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Report of Referee 1  
----------------------------------------------------------------------*  
*This is an interesting manuscript which takes an approach to the problem of plasma turbulence which is very different from most analyses recently published. Three different turbulent plasmas, two in laboratories and the solar wind, are analyzed using the Bandt-Pompe permutation entropy and the Jensen-Shannon statistical complexity. The problem for me in reviewing this manuscript is that I am not at all familiar with either of these two formalisms, and the manuscript does not provide much help to me in increasing that familiarity.  
  
The conclusions of this manuscript make sense; from very basic arguments I would agree that the unbounded solar wind is more likely to be in a high-entropy, fully developed turbulent state than laboratory plasma turbulence which is constrained by the walls of the confining vessels. But I am quite far from understanding the mathematical formalism which leads to the conclusions of this manuscript. So my request to the authors is not to add any more calculations to this manuscript, but to rewrite and re-express their presentation to make it more accessible to the large body of Physical Review readers who are not well-versed in the Bandt-Pompe formalism.*

**We thank the reviewer for this thoughtful suggestion and we've taken it to heart.  The new manuscript features expanded text with some clarifying examples.**

*What is the physical interpretation of the statement that the solar wind “occupies the lower-right region of the CH plane”?*

**It means that virtually every one of the N! = 120 ordinal patterns in the solar wind magnetic field waveform was nearly \*equally\* represented.  If each pattern was exactly equally represented, then the permutation entropy would be the sum of -P log(P) = sum of -(1/N) log(1/N) = log(N).  This is the maximal entropy, and subsequent permutation entropies are normalized to this value.  The fact that the solar wind normalized permutation entropy is 0.95 says that every permutation was nearly equally represented.**

*Both entropy and complexity are defined here. But, although entropy is a concept which most physicists are familiar with, I believe the same is not true of complexity. What’s the physics of the statement that the laboratory turbulence examples considered here “have less  
permutation entropy and more statistical complexity”?*

**Peter had some good words here... I'd say it's a measure of the repetition of particular structures.**

*Figure 1 is hard to read; the symbols and labels are in very small print, the “downward pointing triangle” is not visible to my eyes (Is not the entropy of a precisely-defined sine wave zero?), and the “chaotic skew tent, Henon, and logistic maps” are not explained. Figure 2 is clearer, perhaps because it describes the well-known frequency spectra, but again many of the labels are very small and hard to read.*

**This should be fixed by David.  I think that my generation and older don't realize that any pdf can be magnified on the computer screen. That said, the figures should be readable at the nominal magnification scale.**  
  
**(The comment about the sine wave is an opportunity for clarification (both in the manuscript, the reply letter, and the thesis)).  While a sine wave represents one Fourier mode of the fluctuation, a sine wave represents \*more than one\* permutation of ordinal patterns.  If we use an embedding dimension of N=5 (with, therefore, 120 permutations of ordinal pattern), then a sine wave will have the ordinal pattern 1-2-3-4-5 represent during the ascending part of the waveform \*and\* the pattern 5-4-3-2-1 represented during the descending part of the waveform.  In addition, near the peaks and valleys of the sine wave, a few patterns like 1-2-3-5-4 will be represented.  Therefore, the normalized permutation entropy is slightly more than zero.**  
  
**Another simple example is the linear ramp.  This is a waveform that only has 1-2-3-4-5 represented.  Since log(1) = 0, this is a waveform with permutation entropy of zero (lower left corner of the CH plane).**

*If solar wind turbulence has less statistical complexity, why is it more “stochastic-like” (page 4 below Figure 2), and why is LAPD turbulence more “chaotic” (page 4, near the top the right-hand column).*

**(This is a misunderstanding of what we (and Rosso) mean by stochastic and chaotic.)  Our operational definition is that "stochastic" refers to fluctuations (like fractional Brownian motion fBm, and the solar wind) that have high normalized permutation entropy (near H=1).  Our operational definition of "chaotic" are fluctuations (like the deterministic maps) that have lower permutation entropy (near H=0.5), and higher complexity.**

*In summary, I think that there is the potential for new insight from the unfamiliar formalism used for the analysis, and the conclusions make physical sense. But the authors need to rewrite the manuscript to clarify and illuminate (perhaps through simple examples) the meaning of the various terms used here as well as the overall physical concepts involved. I would be willing to review a revised version.*

**We thank the referee for their careful reading of our manuscript. In particular, the suggestion of illumination through simple examples has been taken to heart.  We now feel it is ready for publication in PRE.**

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Report of Referee 2  
----------------------------------------------------------------------*  
  
*I have carefully read the manuscript by Weck et al., submitted for publication on PRE.  
  
The authors present the results obtained through the analysis of permutation entropy and complexity of three different plasma turbulence datasets. The results are very interesting, as they show the power of the data analysis technique quite convincingly and provide additional new knowledge on the systems studied here. I therefore warmly recommend their publication on PRE after a few points have been considered.  
  
However, while I do not have any major remarks on the scientific content of the work, the manuscript certainly needs some text revision and reorganization before publication.  
  
Here is a list of suggestions, comments and questions for the authors.*

*1. The Introduction section is oddly organized. After a quick introduction to the general framework, the authors describe the main results of the paper, before defining the various actors of their work. For example, they write of LAPD and SSX without having introduced the experiments. The whole second paragraph ("We compute the values...") might be irrelevant at this point, and perhaps moved forward in the paper, or, at least, made more general.*

**We agree with the reviewer's suggestion of reordering the examples (trying a new ordinal permutation if you will).**

*2. Still in the introduction, on page 2, the three datasets should be described in a dedicated section. Also, I'd rather have a different sorting of the experiments, leaving the solar wind for last (it's the only natural plasma, and it doesn't make saense to me to describe it in between two laboratory devices). Also, the paragraph describing LAPD is somewhat unbalanced with respect to Wind and SSX. I frankly do not know the device, and the text doesn't help understanding what kind of experiment it is. I'd suggest the authors to spend more words in  
the description of the LAPD.*

**We have adopted this suggestion as well, and we now have a new section describing each data set.  (I think we decided to do this... description of each device including the Wind satellite, what's measured, at what cadence (3 seconds for Wind, 1/65 microsecond for SSX)...)**

*3. Section II: although the definition of the quantities used here (PE and C) is rigorous, I think this section lacks a short intuitive description of their meaning. Could the author explain better in intuitive way the "information" meaning of the two quantities, and how they should be interpreted?*

**(Here, we should note our new words about simple examples (sine, ramp, and maximal entropy discussed with referee 1 above.  I think here (and in the manuscript, and in the thesis) is a good opportunity to note why if H=0 or H=1, then the Jensen-Shannon complexity is zero.  Here also is where a few sentences on the intuitive description of complexity, from above, should go.)**

*4. Page 3, 10 lines after eq.n (3): The authors describe the CH plane without properly introducing it. I would suggest to add a brief description of "what is the CH plane", and how it should be read (complexity vs entropy: what does it says to the reader?). Also, figure 1 is not easy to read especially if printed in BW. The markers are not easy to distinguish, so I'd suggest the authors to separate the marker style more efficiently. For example, the values for the maps could be indicated using open markers, which would make them more easily recognizable with respect to the experimental ones.*

**(Once intuitive explanations of C and H are given (ala point 3 above), then the CH plane should be described.  Here (and the thesis) is an opportunity to say why \*any\* fluctuation is bounded by the crescent moon shape (at least say that it is so).)**

*5. SECTION III: Is it possible that integration of B introduces some artifact (correlations) on the SSX time series? While it is expected that the two time series will give different results, I understand this is unavoidable, but perhaps the authors could cross-check the "direction" of the changes by performing a complementary operation on the Wind data, i.e. by estimating the dB/dt for that dataset?*

**(This is a good suggestion if it's not too time-consuming. Integration certainly correlates pairs (or triples, depending on the scheme, like Simpson's rule), but I wonder if that could explain something about where the integrated data sits on the CH plane.)**

*6. page 3, line 15 from the bottom: the description of the plot for SSX data is not very easy to read. For example, the author never mentioned the existence of 4 probes (they only talk about an array of probes in the introduction). Also, "position" and "direction" are too generically used in the same sentence, so they are a bit confusing. Perhaps "position in the CH plane" and "component" (rather dan direction) could make the reading smoother?*

**We have added a section describing the data sets used (SSX, Wind, LAPD).  Indeed, we measure the vector magnetic field (3 components) at 16 radial locations in the array (from the center to the edge). We typically report fluctuations from a single probe in the array located at the center of the wind tunnel, away from the influences of the wall.  We have also adopted the reviewers suggestion to clarify position of a probe in our plasma as opposed to position on the CH plane, as well as direction in our device versus "directions" in the CH plane.  
  
(We should be careful, for non-experts, to say "probe array" for the whole thing, "single probe" maybe for one of the 16 triples, "component" for one direction of the triple (x, y, or z)...).**

*7. page 4,figure 2: the exponents of the reference power-laws are too small and difficult to read.*

**We have modified the figures so they are readable at standard magnification.**

*8. page 4, second paragraph ("Previous work using..."): is it really OK to compare analysis of different systems done with or without using the delay?*

**(I'll leave this to the experts.  I think you have addressed this, and have a way to put all the data sets on equal footing.  The idea is to pick an embedding delay corresponding to a frequency just at the dissipation scale, ie about 1/10 microsecond for SSX, 3 seconds for Wind, etc).  This could be explained in the new data sets section (both in the paper and the thesis).)**

*9. Page 4, in the description of the solar wind results, it is odd that the fBm is less stochastic than the wind. Perhaps this point deserves some more discussion.*

**(Peter had a good explanation of this.  We need to add more discussion of the deterministic maps and fBm.)   In particular, truly stochastic fBm noise corresponds to a particular fractional Hurst exponent, and that one has normalized permutation entropy of (nearly) unity.  (all this should be in the new data set section).**

*10. In the same discussion: the higher level of stochasticity of fast solar wind is consistent with the presence of alfvénic fluctuations (which are more uncorrelated than the turbulent fluctuations), and with the generally observed higher degree of intermittency of slow wind (suggesting a more developed turbulence).*

**This is a good point.  Although we haven't checked Alfvenicity for this data set (we only have access to B), we will add a statement to the manuscript hypothesizing this possibility.  In truth, the fast and slow samples of Wind data both have H very nearly unity and C nearly zero, so it's difficult to make strong claims corresponding to fast versus slow wind.**

*11. page 4, discussion on SSX results: once again, it is to be expected that dB/dt is more complex than B. This is probably even more the case for intermittent turbulence. The difference between the "slopes" (-7/3 and -11/3) is compatible with the extra frequency coming from the derivation of B (which would give an -8/3 slope, assuming that E(B)=f^-11/3). The authors could comment on that.*

**(This is a suggestion for David.  We could just say, we appreciate the reviewer pointing this out.  We will consider this in future work.)**

*12. The discussion about the shape of the power spectra is not very clear, and perhaps should be rephrased. In my opinion, this part of the analysis is also not fully objective, and might need some correction. From a visual inspection, it is not very easy to support the authors' claims: while it is evident that the solar wind magnetic spectrum is (as well known) a power law with Kolmogorov-type exponent, and while the LAPD spectrum has a somewhat exponential shape (despite its tail is clearly shallower than exponential, and the comparison with exponential curve in the Figure highlights the discrepancy), I do not clearly see the power-law-ish behavior of the SSX spectra, which in my opinion have a very similar shape as for LAPD.  
  
I still agree with the interpretation given by the authors, but I find it only strictly valid for Wind and LAPD, while the SSX cases stays somewhere in between (as claimed), but not necessarily because a power-law is visible. Perhaps the authors should find a more precise representation for these curves, or at least present a more neutral comparison with reference curves (e.g.: compare with both power-law and exponential for all four cases). Or, at the very least, stress the approximation of the observation.*

**Getting a large separation of scales in a laboratory experiment is difficult.  We will certainly stress the approximation of the SSX observation.  That said, we have looked for exponential shape behavior in the SSX data, and find that a power-law fit is somewhat better.  (David should chime in here)**

*13. Page 5, second paragraph: just as a hint, it could be interesting to check what happens to more turbulent laboratory plasmas, as for example in RFP devices (the Italian RFX is a good example of fully developed turbulence with intermittency, see several works by Carbone,  
Antoni, Martines and co-authors).*

**We don't have access to the RFX data described by the reviewer but we do have access to liquid metal turbulence data from the Madison Dynamo Experiment.  (describe what we see)  We plan to continue this work with comparisons to other MHD-type flows, including reversed field pinches.  We may be able to obtain data from the Madison MST reversed field pinch device.**

*14. Finally, again just a suggestion, it would be very interesting to explore the role of high order moments (intermittency) besides the power spectra. In fact, since intermittency plays a relevant role in the build-up of small scale correlations, this could explain (even more than the spectral shape and slope) the differences between all experiments. It is possible that the laboratory plasmas do not show clear intermittency, because of their low level of turbulence (Re=100), but this could be interesting at least for the fast/slow wind.*

**This is an excellent suggestion, and indeed studying structure functions (higher order moments of the probability distribution function of magnetic increments) is something on our agenda. (some words from David about what we see)  
  
We thank the referee for their careful reading of our manuscript. We have adopted most of the suggestions.  We now feel it is ready for publication in PRE.**