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Nonlinear Processes in Geophysics
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Interactive comment on "Derivation of the entropic formula for the statistical mechanics of space plasmas" *by* George Livadiotis

Anonymous Referee #2

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In this paper, the author present results which show that Tsallis entropy —whose maximum occurs when velocities follow the kappa distribution— can be derived for plasmas where entropy and energy are additive.

It is an interesting paper which deserves publication in this journal, but there are some issues which are not clear, and that need to be solved before accepting it. They are basically precisions on some statements or arguments that should be more explicit to follow the conclusions of the manuscript.

1. Page 1. Line 2. It is said that the fact that space plasmas follow kappa distributions is a "vastly different statistical behavior between classical systems and space plasmas". The sentence suggests that space plasmas are very special in

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this sense, but they are rather just one example of a large family of systems where non-Maxwellian behavior is found. Kappa-like or Tsallis-like distribution functions can be found in spin systems, high-energy physics, turbulent fluids, etc., as well as many examples in biological or social systems. So this should be put in the proper context.

2. Page 1. Line 16. "The induction of any type of correlations. . . departs the system from thermal equilibrium to be re-stabilized to other stationary states. . . described by kappa distributions."

   This sentence is too strong. When correlations are absent, one should expect that the thermal equilibrium is Maxwellian. However, is there any guarantee, in general, that (a) any correlation leads to a non-Maxwellian equilibrium; (b) if it does, the final state is described by a kappa distribution?

   Besides, is it always correct to say that non-Maxwellian distributions mean absense of thermal equilibrium? Isn't it possible that a system reaches thermal equilibrium (in the sense that there is no further flow of energy between its components and with its surroundings) while having correlations?

3. Page 2. Line 34. The absense of collisions is not necessary to preserve correlations. On the contrary, they may well be the reason to preserve them.

4. Page 3. Line 1. Again, energy can be conserved in the presence of collisions.

5. Page 3. Line 1. In principle, energy can be conserved in systems where the energy cannot be separated as the sum of individual particle energies. Please explain.

6. The main claim in the paper seems to be supported by the final paragraphs in Sec. 2 (pages 4 and 5). Although the Tsallis entropy is in general non-additive, there are certain correlations, depending on a certain function $g(x)$, which make

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it additive again. And then an expression for the function g is given, which leads to the Tsallis entropy.

However, this leaves the impression that this is a particular case, which allows to recover the Tsallis entropy, but does not help to understand why the Tsallis entropy should be the "right" form to describe correlated systems.

Can all physically acceptable correlations be expressed as $p_{ij}^{A+B} = g^{-1}[g(p_i^A) + g(p_j^B)]$? And is there any argument why one would expect that this holds specifically for plasmas? (Or space plasmas, which is the subject of this paper.)

One could make an *a posteriori* argument, since Tsallis entropy is known to lead to kappa distributions naturally, but the paper seems to make more general claims.

Given the above, some of the sentences in the conclusions are not clear. It says that "The paper resolved a basic problem about the origin of the distributions. . . in space plasmas", and that the q-entropy can be derived by considering additive energy and entropy. However, this seems to be true as long as the assumptions on the correlations [Eq. (21)] are correct, and there is no argument on its validity either for general systems or for plasmas in particular. Thus, it is not clear if the paper resolves a basic problem.

Please make more explicit arguments for these statements.

7. Page 9. Line 11. Entropy is stated to be symmetric on probabilities, arguing that "none of the probability components should have special effect on the entropy". This is too vague and should be rephrased. In the canonical formalism some states are more probable that others. What does "special effect" or "equal weights" means, then? Maybe it is an argument on the states rather than on the probabilities: relabeling the states does not change the entropy?

8. Tsallis entropy was proposed in the late 80s, and it was always proposed as a way to model the ubiquity of power-law/kappa distributions, understanding that they

are "equilibrium" (maximum entropy) configurations, but for an entropy different to the Boltzmann's one.

It is thus not clear why the paper says in the conclusions that "it was just in the last decade that was completely understood that the statistical origin of these distributions is not the Boltzmann-Gibbs' classical statistical mechanics."

Please explain.

A few additional formal issues:

1. Page 5, line 4: "both the formalisms".
2. Page 9, line 5: "these component"
3. Page 10, line 15: "$WA \neq WB$".

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