Letters on the EPN 36/6 Special Issue

Editors note

In the 2005 Special Issue of EPN on "Nonextensive statistical mechanics: new trends, new perspective" (Vol. 36/6), the guests Editors amply developed their views on the subject through 15 features over an exceptional set of 48 pages. It seems that some readers from the same community see the field in different ways. EPN is too limited in space to open a forum of discussion in its pages. The subject will be closed for EPN after publication of the following two letters and a comment with its specific answer. ■

Letter to the Editors:

Roger Balian (SPhT • Saclay • France) and Michael Nauenberg (Physics Dept., University of California • Santa Cruz)

The 2005 special issue of Europhysics News (Vol. 36/6), entitled " Non-extensive statistical mechanics: new trends, new perspectives", contains a number of papers which make either misleading or incorrect claims. For the past decade one of the guest editors of this issue, C. Tsallis, and his collaborators and followers have generated a vast literature on this subject, organized conferences, and published several books. All of this promotion gives the wrong impression that the topic of your special issue is well established in statistical physics.

For systems in thermal equilibrium, which are characterized by a temperature T, the applications of the so called q-entropy extension of Boltzmann-Gibbs statistics are flawed, because the maximization of the q-entropy violates a fundamental law of thermodynamics for $q \neq 1$: then, temperatures that must be equal for systems in thermal contact cannot even be defined [1]. This flaw has recently been recognized by Tsallis, who has admitted that " the $q \neq 1$ theory ... by no means applies to thermal equilibrium" [2]. Unfortunately, this disclaimer is nowhere mentioned in your special issue. Instead, in one of the articles entitled "Nuclear astrophysical plasmas: ion distribution functions and fusion rates" [3], the authors apply the $q \neq 1$ statistics at a fixed temperature T to discuss in a purely hypothetical way how this might give rise to anomalous fusion rates in the interior of stars — while incorrectly claiming that such "distributions different from the Maxwellian one can be obtained axiomatically from non-standard, but mathematically consistent versions of statistical mechanics that use q-entropies" [3]. Indeed, a large bulk of the papers on q-entropy have been addressed to systems in thermal equilibrium, but to date the conclusions of these papers have not been retracted. On the contrary, these papers continue to be included in a "regularly updated bibliography, http://tsallis.cat.cbpf.br/biblio.htm", as publicized by the reference (2) of the article on "Extensivity and entropy production" [4] in this issue.

Concerning the application of the non-extensive q-entropy formalism to non-equilibrium stationary systems with long range forces, to quasi-stationary states, and to chaotic maps, the value of q is selected only through disputable numerical fits, never through theoretical arguments. On the other hand, not a single paper in this special issue gives a hint about the existence of strong controversies and disagreements on the validity of these applications.

1/ For example, the authors of the article entitled "Non extensive thermodynamics and glassy behaviour" [5] try to explain why molecular dynamics simulations of the mean-field coupled rotator model exhibit long-lived quasi-stationary states which violate Boltzmann statistics. They claim that their results show that " Tsallis' generalized formalism is able to characterize the dynamical anomalies". But they fail to point out that other physicists have reached exactly the opposite conclusion [6].

2/ Likewise, the author of the article entitled "Critical attractors and q-statistics" [7] claims that "it has been recently corroborated that the dynamics at the critical attractors ... obey the features of the q-entropy". However, in a recent paper on the same subject [8], P. Grassberger shows that "recent claims for the non-stationary behaviour of the logistic map at the Feigenbaum point based on non-extensive thermodynamics are either wrong or can be easily deduced from well known properties of the Feigenbaum attractor", and concludes that " non-extensive thermodynamics is (at least at the onset of chaos) just a chimera".

3/ In a paper of this issue entitled "Atmospheric turbulence and superstatistics" [9], the authors show that the q-entropy statistics fails to fit both the data for atmospheric turbulence, Fig. 2, and the data for Taylor-Couette turbulence, Fig. 3. But this failure is hidden from the casual reader, because the dotted curves are inconspicuously labelled "Gamma" instead of "Tsallis" or "q-entropy" Furthermore, it is claimed that the "superstatistics approach gives a plausible physical explanation to the entropic index q"; in fact, the authors define an index q related to temperature fluctuations, but this index coincides with that of the q-entropy only for the so-called Gamma statistics which does not fit the data at all. Moreover, the lognormal statistics, which does fit the turbulence data, is not linked up to the topic of this special issue, which is non-extensive statistical mechanics.

At best, q-statistics can be viewed as a phenomenology without any physical justification which provides interpolations between exponential and power law dependences in attempts of fitting data.

In a Brazilian newspaper, A Folha de Sao Paulo, March 3, 2002, Joel Lebowitz was quoted as saying "that the promotional movement around it [Tsallis q-entropy] has been more harmful than useful for science". We believe that this is particularly true for the Latin American statistical mechanics community, where many researchers have been misled into working on this "chimera". It is unfortunate that this special issue of Europhysics News has lent itself to this " promotional movement". ■

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Comment to

"Nonextensive Thermodynamics and Glassy Behaviour in Hamiltonian Systems" by A. Rapisarda and A. Pluchino, Europhysics News 36/6, 202 (2005).

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The dynamics of the Hamiltonian Mean-Field (HMF) model [1] shows many intriguing non-equilibrium behaviours. In particular, it has been reported several times that the system get stuck into quasi-stationary states (QSS), whose lifetime increases with system size. As correctly pointed out by C. Tsallis and co-workers (see e.g. Refs. [2]), the presence of such non-equilibrium states is tightly linked to the fact that the infinite time limit and the thermodynamic limit do not commute in systems with long-range interactions. However, contrary to what is claimed in Ref. [3], the non-extensive statistics approach does not convincingly "explain" any of these non-equilibrium behaviours.

Two main quantities have been tested up to now: velocity distribution functions and correlation functions. In Ref. [4], the authors fit single particle velocity distribution functions in QSS using q-exponentials. They obtain a value of the index q = 7. In Ref. [3], an analogous fit of correlation functions with q-exponentials gives values of q between 1.1 and 1.5.

The fact of being in a non-equilibrium state could in principle allow the use of an entropy other than Boltzmann-Gibbs. However, there is up to now not a single paper which justifies the use of non-extensive entropy for the HMF model. Hence, there is no compelling reason of using q-exponentials as a fitting function. Moreover, as recalled above, different values of q are reported in the literature for the same model and the same physical conditions.

A general alternative approach has been introduced to explain the presence of QSS in systems with long range interactions. This approach begins by performing first the thermodynamic limit and then looking at the time evolution. It leads to associate to the HMF model appropriate Vlasov and kinetic equations. This method is fully predictive and has been extensively exploited in Ref. [5] to obtain the Vlasov equation predictions for the HMF model. Velocity distribution functions of QSS have been analysed, reaching the conclusion that they cannot be fitted by q-exponentials. This conclusion has not been questioned so far in the literature.

Restricting to homogeneous QSS, this approach also allows to derive properties of the correlation functions, deducing them directly from the HMF model [6]. Such homogeneous states are of paramount importance, since they appear to be "attractive" for a large class of initial conditions. For instance, it can be shown that the plateau values of the magnetization M_0 shown in Fig. 1 of Ref. [3], all converge to $M_0 = 0$ when N increases, which is a distinctive sign of homogeneity.

The Vlasov equation approach is just in a beginning stage. However, the already existing results are encouraging and we believe that the difficulty of treating inhomogeneous QSS is of technical nature. This problem will be solved in the near future. Hence, the conclusion of Ref. [3]: "However the actual state of the art favours the application of non-extensive thermostatistics to explain most of the anomalies observed in the QSS regime" is highly questionable. As a final remark, we think that, as physicists, we should pay great attention to the difference between "fitting" and "explaining". ■

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Reply to the Comment

by T. Dauxois, F. Bouchet, S. Ruffo on the article by A. Rapisarda and A. Pluchino, Europhysics News, 36/6, 202 (2005)

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In the comment by T.Dauxois et al. [1] the authors question our application of nonextensive statistical mechanics proposed by Tsallis [2] and discussed in [3] to explain the anomalous dynamics of the Hamiltonian Mean Field (HMF) model. More specifically they claim that the explanation of the metastability found in the out-of-equilibrium dynamics is only a fitting procedure and is also in contrast with a previous application done in ref. [4]. This criticism mostly relies on recent studies based on the Vlasov approach and discussed in refs. [5, 6], where the authors claim to explain the anomalous behaviour of the HMF model in terms of a standard formalism. In order to reply to this comment we want to stress a few numerical facts and conclude with some final considerations.

1/ In our numerical simulations we consider always a finite number of particles, which plays the role of a collision term absent in the Vlasov equation. This collision term is very important since it drives the systems towards the equilibrium.

2/ In our paper [3], we use a finite initial magnetization which leads to a violent thermal explosion. The quasistationary state which follows is microscopically nonhomogeneous, with a hierarchical cluster size distribution [7]. The Vlasov-like approach proposed in [5] has severe problems in dealing with these inhomogeneities. Up to now all the derivations presented in the literature start from a homogenous metastable state, where no violent relaxation occurs [8]. In this case, the decay of the velocity correlation function is very fast (almost exponential), in remarkable contrast to what observed for an intial finite magnetization, where a q-exponential (with q > 1) is found [3, 8].

3/ The predictions of Tsallis thermostatistics [2] are successfully compared with the numerical results, as shown in figs.3,4 of ref. [3]. In this case, it is not true that we perform simply a fit of numerical data. By means of Tsallis statistics and using q-exponentials to reproduce extremely well the anomalous diffusion behaviour, we can predict the correlation decay with great precision and vice-versa. At variance, the results of the approach proposed by Dauxois et al. [5] have not been tested with numerical simulations, so that no real prediction can reasonably be claimed.

4/ The results presented in [3] are not in contradiction with previous papers since they refer to velocity correlations decay and not to the marginal velocity probability density functions discussed in [4], where the entropic index extracted was only an effective one and indicated a strong departure from a Gaussian shape. On the other hand, the possible application of Tsallis statistics in long-range Hamiltonian systems is confirmed by several other studies [9].

In conclusion the HMF model is a paradigmatic example of a large class of long-range Hamiltonian systems which have important physical applications, ranging from self-gravitating systems to plasmas. The nonhomogeneous metastability observed for the HMF model goes undoubtedly beyond standard Boltzmann-Gibbs statistical mechanics and has a dynamical origin, therefore a new kind of kinetics should be used [10]. In general, adopting different perspectives is a useful procedure to shed light on a tricky problem. Tsallis statistics is a good candidate to explain and interpret the strange behaviour of long-range Hamiltonian systems, and this is not in contradiction with other possible formalisms, including that one of Dauxois et al. (analogously the Langevin and the Fokker-Planck phenomenological formulations are not in contradiction with Boltzmann-Gibbs statistical mechanics). We have also successfully applied techniques normally used for glassy systems [7], and interesting connections with Kuramoto model and the synchronization problem have been advanced [8]. In any case further work is needed to understand in detail this intriguing new field.

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Letter to the Editor:

"On fallacies concerning nonextensive thermodynamics and q-entropy"

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On the question in the special issue 36/6 (2005) of Europhysics News it should be noticed that claims about the formulation of a grand-generalization of non-extensive thermodynamics, based on a so-called q-entropy, have given rise to a flood of papers which are receiving critical scrutiny: Several distinguished researchers in the area have pointed out the misconceptions involved in such an approach.

In a recent publication Peter Grassberger stated [1] that during the last decade has appeared a vast literature on a "new non-extensive thermodynamics" (NNET), which uses a maximum entropy principle with the Shannon entropy replaced by the Havrda-Charvat [2] (Tsallis) entropy [3,4], an approach plagued by misconceptions about basic notions [1,5-9]. The basic serious error committed by the followers of such line of thought, consists in considering that they have developed a generalization of the traditional, well established, and physically sound, statistical thermodynamics (founded by Maxwell, Boltzmann and Gibbs), which would cover all physical systems in nature, particularly including those with complex behaviour which they wrongly consider to be outside the scope of the latter.

The fundamental misconception behind this consists into identifying Havrda-Charvat structural α-entropy [2] (later called Tsallis q-entropy) with the thermodynamic entropy of a physical system [6]. Michael Nauenberg has forcefully shown that it is in fact inconsistent with the laws of Thermodynamics: He points out that for a q-index different from 1 it has been claimed that it leads to a formalism that is consistent with the laws of Thermodynamics, but, however, it can be shown that the joint entropy for systems having different values of q is not defined in their formalism, and consequently fundamental thermodynamic concepts such as temperature and heat exchange cannot be considered for such systems. Moreover, for $q \neq 1$ the probability distribution for weakly interacting systems does not factor into the product of the probability distribution for the separate systems, leading to spurious correlation and other unphysical consequences [5].

Related to these aspects of the question, we recall that in Statistical Mechanics the probability distribution (statistical operator) usually derived from heuristic arguments, can also be derived from a variational method, which is related to Information Theory [10-13]. It consists into making extremal — a maximum —, subject to certain constraints, a functional (superoperator) of the probability distribution (statistical operator). Such quantity, first introduced in Shannon's Theory of Communication [14], can be referred-to as a *measure of information*. It has also been called statistical measure and entropy, with the understanding that it is *informational-entropy*, and once again we emphasize the essential point that the different possible informational-entropies are not to be interpreted as the thermodynamic entropy of the physical system. Richard T. Cox has noticed that the meaning of such entropies is not the same in all respects as that of anything which has a familiar name in common use, and it is therefore impossible to give a simple verbal description of it, which is, at the same time, an accurate definition [15]. Edwin T. Jaynes has also commented that it is an unfortunate terminology, and a major occupational disease in that there exists a persistent failure to distinguished between the informational-entropy, which is a property of any probability distribution, and the experimental entropy of thermodynamics, which is instead a property of the thermodynamic state: Many research papers are flawed fatally by the authors' failure to distinguish between these entirely different things, and in consequence providing nonsense results [16], as it is the case with NNET.

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