

The authors have made a substantial effort to respond to my comments, and I thank them for their diligence. Their responses satisfy concerns 2-6, 8. With regard to the other points:

1. The authors do not show “Evidence for the role of current sheets and reconnection sites in the generation of this intermittency is provided...”

By the authors admission the evidence is either circumstantial (ion temperature bursts and modification of soft X-ray signal), or indirect (through the presence of fat tails in the pdf of increments, combined with the correlation between MHD simulations of solar wind plasma and in-situ satellite data). In their response to me the authors have used the terminology “valid hypothesis”. The authors need to either qualify the statement, or weaken it.

The phrase in the abstract,

“Evidence for the role of current sheets and reconnection sites in the generation of this intermittency is provided, but the true nature of the observed intermittency remains unknown.”

has been modified to,

“While evidence is provided which supports the hypothesis that current sheets and reconnection sites are related to the generation of this intermittent signal, the true nature of the observed intermittency remains unknown.”

We hope this rewording is sufficient to address the referee’s concerns and we think it better reflects the nature of the statements made in the rest of the manuscript, as well.

7. While it may be a standard result in turbulence literature, I suspect it is not clear to much of the broader PRL readership why intermittency is related to the flatness of the distribution function. A reference would help readers outside the field.

The references mentioned in the first paragraph (Greco08, Greco09, Wan09, Servidio11b) discuss this connection in detail though this sentence has been added to the first paragraph for clarity: “This intermittency, or ‘fat tails’ of a probability distribution, indicates large excursions from a mean which suggests the presence of coherent structures rather than purely Gaussian fluctuations.”

I accept that the authors have addressed my concern. I have a more significant concern which I did not raise in my initial review. If I consider a coherent monochromatic field oscillation of the form $B_r = A \exp(i \omega t)$, then

$$\Delta \dot{B}_r = d B_r(t+\Delta t) / dt - d B_r(t) / dt = A i w \exp(i w t) (\exp(i w \Delta t) - 1)$$

Calculation of the real part will just compute the pdf of a sine/cosine modulated by a factor $(\exp(i w \Delta t) - 1)$, whose real component will vary as $\cos(w \Delta t)$. As Δt reduces to zero, the pdf will become flatter, and the flatness will presumably increase, which is what is observed in Fig. 3(b). In the working of Tsurutani and Smith (1979) it would appear a minimum time separation for Δt , was applied to ensure they do not sample the same phenomena when computing the difference. Does the pdf of the increment reduce to something that has monochromatic properties as Δt approach zero? How do they reconcile this problem in their working?

As I did not raise the point in my initial review I think it inappropriate to insist the authors address the point prior to my acceptance, but I would welcome a response from the authors to clarify my understanding.

Our overall understanding of the referee's question is whether or not we have taken into account multiple counting of the same intermittent event which, as discussed in the Tsurutani and Smith paper, can occur if a large change in the timeseries (thick intermittent event) occurs over a period greater than the particular Δt used to calculate the increments, and thus results in an artificially higher counting of large increments. This in turn would skew the PDF to larger flatness values. The short answer to this question is no, this distinction was not taken into consideration in this analysis and the flatness values are presented all the way up to the smallest time step achievable based on our diagnostic sampling rate. The referee's question is quite intriguing and has actually spurred a great deal of thought regarding this issue. Our conclusion, however, is that for the sake of the particular analysis we are conducting, making a minimum time separation restriction is not needed. In some sense, the double counting that could occur at smaller timescales is unavoidable. If there is some prior knowledge as to the smallest size of the discontinuity, then some minimum time range can be applied. However if the discontinuity sizes are not known a priori, or if there is a wide distribution of sizes, there will always be an issue of double counting and thus the normalized flatness measurement will always tend to grow as a function of decreasing time step. On the other hand, this means that comparison of two different curves (or more) as is shown in Figure 3, can be used to get a sense of the relative distribution of discontinuity sizes (again assuming it is a discontinuity that causes the distribution to be flatter). A steeper curve suggests that the probe is seeing more small discontinuities than a shallower curve. This is ultimately the conclusion we arrive at with this dataset: the increase in helicity increases the intermittency which is interpreted as an increase in the number of current sheets observed in the plasma and a decrease in their relative size. The true nature of the discontinuities present can only be unraveled by looking at each timeseries directly (as in Tsurutani and Smith), and is work that is being under taken now.

For this paper, however, and again in distinction from Tsurutani and Smith, we are seeking a more global nature of the plasma rather than trying to seek out particular discontinuity

events. However, the referee's point is useful to keep in mind for future PDF analyzes where such event finding will be more of a focus.

Regarding monochromatic properties, it is not clear that the PDF's would approach a monochromatic distribution function (which would be mostly fat tail and almost zero middle) at even the smallest time steps achievable in the lab. This is mostly due to the assumption that the underlying plasma is made up of many modes and one would have to probe at frequencies corresponding to the wavelengths of the shortest wavelength modes in order for this monochromatic effect to become appreciable. Another way of putting it is that this analysis technique of building PDFs of increments would not be really appropriate for a plasma that was dominated by a few coherent modes and did not presented a fairly broad spectrum.

9. Other than demonstrating the need to use higher order tools to compute evidence of intermittency, I do not understand the physical significance that "there is a change to the intermittent character of B_{dot} fluctuations as a function of injected helicity while simultaneously showing little to no change in the turbulent frequency-domain power". Is there one?

High helicity plumes have excess "twist" that needs to be relaxed. Or maybe another way to say it: the energy-to-helicity ratio (related to λ) is mismatched from the final, relaxed state. More dynamics have to ensue to transport the state from initial turbulent to final relaxed. Though it is not entirely clear that this is exactly what is occurring, it is interesting to speculate that the relaxation process generates a similar turbulent process (reflected in the spectra) despite starting at differing levels of "twistedness". The increase in intermittency would then seem to reflect a change in the magnetic structure of the plasma but in such a way that the cascade of energy to smaller scales is maintained at the same rate.

Thank you for providing this speculation. As the speculation has no bearing on the manuscript, I do not offer any further comment.

With an appropriate response to issue 1 I would be happy to accept the manuscript.

We thank the referee again for the comments and appreciate the interest taken in our work based on the intriguing questions. We believe the changes we have made and presented here will be sufficient for meeting your expectations for publication.