
Report of Referee 1

The manuscript presents scaling analysis of magnetic fluctuations measured in the laboratory plasma device SSX. The authors attempt to make a connection between their analysis and the reported observations from the solar wind plasma. The focus is on the difference between scaling behaviour in the presumed inertial range and a "dissipative" range of scales. The methodology is standard - structure functions are constructed and probability density functions of fluctuations are used to infer the statistical features on different scales. While results of the paper are of some interest, the authors should be more careful when comparing two distinct plasma systems.

I have three major issues that, I think, the authors should address in the paper and a couple editorial changes, which may clarify the text.

We thank the reviewer for this close and thoughtful reading of our manuscript. We realize that attempting to make comparisons between laboratory plasma and astrophysical plasma can be a precarious endeavor and we understand that the plasmas can be very disparate. However, our main goal is to find some aspects of our laboratory plasma that a heliospheric plasma such as (but not limited to) the solar wind may have in common and explore those aspects in a controlled setting. We believe the main result of this paper is one such aspect that the plasmas share, particularly when viewed through the metric of structure functions. On the other hand, the primary results of this manuscript *are* focused on the experimental observations, so that comparisons to heliospheric-relevant plasma such as the solar wind are made to help provide context, and potentially, to help elucidate underlying physics. The new manuscript addresses some of these points as well as tackles the major and minor issues laid out by the referee.

Major issues:

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1) The paper does not address the issue of stationarity at all. There are no time traces of magnetic field measured in time and at each point in space so that the reader can assess the stationarity of the data. What is the life span of the plasma in the experiment? How does it compare with the largest temporal scale used in the analysis? Related to this, there is also a question of the origin of turbulence here. Should it be understood that the turbulence is driven on large scales, as in the solar wind, or is this setup more relevant to magnetically confined *plasmas, where the driving is on the small scales, as in drift-wave turbulence. This is absolutely critical to any interpretation of these results, I believe.*

We thank the referee for highlighting this important omission in the description of our laboratory plasma. Our plasma has a stationary period and we have observed that an autocorrelation function with a width of about 1us is found regardless of time or position within the stationarity window. Previous work has addressed the issue of stationary for the data used in this manuscript including Schaffner PPCF 2014, Brown and Schaffner PSST 2014, and Brown and Schaffner JPP 2015. We have also added some text to clarify the issue:

"The gun produces a 120μs discharge of plasma; though the plasma is pulsed, there is a window of time in which the energy input from the gun roughly balances the energy loss in the system, generating a period of stationarity of fluctuations (Schaffner et al. 2014a). This time range, from 40 to 60μs after initiation of the discharge, is extracted from each shot to form an ensemble for analysis."

Also, while the structure functions are computed up to the largest time span possible given this 20us window, the main results stem from time delays at about an order of magnitude smaller than this stationarity window. A line has been modified to clarify this point:

"Increments are then increased in multiples of these minimum values. Maximum separation values are limited by physical distance or data acquisition time span, as well as the ability to generate enough statistics for a valid calculation, *though the main focus of results here will be on scales much smaller than the available time span to avoid issues of non-stationarity*."

Regarding the origin of turbulence, the energy injection occurs at a large scale and cascades down to smaller scales. In this case, large scale magnetic structures are generated by the plasma gun and during their evolution and relaxation process, the energy in these structures is transferred to smaller scales along the lines of a traditional turbulent cascade. It is unlike the situation in strongly magnetized, magnetically confined plasmas, where coherent gradients can be established and generate instabilities at scales smaller than the largest or outer scale. In fact, our lab has been exploring the nuances of the distinctions between more conventional fluid-like MHD turbulence and drift-wave turbulence where the cascade paradigm does not really apply. It is thought by some that drift-wave turbulence tends to produce more exponential-like spectra, rather than power-law like spectra and that this distinction may be related to how and where energy is injected and cascaded. See recent work on this topic (Weck PRE 2015, Maggs PPCF 2015). A line has been added in the description of the laboratory plasma to emphasize this point as well as a reference to Schaffner ApJ 2014 where the issue of cascade mechanism is also discussed:

“A turbulent cascade is generated by the injection of large scale (size of the column radius) magnetic structures by the gun which evolve and relax under the constraint of constant magnetic helicity inside the flux-conserving column, transferring energy to smaller scales (Schaffner et al. 2014c).”

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2) The distribution functions of spatial fluctuations could be well approximated by superposition of two Gaussian distributions, at least visually it appears so. Some features in the temporal distributions also appear to support a hypothesis of two distinct populations. Is there any way to verify what is the origin for these two classes of fluctuations?

As noted in the caption, the effect of the asymmetric distributions, particularly of the smallest scale spatial PDF in the upper right corner, is likely due to the boundary effects of the edge of the flux conserver to the outermost probe locations. The boundary may in effect be creating a secondary population of fluctuations, perhaps due to the effect of image currents in the copper. Better, more isolated spatial measurements is something that our lab is striving for in future investigations. However, the focus of this manuscript is on the tails of the distribution which are clearly non-Gaussian. Even if all the distributions could be better fit to a dual-Gaussian rather than a single Gaussian, the combination would not produce the non-Gaussian nature of the wings, which lies at the heart of this analysis.

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3) The authors cite many papers related to solar wind studies, for example these by Kiyani et al. If I am not mistaken, in his analysis Kiyani quantified the conversion rate to mono-fractal scaling for some solar wind fluctuations, which appeared multi-fractal only due to a lack of statistical significance for some large amplitude fluctuations (not enough of these measured). I would strongly encourage the authors to see how scaling properties of their fluctuations vary as the most intermittent events are excluded from the sample.

While there have been some publications on this topic which have explored the use of conditioning the structure functions such as Hnat GRL 2003 (referenced in the paper), we have opted to not use this technique for a couple of reasons. There is certainly a philosophical reason for including as much data as possible and there can be danger in conditioning data to exclude events which could in fact be a primary driver for the physics being investigated. More practical reasons for not excluding events include the fact that our data does not register as extreme a fluctuation level as that seen in the solar wind (Kiyani ApJ 2013) or the magnetosphere (Hnat GRL 2003) and that given the nature of our experiment, we can produce arbitrarily large amounts of statistics for a given configuration (unlike in observational data where analysis regions must be selected from the ever-changing

environment of the solar wind). Note in the PDFs presented in these papers, the range of fluctuations is up to 10 or 20 standard deviations whereas fluctuations in our data do not exceed a sigma of 6. We have also found that the statistical nature of our plasma does not improve significantly beyond an ensemble of 40 shots or so. Given these parameters, we feel that our results based on inclusion of all fluctuations are robust regardless of conditioning.

Minor issues:

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*1) On page 3 the sentence "A flat line is the extreme case and indicates a lack of intermittency in the signal.", is in conflict with some earlier statements about the intermittence and scaling. The straight line with the slope m*p, where p is order of the moment computed as function of delta x, would not mean that the signal is intermittent, just indicates mono-scaling.*

This issue is addressed later in that same paragraph: "It should be noted, however, that this trend is not a perfect predictor of the presence of intermittency. For example, it can be shown that a time-series of fractional Brownian motion (fBm) can be constructed with the same structure function slope (Hnat et al 2003) but fBm produces a Gaussian probability distribution of increments. However, when intermittency is known to be present based on the PDF of increments (as is true for this SSX dataset) then the relationship between slope and degree of intermittency generally holds true."

2) In the caption of figure 1, I think there is a misunderstanding in the sentence: "Both temporal and spatial PDFs exhibit non-Gaussian tails indicating intermittency...". Previously, it was stated that K41 theory predict mono-scaling, that is lack of intermittency. Surely, K41 theory also predicts non-Gaussian PDF of fluctuations (in velocity, in this case), since the skewness, S ~ <(dv)^3>, is not 0.

There is a distinction between intermittency and scaling. Intermittency means that there is a preponderance of large fluctuations beyond what would be expected given a Gaussian distribution. In the data presented here, intermittency is almost always observed as the PDFs of increments show non-Gaussian tails (literally more counts of large fluctuations than would be expected for a Gaussian distribution). In contrast, the scaling of the fluctuations reflects the relative sizes of the structures at different scale lengths (or times). A fluctuation can be both intermittent and mono-fractal as is the case for K41 based turbulence. This just means that the non-Gaussian fluctuations have a self-similar size as the scale is changed. A Gaussian based fluctuation is not intermittent, but is mono-fractal (self-similar). For this field, it is well-accepted that fluctuations which exhibit non-Gaussian tails are thus intermittent.

Additionally, a line has been reworded to better reflect this distinction and to clarify the description:

"The work has led to the development of models which attempt to reconcile the observation of intermittency *and* non-self-similar statistics in turbulence fluctuations with the 1941 Kolmogorov turbulence model which implies a self-similar fluctuation structure, and which can be described in terms of a monofractal scaling exponent(Kolmogorov 1941; Frisch 1995).

3) I am a bit worried about the upper panel of Figure 4, where the scaling of fluctuations in B_z is a non-monotonic function of the order. Any comments on this?

This effect is due to a slight drawback of this analysis. Due to subtle variations in the structure functions computed for the various versions of the measurement, the curve of the structure function can have breakpoints at slightly different points in tau space. As the order of the structure functions is increased, these slight variations are extremely magnified. In order to better address this, slightly different regions of the structure function should really be fit to each structure function to reflect the scaling. However, this can add complication to the interpretation the results as well. Thus, we have decided to keep the fitting range constant and let the analysis proceed as it will. It should be noted that this is why focus is placed on the Bmod results for much of the rest of the results. The aggregate nature of Bmod (versus separate components) helps to insulate the analysis from these types of subtleties. Moreover, the physical mechanisms ultimately of interest (current sheets, dissipation mechanisms) are going to be more dependent on the aggregate nature of the B-field (i.e. Bmod) rather than of the individual components. That said, the Bz results do highlight the limits of this analysis technique and that pursuit of structure functions of high orders should be done carefully. For the purposes of this manuscript however, the exact form of the curve was not a focus, but more whether the curve was linear or non-linear.

We again thank the referee for a detailed and thorough review. We believe that our clarifying responses in this document and in the updated version of the manuscript now make this paper suitable for publication.