Reply to C. Tsallis's "Comment on Critique of q-entropy for thermal statistics by M. Nauenberg"

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May 13, 2003

Abstract

In this note it is shown that in his "Comments", Tsallis did not point out any flaws in the main criticism of my paper, namely, that the nonextensive q-entropy formalism fails to satisfy the zeroth law of thermodynamics. Details are also given of a rigorous thermodynamic proof which demonstrates that, contrary to Tsallis's assertion, the application of a nonextensive formalism to black-body radiation does not lead to the well known T^4 Stefan-Boltzman law.

In the abstract of his paper [1], and in a section entitled "About thermal contact between systems with different values of q and the 0^{th} principle of thermodynamics", Tsallis acknowledges that the essential point of my critique [2] is that the q-entropy formalism does not give a prescription to obtain the equilibrium temperature for such systems, as is required by this principle. I paraphrased this failure of the formalism by stating that a conventional thermometer satisfying Boltzmann-Gibbs (BG) statistics could not measure the temperature of a q-entropic system with $q \neq 1$, and concluded that "the laws of thermodynamics would therefore fail to have general validiy" unless all systems have the same value of q. Since there are systems in thermal equilibrium for which $q = 1$, corresponding to BG statistics, my conclusion implies that all systems *in thermal equilibrium* must have $q = 1$, and thus correspond to BG statistics. But rather than point out supposed flaws in my criticism, Tsallis responded in [1] by presenting the results of a molecular dynamics simulation of coupled rotators with long range interactions, which are known to exhibit a quasistationary state below the transition temperature [3]. Tsallis claimed that this state has "*non* Boltzmannian statistics" without, however, providing any evidence, and then purported to show that a thermometer constructed out of rotators with nearest-neighbor interactions that satisfy BG statistics measures the temperature of this state. Tsallis's temperature is twice the mean kinetic energy of the rotators, but this definition is only justified if the model satisfies BG statistics. However, Tsallis admits that this is not the case for the rotator-model quasistationary state. Moreover, the general thermodynamic definition of temperature (which is also introduced in the q- entropy formalism) is the derivative of the entropy with respect to the energy, but neither the entropy nor this derivative was evaluated by Tsallis's in this simulation. The details of this simulation were discussed in a separate paper [4], but not a shred of evidence was presented that this quasistationary state satisfies the statistics associated with q-entropy. In conclusion, Tsallis's Comments [1] cannot in any way be regarded as a rebuttal to my main criticism of the q-entropy formalism in reference [2].

As an example of one of the many unphysical results which have been obtained during the past decade by applying the q-entropy formalism to problems in thermal statistics, I discussed at some length in my critique the case of black-body radiation. In particular, I pointed out that this formalism leads to a violation of the well-known T^4 Stefan-Boltzmann law. In his Comments [1] Tsallis responded that it can be "*trivially* shown that the T^4 proportionality law remains the same for all energy statistical distribution (hence not only the BG ones) as long as...for photons the distribution depends on the light frequency ν and the appropriate temperature T, only through [the ratio] ν/T". But as I explained in my critique, for a *nonextensive* theory both the energy and the entropy density depend not only on ν/T, but also (by definition) on the volume V of the cavity holding the black-body radiation. Otherwise the theory would be *extensive*. This volume dependence implies that these distributions must be functions not only of a dimensionless ratio $h\nu/k_B T$, but also of another dimensionles ratio $V/a(T)^3$, where

$a(T)$ is a length parameter, characteristic of the system, which depends on the temperature T. By dimensional arguments or from ordinary statistical mechanics we know that $a(T) \propto hc/k_B T$ is the mean thermal wavelength of the photons.

To show the fallacy in numerous publications of Tsallis and others (for references, see [2]) who argue that a *nonextensive* formalism lead to the T^4 Stefan-Boltzmann law, I give here the details of a thermodynamic derivation of this law in such a formalism. Suppose that $U/V = f(T)V^{\delta/3}$ and $S/V = g(T)V^{\delta/3}$, where U is the energy, S is the black-body entropy in a cavity of volume V, $f(T)$ and $g(T)$ are unknown functions of T, and the exponent δ is a constant which characterizes a nonextensive formalism. Inserting these expressions into the well known thermodynamic relation

$$dU = TdS - pdV, \tag{1}$$

where p is Maxwell's isotropic electromagnetic radiation pressure $p = (1/3)U/V$, one obtains the equations

$$\frac{df(T)}{dT} = T\frac{dg(T)}{dT} \tag{2}$$

and

$$(4+\delta)f(T) = (3+\delta)Tg(T). \tag{3}$$

These two equations can be readily solved to give

$$f(T) \propto T^{4+\delta} \tag{4}$$

and

$$g(T) \propto T^{3+\delta}. \tag{5}$$

Hence, contrary to Tsallis's assertions, this general proof demonstrates that to obtain the Stefan-Boltzmann law, both the energy and the entropy of black-body radiation must be extensive, i.e. $\delta = 0$. Indeed, Boltzmann made this assumption in his original 1884 derivation of this law [5]. I would also like to point out that already in 1916, Einstein showed that Planck's formula for the black-body energy density, which leads to the Stefan Boltzmann law, can be derived from his quantum transition probabilities for the emission and absorption of radiation [6]. These transition probabilities lead to the conventional Bose-Einstein statistics [7], and not to the modified versions which have been obtained by various applications of the q-entropy formalism to black-body radiation (for references, see [2]). Hence these applications are also inconsistent with the laws of quantum electrodynamics.

As I have already indicated in the title of my paper, my critique of the q-entropy formalism was confined only to its applications to systems in *thermal* equilibrium. Tsallis, however, devoted a major part of his Comments [1] to applications of this formalism to non-thermal problems such as the logistic map, the standard map, growth models, etc. These applications, however, are not relevant to my critique, and therefore I will not discuss them here.

References

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