence of a new local acceleration phenomenon they named cusp energetic particle events, or CEP, characterized by a high density of locally accelerated energetic ($E > 30$ keV) ions in a diamagnetic cavity in the vicinity of the high latitude cusp for which they presented 75 instances. TETAL argue that 53/75 of CFS events are bow shock accelerated and therefore not locally accelerated. Even supposing TETAL were correct, such limited reanalysis cannot disprove the CFS hypothesis, because they have not reanalyzed all the events.

More significant for TETAL's reanalysis is the justification they give for excluding 22/75 events. They implicitly argue that CFS mistakenly included them in CEP events. In their Reply they explicitly state that the cusp is identified with reconnected field lines possessing low-energy ($E<10$ keV) magnetosheath plasma which is missing in the 22/75 events. This explanation of the filtering shows both experimental and theoretical misconceptions.

First, since the CEP identification is indifferent to a low-energy, $E < 10$ keV, component (remember the definition was for energetic ions), such a filtering criterion is completely inappropriate. The phenomena is defined experimentally, not semantically, no matter what title CFS christen it with.

Second, the cusp has been defined in a number of non-identical ways in the literature, including a spatial location given by latitude and longitude, an ionospheric precipitation signature, a topological region defined by gradients in the B-field, as well as TETAL's method of the existence of a low energy sheath component. Clearly CFS and TETAL disagree on the proper definition of the cusp, which should at least flag the TETAL filter as an inappropriate and subjective selection criterion.

Third, there is deep significance in the difficulty in defining the cusp with particle signatures. Low energy particles, such as magnetospheric sheath particles, ($E<10$keV), are viewed as "glued" to the field line due to their dominant $\mathbf{E} \times \mathbf{B}$-drift, and thus become ideal tracers of the reconnection history of a particular field line, which is one definition of the cusp. In contrast, energetic particles ($E>30$keV) are dominated by $\nabla\mathbf{B}$-drift and thus become ideal tracers of the global gradient/curvature topology of the field. As Sheldon et al. [1998] describe, the cusp field line that passes through the minimum B-field at the magnetopause forms the center of a quadrupole trap that provides permanent confinement of energetic particles. Ever since the earliest B-field models for the Earth were discussed, the vicinity of this quadrupole minima field line can and has been referred to as the cusp. These two definitions of "cusp" overlap just as the plasmasphere and the ring current regions of the dipole trap overlap, and are complementary views of the same region of space. The diamagnetic cavities observed by CFS would not be in static equilibrium and would therefore not survive as trapping regions unless they occurred at the quadrupole minimum (Sheldon [2002]). It is this quadrupole topology that CFS refer to as "cusp", and is entirely appropriate terminology for all 75 CEP events described. Thus, applying a low energy criteria to a high energy phenomena is a misunderstanding of the appropriate energy-dependent physics.

Finally, the consequence of applying any filter to a data set, whether justified or not, is a well known bias introduced into the analysis, which in the world of statistical analyses, is known as "cherry picking". Since many data analysts assume that the use of data filters is subjective anyway, they condone this practice implicitly, if not explicitly, but as we endeavor to show next, it has strongly biassed the TETAL reanalysis.

Scientific Method

The scientific method is an approach to analyzing data that attempts to evaluate competing hypotheses by comparing them to unbiased data. This can be cast in terms of Bayesian statistics [*Silva, 1996*], which we omit for the sake of brevity, but which argues that a division or filtering of a data set is unbiassed only if it is uncorrelated with the result, say, by selecting every 3rd event. However TETAL's filtering removed those events which had no low energy component, and then maxwellian fits to the low energy component were subsequently used to argue for a bow-shock source. That is, by choosing only those CEP events that look like bow shock acceleration, and rejecting those that "will have a strong influence on several of the key parameters in this investigation", it is not so surprising that TETAL found that their subset of CEP events are similar to bow shock ions. Regardless of how impressive the statistics of the remainder of the paper, the first conclusion is suspect because it has been introduced in the assumptions.

A second mistake in hypothesis testing made by TETAL concerns the evaluation of the multiparameter models on a given data set. This is commonly known as Occam's razor, which is interpreted to mean that the fewer the number of adjustable parameters required, the better. Again, Bayesian statistics can quantify this qualitative discussion which is a neglected point in data analysis.

Let there be two hypotheses, A and B, with B having one additional adjustable parameter k, which is constrained by prior knowledge to lie between k_{MIN} and k_{MAX}. With some rather weak restrictions we can then write the ratio (*Silva [1996]*):

$$\frac{\mathbf{prob}(A|D,I)}{\mathbf{prob}(B|D,I)} = \frac{\mathbf{prob}(A|I)}{\mathbf{prob}(B|I)} \times \frac{\mathbf{prob}(D|A,I)}{\mathbf{prob}(D|k,B,I)} \times \frac{k_{MAX} - k_{MIN}}{dk\sqrt{2\pi}} \tag{1}$$

If the ratio is greater than 1, then hypothesis A is better, otherwise B wins. Quoting extensively from *Silva [1996]*:

“The first term on the right-hand side reflects our relative prior preference for the alternative theories; to be fair, we can take it to be unity. The second term is a measure of how well the best predictions from each of the models agree with the data; with the added flexibility of his adjustable parameter, this maximum likelihood ratio can only favour B. The goodness-of-fit, however, cannot be the only thing that matters; if it was, we would always prefer more complicated explanations. Probability theory tells us that there is, indeed, another term to be considered. ...As such, the final term... acts to penalise B for the additional parameter; for this reason, it is often called an *Ockham factor*.”

Now if we look at figure 2 or figure 3 of TETAL, we see that they have used four 3-parameter maxwellians (or as described in their Reply, a 2-parameter linear fit in log(f) space with a 1-parameter Energy breakpoint constraint), or 12 adjustable parameters to fit their spectra. In contrast, CFS argue for a 2 parameter power-law fit, which if we include the data rollover at low energy (not strictly necessary for CEP event characterization) becomes a 4 parameter kappa-function (or Mittag-Leffler function) fit. The extra 8 parameters in TETAL introduce 8 Ockham factors, which, unless highly constrained theoretically, would greatly favor the CFS hypothesis.

This is a significant point, and worth restating. If TETAL’s extra 8 parameters were forced by theory to lie in a very narrow range, then they are no longer completely free parameters and will have little penalty in hypothesis evaluation. However, we find just the opposite, that not theory, but practice constrains the extra parameters of the fit, suggesting that the penalty terms are very large. That is, no matter how elegantly TETAL fit their data with a 12-parameter function, a simpler 4-parameter function with similar residuals must be preferred from a Baysean viewpoint.

TETAL argue in their reply that they use a minimum regression analysis of two free parameters to do a piecewise continuous fit to their data in 4 segments giving 8 adjustable parameters. This is erroneous. If the breakpoints were not specified beforehand, TETAL’s 8-parameter regression fit would likely not converge to their published value. There remain 12 fit parameters, only TETAL have used a human intelligence to fix four of them. This is significant, for without proper treatment of human bias, (where to place the break points) the statistics can become grossly distorted by inappropriate limits.

Naturally, TETAL give reasons why they think a 12-parameter function is to be preferred, (it worked well in a previous paper analyzing bow shocks) and where to place the break points, but all such *ad hoc* reasoning reintroduces the conclusions back into the analysis. Thus both in segregating the data, and in fitting the data, TETAL show a propensity for allowing their conclusions to dictate their analysis. The grave danger here, and all data analysts should beware this trap, is that we attribute more importance to a self-referential analysis than it deserves in a Baysean sense.

In addition to errors of logic and scientific method, many of the statistics used by TETAL do not reflect a thorough understanding of error propagation and model testing, which may also invalidate their conclusions as we discuss next.

Statistical Analyses

Fitting Errors:

In TETAL's Figure 2, a CEP event is fit with 4 maxwellian distributions, 12 free parameters, or 8 fitted parameters. No table of fitted parameters, confidence limits on the parameters or correlation coefficients is presented. Cursory examination of the plot shows that only the first maxwellian is constrained in magnitude (density), break-point position and width (temperature). Clearly the next 3 maxwellians have coupled (large cross terms in the covariance matrix) magnitude, break-point position and widths.

TETAL's analysis technique, serially performing linear regression analysis to the segmented data, excludes the possibility of measuring the cross terms that appear to be so large. This is significant, because the technique of reducing a maxwellian to a linear function by taking the logarithm obviously cannot be performed on the sum of 4 maxwellians, since the logarithm of a sum is not so easily reduced. But note that this linearization technique, popular before the advent of computer fitting, cannot tolerate an additive background term which invariably exists from the adjacent maxwellians, as TETAL admit in their Reply. Thus by default, TETAL cannot find the cross correlation terms, and must completely trust their proper identification of energy-breakpoints.

If TETAL had done all 12 parameters at once, using a non-linear maximum likelihood technique appropriate to the problem, a smaller chisquared would probably be obtained with only 3 maxwellians just due to the three-fewer parameters. Which is to say, the use of 4 fitting maxwellians must involve fixing either the break-point position or the amplitude of the 3 energetic peaks, or otherwise the maximum likelihood method would drive them to ridiculous values. As TETAL make clear in their reply, at least 3 parameters (the breakpoints) of the 12 parameter fit are assumptions, and not fits at all.

Thus no error bars are plotted on the data because a maximum likelihood method to the full problem has not been employed in the first place, and no cross-correlation was permitted in the fit. And the values for these fixed parameters are determined by the implicit requirement that it be consistent with previous theory, which is of course, one of the conclusions of the paper. Once again assumptions are masquerading as conclusions.

Errors of Fit:

These four maxwellians are interpreted in terms of temperature and densities that are used later, as seen in TETAL's figure 5. What errors are associated with these derivative quantities? As TETAL reply, the density is dependent on the break-points and the temperature (width). Since one of these is fixed by the assumptions, the density is artificially constrained within some limits as well. Worse, the errors in the fit are compounded by multiplying two related quantities through the unmeasured cross terms in the covariance matrix so that we estimate (from TETAL's figure 2 and 3) that the error in the density can be easily 100% or greater. Worse still, TETAL compare ratios of densities which introduce even more cross terms from the matrix. Fortunately, the first maxwellian is fit rather well in their figure 2, though not so in figure 3 (which fits a 3 parameter maxwellian to exactly 3 points and misses the third one), indicating that the ratio of densities, as plotted in figure 5, has even greater compounded error, with at least 4 cross terms contributing. Qualitatively from their figure 5, the presence of a peak in *Fuselier [1994]*, whereas the lack of a peak in the CEP data indicate that at best, this magnification of the error has completely washed out the effect purported to be demonstrated in the figure. Since no effort has been made to calculate the error in this density ratio, we can only speculate on whether the peak is actually completely absent in the CEP events.

Reliance on Moments:

This desire to discuss moments of the distribution, rather than the distribution itself is highly misleading. Moments, such as averages, are used only when actual distributions are highly regular or unavailable. Neither condition is true of this data set. That is, claiming two distributions are similar based on their moments can be easily disproved if the two distributions are highly dissimilar in shape. Yet this is exactly what TETAL do, for example, in their figure 5 where two distributions are plotted with one consistent with a linear slope and the other possessing a distinct peak. Yet TETAL claim the two distributions are drawn from the same population because "The average values of the density ratios with 0.34% for the quasi-parallel intervals and 0.26% for the CEP intervals are similar." Clearly using the moments when the distributions are available is misleading at best.

Improper comparison of moments:

Even had the two distributions looked similar, such a conclusion is unwarranted without, at a minimum, specifying the average deviation of the mean, which is not given. More accurately, one should have done a F-test whether one distribution is likely to be produced from a random sampling of the other. Again, this is not possible without knowing the error bars on the bins, but it seems relatively apparent that a peaked distribution is unlikely to become linear no matter how it is sampled. To their credit, TETAL speculate that a biased sampling might produce a linear relation out of a peaked distribution, but no quantitative estimate of the size of the biasing is presented, and indeed, a quite extreme bias apparently would be required that should invalidate the previous conclusion that the averages were similar. Thus we see TETAL drawing spurious conclusions without adequate statistical support, or even attempted support. This reliance on *ad hoc* speculation instead of quantitative analysis is a persistant feature that would alone undermine any confidence in the conclusions reached.

Erroneous error bars:

In the above discussion, we noted how a ratio of derived quantities introduce large error bars into the analysis. TETAL push this even further with a ratio of derived quantities from separate spacecraft shown in their figure 6. Now we have the additional uncertainty of the spatial correlation between WIND and POLAR, never mind the temporal lag. *Collier et al. [2001]* show that this spatial correlation grows smaller the larger the transverse distance, and we would suspect that at least a factor of 3 uncertainty is introduced by this method. Since the derivation of partial pressure is never defined, we can only assume that some parameter of Maxwellian #3 is involved, which brings in the same errors as the above density ratios. The net result is that we would expect several 100% error in the ratio, so not surprisingly, we find a washed out peak in the CEP data compared, as usual to a narrow peak from earlier work. Again, TETAL compare moments, finding "The average value for the CEP acceleration efficiency ratio is 2.3 ± 1.9 in agreement with the result from the quasi-parallel magnetosheath."

Finally TETAL give us an error bar, yet clearly it is not the error bar in the measurement, only the error bar in the last averaging step of the plot. This is not an error bar at all but an erroneous and misleading number, which can be seen by the ease with which it can be reduced by using fewer bins. Indeed, one could change the average itself by changing the number of the bins, since the average is calculated from derived quantities, not the data itself. Finally, a F-test comparing the two distributions would show that in actuality, the two distributions are clearly distinct. An eyeball test to determine this would note that the "sigma" of the Fuselier peak is on the order of one bin wide, and that the peak of the CEP data is 2 bins away from the peak of the Fuselier data, or more than 1 sigma away. This exceeds the Rayleigh criteria of distinguishing two stars in an image, and suggests that we do not have similar distributions.

Data-independent Conclusions:

Once again, after this *prima facie* disagreement, TETAL argue that the averages are in agreement with the hypothesis that they come from the same population. Then they go on to discuss a bias in the data of 40%. Why this bias should not affect the average, they never explain. They do this not once, but in their figure 5, figure 6 and in comparison of unplotted results. "The average value for the $O^{>2+}$ to H^{+} acceleration efficiency ratio for the CEP events is 2.3 ± 4.6, in agreement with shock acceleration theory which predicts a ratio of 2.6". It is unnecessary to comment that these error bars (which we argue are far too small) are inconsistent with support for any quantitative theory. Yet no matter what number is derived, TETAL find it to be consistent with their hypothesis, even when it contradicts earlier support.

Abuse of linear regression:

In their section 5, after some preliminary discussion about Fermi acceleration that apparently invalidates the analysis used in sections 2-4, TETAL compare two data sets on which they had performed linear regression analysis. We quote, “The linear regression returned a correlation coefficient of 0.68, the same as in the study with bow shock events above. In addition, the dependency of the exponential spectral slope E0 from the solar wind velocity for the diffuse bow shock ions and the CEP ions is very similar, supporting also the argument that these ions are of the same origin.”

The use of the conjuction “In addition” suggests that TETAL find a similar correlation coefficient (R) to be support for identical distributions. Now using statistical assumptions, R^2 gives the percentage of data points that fit the hypothesis of the straight line. In other words, 46%, or less than half the data fit the hypothesis of a straight line in the CEP data set, 43% in the AMPTE data set. Many analysts would say that a linear regression is only meaningful when more than half the data fit the straight line hypothesis, or $R >$ 70%. Neither data set is remarkable either for its agreement with Fermi acceleration theory or with each other. It is also highly imprudent to argue that similar correlation coefficients should be taken as evidence of two data sets being drawn from the same distribution.

Even more puzzling is that neither the slope, the error in the slope, the intercept nor the error in the intercept of the linear regression fits to the two data sets are given, though clearly the analysis has been done, which would be the real criterion for deciding whether the data sets are similar. Not having the data, and unable to extract the values from the messy overplotted data of their figure 7, we cannot estimate the errors accurately, but we can find the slope and intercept. From the two graphs we get:

$$\begin{aligned} \mathrm{AMPTE_{E0}} &= 0.035(V*\cos(\theta_{BV}))+0.2 \\ \mathrm{CEP_{E0}} &= 0.0475(V*)-6.2 \end{aligned} \tag{2}$$

Since it seems likely that the fitted error in the slope is $>$ 40%, one might conclude that the dependence of E_0 on V_{SW} is similar in the two plots, though of course, we cannot be sure because TETAL plot different quantities on the abcissa of each plot, but no effort has been expended to explain the differing offsets. This is important, because E_0 is a derived quantity, the exponential slope of the spectrum, and should remain invariant from source region to observation region. What these two plots indicate, is that magnetosphere has a softer spectrum than the magnetosheath for a given solar wind speed, so that energetic particles are less likely appear in the magnetosphere. Since it is an enhancement in these same particles that defines CEP events, one might argue that these data mitigate against a bow shock model.

Since the fit must have been performed to extract R, we find the failure to report the discrepancies in the slope and intercept to be disengenuous at best. Unfortunately, this conclusion is amplified by a detailed look at the way TETAL have handled the reanalyzed data, which we discuss next.

Mishandled Spacecraft Data

Misrepresentation of reanalysis

Figure 1 plots the 9/22/96 CEP events, where the two dotted lines are at 00:40 UT and 00:51 UT; bracketting TETAL's typical event time interval. Note that in TETAL's paper this "typical" CEP event started at 00:40 UT; however, in the CFS CEP event list, this event started at 00:42 UT, meaning that more than 22% of the time interval of TETAL's "typical" CEP event was outside the original CEP event time interval. Now looking at the axis of Figure 1, the event was observed at altitudes more than 8 R_E in the cusp, which is different from TETAL's stated selection criteria that "observed at altitudes between 3.5 and 6 R_E in the cusp." In fact, all of the CEP events reported by CFS were measured at altitudes larger than 6.7 R_E in the cusp, as TETAL acknowlege in their Reply.

Lack of statistical correlation with solar wind

TETAL claim that the energetic ions in the CEP events are accelerated at the quasi-parallel bowshock "which depends on upstream solar wind velocity". This dependence of CEP flux on velocity may exist over longer time intervals of days or weeks, but does not appear to be the case in the several hour long time interval of our analysis.

During the 9/22/96 CEP event period, the 0.52-1.15 MeV helium flux was enhanced and changed by more than two orders of magnitudes (Figure 1, middle panel), while the local magnetic field showed diamagnetic cavities with large fluctuations (Figure 1, bottom panel). The lower two panels display the upstream solar wind velocity measured by the WIND spacecraft. They indicate that during this period, the solar wind velocity was rather stable. Figure 2 compares the proton energy spectrum at 1:04-1:15

UT (bracketted by dashed lines in Figure 1) with that during TETAL's "typical" CEP event period at 0:40-0:51 UT on 9/22/96. The MICS (Magnetospheric Ion Composition Sensor) data in Figure 2 show that over 1-30 keV, the proton fluxes at both 0:40-0:51 UT (squares) and 1:04-1:15 UT (pluses) were almost the same; however, at energy $>$ 40 keV, a higher proton flux was measured at 1:04-1:15 UT than at 0:40-0:51 UT. The key point is that during these two periods, the upstream solar wind velocities were almost the same (Figure 1, top panel), but the proton fluxes (at $>$ 40 keV) exhibited a significant difference by as large as a factor of 18 (Figure 3), demonstrating a singular lack of correlation.

In their Reply, TETAL argue that density is also important in upstream acceleration, which varied during this event. However they gloss over the very real difference between velocity and density in the Fermi acceleration model, in which velocity increases the seed population energy, and hence is exponentially magnified in its effect on energized particles, whereas density is simply a linear effect. Thus a 30% increase in density hardly explains the factor 18 higher energetic particle density. TETAL hypothesize that connection efficiency of the reconnecting region is responsible, but clearly this event reveals the difficulty in reconstructing a multi-step process to account for CEP events, so many unknowns make prediction or disproof impossible.

Nor does this reconnection refinement explain why reconnection efficiency, a low energy particle phenomenon, should have any effect on energetic ions in the first place. Nor why the spectral index of E$>$40keV particles for event 1 (0:40-0:51) is substantially different from event 2 (1:04-1:15), and temporally changing (Figure 3.)

Inapplicable model

If the quasi-parallel bow shock acceleration model mentioned by TETAL were correct, it should energize protons from their thermalized solar wind energy of 2 keV up to about 40 keV, not 150 keV. Conversely, if protons are energized up to 150 keV without simultaneously energizing the E$<$40keV population (Figure 3), then the quasi-parallel bow shock acceleration model described by TETAL is inapplicable to CEPs. Whatever the merits of bow shock acceleration elsewhere, it doesn't apply to this data set.

Forced interpretation of data

With an inspection of their figure 2, one finds that the observed proton spectrum at 0:40-0:51 UT on 9/22/96, in contrast to TETAL's assertion, does not show "breaks" at ∼10 and 150 keV/e alone, but also at ∼50 keV/e. This is implicit in TETAL's analysis that used the sum of 3 maxwellians to cover this region, with the middle maxwellian centered around 50 keV/e. One can verify with a straightedge, however, that a single power law will fit nicely from 5-30 keV/e, and again from 80-150 keV/e, with an excess residual "bump" centered at 50 keV/e (which grows and moves toward higher energy as expected for a local Fermi acceleration process, see Figure 3.) In other words, TETAL's claim that the proton spectrum broke at ∼ 10 and 150 keV/e indicating three separate sources is artificial, better represented as a (growing) bump at 50 keV/e superposed on a power-law tail.

Nor does our replotting of CAMMICE and IPS data (squares and diamonds in Figure 2) find their 10 and 150 keV/e breaks, indicating the marginal nature of their observational keystone. Since this is likely their best "typical" proton spectrum, all of their conclusions in their section 6, based upon the multiple maxwellian fits to the breaks at ∼ 10 and 150 keV/e, are likely meaningless, and with the inability to estimate errors on the coupled 12 fitted parameters, entirely misleading.

Adjustment of data to theory

In addition to finding spectral breaks where convenient, TETAL miscompare data which allows better agreement with their model. In their figure 5, larger bins for the CEP events were compared with smaller bins in the magnetosheath, which invalidates their conclusion 3 in section 6. In their figure 6, the peak value for the magnetosheath was compared with the average value for CEP events which bring it in better agreement with the theory, invalidating their conclusion 5 in section 6. But more egregious is their handling of the particle spectra.

TETAL stated that *Chang et al. [1998]* and *Trattner et al. [1999]* showed that CEP spectra are very similar to ion spectra upstream/downstream from the quasi-parallel bow shock, "and the observed CEP spectra below 150 keV/e can be simply explained by transporting bow shock accelerated particles from the magnetosheath along the connected magnetic field lines into the cusp." Again, as in *Trattner et al. [1999]*, they mistakenly compare peak upstream ion flux with averaged CEP flux. Worse, in figure 3 of *Chang et al. [1998]* they present POLAR ion data incorrectly, moving the MICS lower energy limit from 1 keV/e to 0.6 keV/e which brings the fluxes into better alignment. In addition, they move the HIT (Heavy Ion Telescope) helium data point on August 27, 1996 by a factor of 2 below the actual observed value. Finally, they made no solar modulation effect correction for 1984 AMPTE data when comparing with 1996 POLAR data. Therefore, their conclusion, based upon their figure 3, that "Energetic ion fluxes downstream from the quasi-parallel bow shock are comparable to those observed in the cusp" is questionable, since it appears adjusted for better agreement with the theory.

Misrepresentation of prior work

TETAL stated that *Chang et al [1998]* "found that CEP events occurred mainly for $\Theta_{Bn} < 45°$". However, nowhere in *Chang et al.'s [1998]* paper, is a Θ_{Bn} value stated or plotted. In fact, the only Θ_{Bx} (cone angle) shown in figure 2 reveals that during the hour of "intense ion fluxes from 6 to 7 UT" (CFS's CEP events), $\Theta_{Bx} > 45°$ for all but the first 5 minutes, necessitating a quasi-perpendicular bow shock at the sub-solar point during the CEP events. Now a quasi-parallel shock acceleration event may have been occuring somewhere on the flanks of the magnetosphere, but as *Chang et al. [1998]* are at pains to point out, these conditions do not favor high energy. Thus, TETAL's claim concerning the importance of quasi-parallel bow shock acceleration for the observed energetic fluxes in *Chang et al. [1998]* is rendered entirely irrelevant.

TETAL also stated that *Trattner et al. [1999]* compared two CEP events with simultaneous observations by Geotail upstream and downstream of the shock "and found a remarkably good agreement between the CEP cusp spectrum and the bow shock spectrum up to 200 keV/e". This is entirely misleading. *Trattner et al. [1999]* compared the peak (over one minute) upstream ion flux with the averaged (over 30 minutes) cusp ion flux and even unjustifiably increased the peak upstream ion flux by a factor of 5.3, yet the resulting peak bow shock flux was still lower than the averaged CEP spectrum at energies $>$ 50 keV, indicating that the quasi-parallel bow shock accelerated ions cannot account for the CEPs by any stretch (see *Fritz and Chen, [1999]*).

Theoretical Support

We have so far attempted to show how TETAL's reanalysis of the data was both mistaken and misleading. We now would like to argue without resort to detailed analysis that the CFS hypothesis is superior to that of TETAL because of what it attempts to prove (rather than disprove).

TETAL's second conclusion (from the abstract) was that no local acceleration mechanism was operating in the cusp. They present a weak theoretical argument against local acceleration, but rely almost completely on experimental evidence for an alternative mechanism to discredit local acceleration. Their theoretical argument is that CEP power-law spectra show a break at 150 keV/e indicative of two populations. Thus, "it is unlikely that a single cusp acceleration process will be able to produce two distinctly different energetic cusp distributions."

We demur that nothing is more likely. Even bow-shock (a.k.a. Fermi) acceleration will show a break in the power law tail at sufficiently high energies, if for no other reason than the gyroradii of the energizing particles become larger than the containment volume, and the acceleration efficiency drops precipitously. Thus a break, per se, is not evidence of two populations. But note that such limits always produce a convex break in the power-law tail, indicative of a spatially limited single acceleration process. In agreement with TETAL, a concave bend in the power law tail is strong evidence of a second population. The mistake in TETAL is that by ignoring the pattern of breaks–concave, convex, concave–they misidentify the second population as occurring above 150 keV/e, rather than centered at 50keV/e and growing (see Figures 2 and 3). That is, they attribute this peak to two separate populations because it produces 2 concave breaks, which seems unlikely given that both populations would have to increase together to simulate a single peak.

This is a crucial point, and perhaps the source of TETAL's confusion. We do not doubt that bow shock acceleration is occuring and may supply energetic particles to the magnetosphere. We have never claimed that bow-shock accelerated particles could not penetrate the magnetopause. It may even be the source of the background power-law tail which precedes CEP events. But a CEP event is characterized by a large enhancement, observed to temporally increase, of the energetic particles above a threshold cutoff energy ($\sim$60 keV/e in Figure 2 and 3), which cannot be accounted for in the upstream region, nor by classical quasi-parallel bow shock acceleration (which must begin with a seed population around 2 keV/e). When one subtracts the background spectrum from the enhancement (Figure 3), it becomes immediately apparent that an energetic peak far above the solar wind 2 keV/e is responsible. Such isolated peaks have frequently if not exclusively been assigned a single, local acceleration mechanism in the past by countless investigators. Thus TETAL's theoretical objections do not stand.

Nor will TETAL's experimental objections bear much scrutiny, for as we argued above in section 1, TETAL will have a difficult time empirically proving the logical non-existence of any phenomenon even if they had done their analysis correctly. However, TETAL actually undermine their position in their section 5, where they glibly point out that Fermi acceleration, the mechanism they propose for bow-shock acceleration, produces a power-law spectrum, not a Maxwellian. Thus the 4 Maxwellian fit to the spectrum of TETAL's sections 2-4 must be an approximation. Worse still, the power law distribution for energetic particles as described by CFS, is precisely the distribution expected if Fermi acceleration were occurring locally, say, from a shrinking diamagnetic cavity. Thus TETAL have inadvertantly supplied a theory in support of local acceleration.

Furthermore, their model to replace local acceleration involves a highly unlikely 5 step process, beginning with 1) an acceleration outside the bowshock where the magnetic field is quasi-parallel to the shock normal, 2) a transport through the turbulent magnetosheath to a specified location on the magnetopause, 3) an energy and species dependent penetration across the magnetopause possibly mediated by an unspecified magnetic reconnection mechanism, 4) a transport along field lines through the magnetosphere, (which in *Blake [1999]* can also be a chaotic wandering across field lines), and finally 5) a simultaneous leakage of magnetospheric populations from low latitude dipole trapped populations to the high latitude cusp in such a way that the "spectral breaks are consistent with observations in the quasi-parallel magnetosheath". Note that this 5-step process has to occur in a time-synchronized fashion, in highly localized diamagnetic cavities with extremely sharp boundaries that CFS has identified as CEP events. In comparison, CFS argue for some sort of 2 step process that can explain all the data: a) particles are trapped inside the cusp, b) particles are locally accelerated.

It is almost fruitless to speculate on the probability of such a 5 step process producing nearly identical spectra with identical spectral breaks in two separate CEP events, much less 75. Occam's razor should certainly prefer a two step local acceleration mechanism barring any theoretical reason why such local acceleration is not permitted. Indeed, we might raise several dozen objections to such a

5 step process even without an alternative hypothesis, but one should suffice.

As the Blake analysis demonstrates, ions of the energy observed by CFS and characteristic of CEP, do not remain fixed on a flux tube the way 1 keV solar wind plasma does. Indeed $\nabla \mathbf{B}$-drift dominates the motion of such ions, such that the transport through the magnetopause or cusp is totally dominated by such inhomogeneous magnetic fields. It is completely impossible for ions with energies from 40-400 keV/e, localized in a region of quasi-parallel geometry, to have followed similar paths through the magnetopause to the magnetosphere that brings them back together in the concentration observed in a CEP event, making TETAL's figure 1 highly misleading. And should one argue that the source is more broadly distributed, thereby permitting different source regions to supply different parts of the CEP spectrum similar to Blake's conjecture, then one must explain why a CEP event has such abrupt boundaries. Thus TETAL's mechanism may be a viable mechanism for supplying energetic particles to broad swaths of the magnetosheath, but is singularly incapable of describing any of the discrete features of CEP events.

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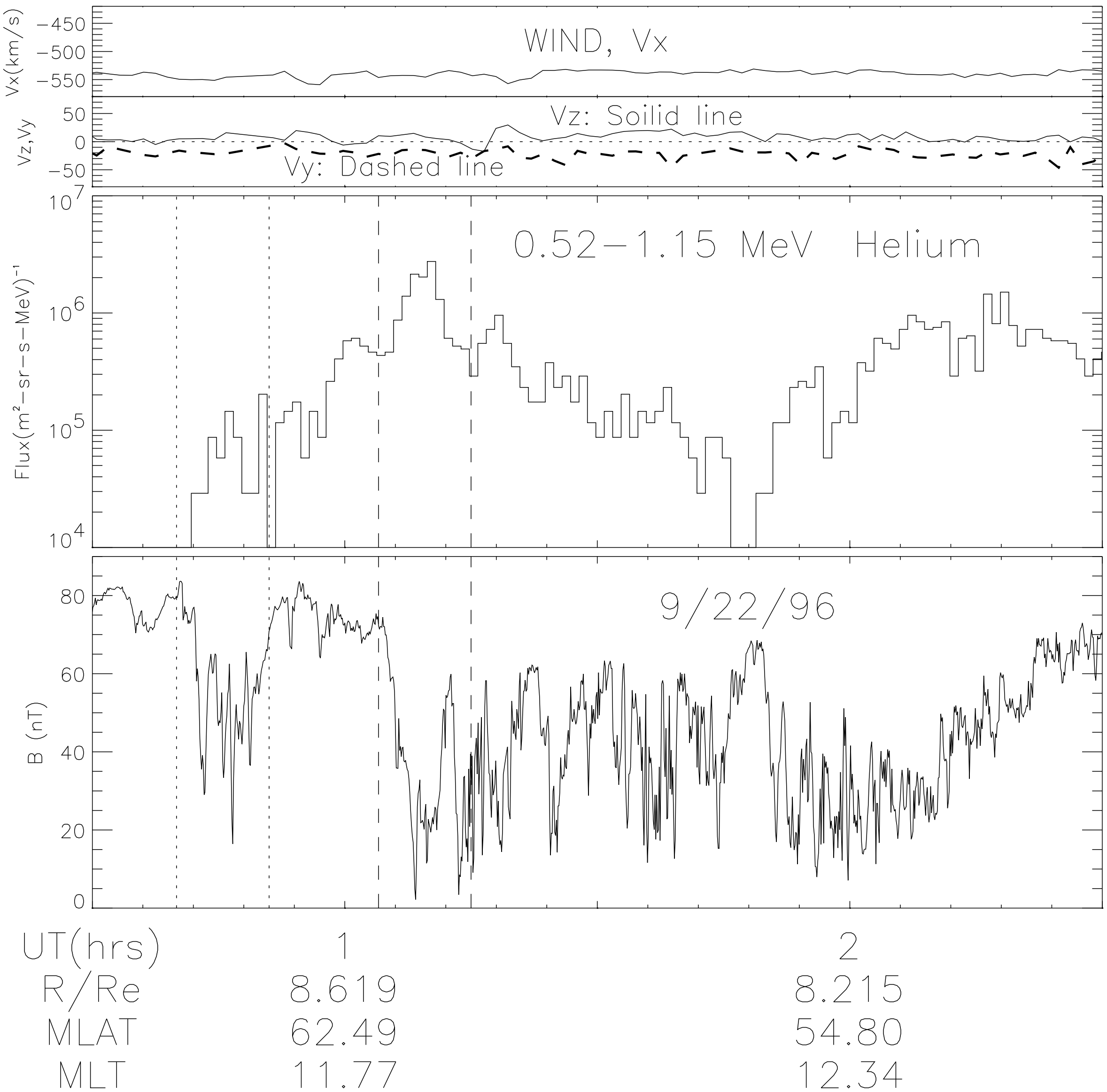
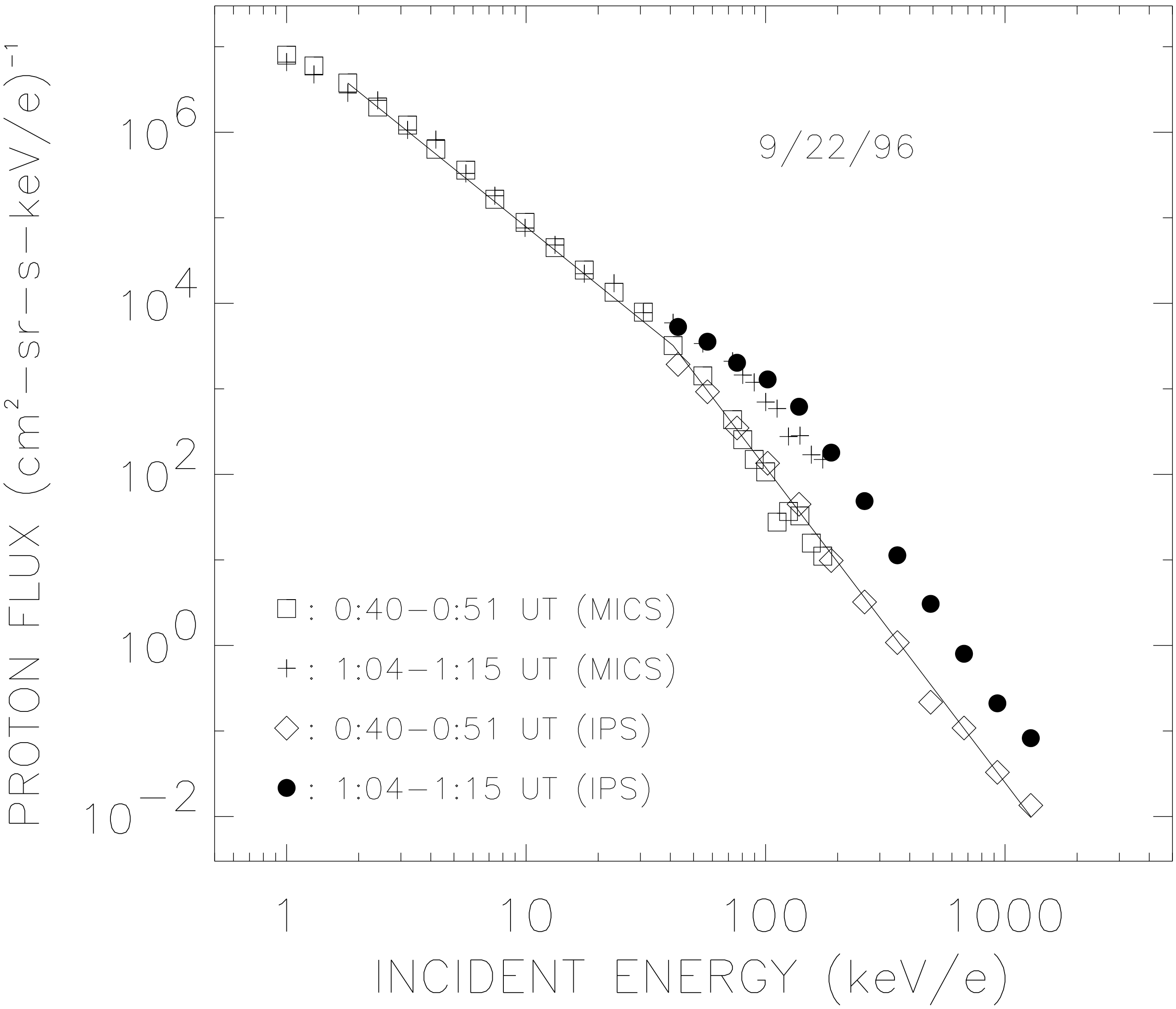
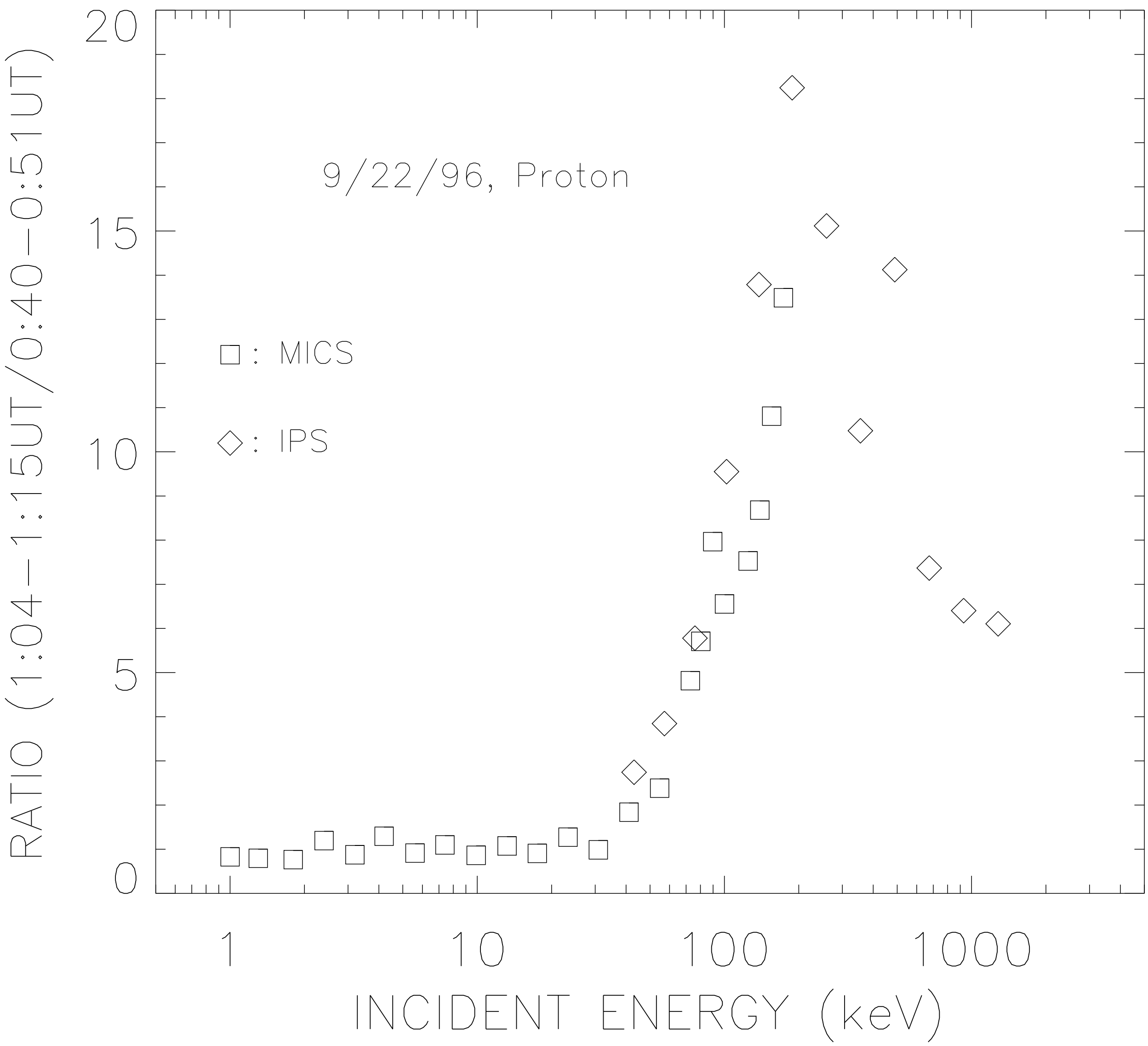
Figure 1. The CEP event on September 22, 1996. Time profiles of solar wind velocity measured by WIND in GSE coordinates on September 22, 1996 are plotted in the top 2 panels. Corrections have been made for the propagation time from WIND to POLAR. The middle panel is the spin-averaged flux of the 0.52-1.15 MeV Helium versus time, and the bottom panel is the corresponding variation of the local magnetic field strength, where the two dotted lines in each panel are at 00:40 UT and 00:51 UT corresponding to TETAL's typical event time interval and the two dashed lines in each panel are at 1:04 UT and 1:15 UT. The distance of POLAR from the Earth (in R_E), the magnetic latitude (MLAT), and the magnetic local time (MLT) are shown at the bottom of the figure.

Figure 2. Proton energy spectra observed by POLAR/CAMMICE/MICS and POLAR/CEPPAD/IPS at 0:40-0:51 UT (open squares and open diamonds) and at 1:04-1:15 UT (pluses and solid circles) on September 22, 1996. The double power-law line (solid line) well represents the spectrum at 0:40-0:51 UT.

Figure 3. Intensity ratio of proton flux at 0:40-0:51 UT to proton flux at 1:04-1:15 UT on September 22, 1996.


WIND, Vx
Vx(km/s)
-450
-500
-550
Vz: Soilid line
Vy: Dashed line
Vz,Vy
50
0
-50
0.52-1.15 MeV Helium
Flux(m²-sr-s-MeV)⁻¹
10⁷
10⁶
10⁵
10⁴
9/22/96
B (nT)
80
60
40
20
0
UT(hrs) 1 2
R/Re 8.619 8.215
MLAT 62.49 54.80
MLT 11.77 12.34


9/22/96
PROTON FLUX (cm²-sr-s-keV/e)⁻¹
10⁶
10⁴
10²
10⁰
10⁻²
□: 0:40–0:51 UT (MICS)
+: 1:04–1:15 UT (MICS)
◇: 0:40–0:51 UT (IPS)
●: 1:04–1:15 UT (IPS)
1
10
100
1000
INCIDENT ENERGY (keV/e)



20
15
10
5
0
RATIO (1:04−1:15UT/0:40−0:51UT)
9/22/96, Proton
□ : MICS
◇ : IPS
1
10
100
1000
INCIDENT ENERGY (keV)