MS No.: npg-2017-54
Title: "Derivation of the entropic formula for the statistical mechanics of space plasmas"
Author: George Livadiotis

Respond to Referee #2

The author is grateful for your valuable comments that have improved the paper significantly. Thank you also for finding the paper interesting and that deserves publication. The revised version has been prepared by taking care all of your comments and resolving the confusing issues you have mentioned. Below there is a reply to each of your comments separately.

Together with this response, we have included a "track changes" version of the revised manuscript. In the "track changes" version we mark all the changes-corrections in regards to your comments:

Again, thank you for your valuable comments and the effort to improve this work.

George Livadiotis

Reply to each comment:

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 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.

The referee is right. The text has been revised accordingly. Thanks!

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;

Yes. This has been proved in Livadiotis & McComas 2011b, and generalized further in Livadiotis 2015c and 2017a, Chapter 5.

(b) if it does, the final state is described by a kappa distribution?

Yes! The existence of particle stationary states characterized by both (i) temperature, and (ii) correlations, means necessarily the formation of kappa distributions (or, combinations thereof). Particle systems, with or without correlations, may exist in other formulations, but they will not be characterized by a physically meaningful definition of temperature as follows from thermodynamic laws.

Besides, is it always correct to say that non-Maxwellian distributions mean absence of thermal equilibrium?

If the system is thermodynamically stabilized into a stationary state, then any non-Maxwellian distribution means: the distribution can be written as a combination or superposition of kappa distributions, the existence of correlations, and the system is not at 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?

That is actually what happens when the system is reaching a stationary state. The classic understanding of thermal equilibrium is a stationary state, but not all stationary states are in thermal equilibrium. The special about it is no correlations and Maxwellian behavior. All the κ- (or q-) stationary states could have similar characteristics with thermal equilibrium but with the highlighting property of local correlations among the particles.

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.

Indeed! While thermal collisions destroy correlations, organized collisions may not. However

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

In the presence of collisions, energy may be conserved or not. But in the absence of collisions it can be certainly be conserved, unless other particle interactions are taken place.

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.

You are right! The text has been revised. Thank you!

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 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 $p_{A+Bij} = g_{-1}[g(p_{Ai}) + g(p_{Bj})]$? 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.

- The BG entropy and the produced Maxwell-Boltzmann distribution follow a certain correlation type, that is, no correlation, as given by Eq.(11): $\ln(p_{ij}^{\mathrm{A+B}}) = \ln(p_i^{\mathrm{A}}) + \ln(p_j^{\mathrm{B}}) \;\Rightarrow\; p_{ij}^{\mathrm{A+B}} = p_i^{\mathrm{A}} \cdot p_j^{\mathrm{B}}$. Section 2 is based on one of the following three equivalent assumptions: (i) BG entropic formulation; (ii) Maxwell-Boltzmann distributions in the canonical ensemble; (iii) no correlation or independence, as given by Eq.(11).

- The Tsallis entropy and the produced (canonical ensemble) q-exponential or kappa distributions follow a certain correlation type, that is the one given in Eq.(21): $\ln_q[(p_{ij}^{\mathrm{A+B}})^{-1}] = \ln_q[(p_i^{\mathrm{A}})^{-1}] + \ln_q[(p_j^{\mathrm{B}})^{-1}] \;\Rightarrow\; (p_{ij}^{\mathrm{A+B}})^{q-1} = (p_i^{\mathrm{A}})^{q-1} + (p_j^{\mathrm{B}})^{q-1} - 1$. Section 3 is based on one of the following three equivalent assumptions in the canonical ensemble: (i) Tsallis entropic formulation; (ii) q-exponential or kappa distributions; (iii) Special Correlation type called also q-independence, as given by Eq.(21).

- There is no assumption on correlations to produce the main claim of the paper. The only assumptions are the additivity of the entropy and energy. Section 4 is based on these assumptions to produce that Tsallis entropy describes particle systems such as space plasmas; thus, kappa distributions too; and thus, the correlations of Eq.(21), too. In addition, superposition of stationary states can be described by more complicated distributions or correlations (e.g., see Livadiotis & McComas 2013a; Livadiotis 2017a, Chapter 4).

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?

Correct, it is an argument on the states, and the text has now been rephrased. Thanks!

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.

Empirical Kappa distributions have been used without any statistical framework for almost half a century. The connection of these distributions with the statistical framework of non-extensive statistical mechanics is the one that has been completed about a decade ago. Corrections have been made to resolve the confusion. Thanks!

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 6= WB”.

All corrected. Thanks!