Comment on "Critique of *q*-entropy for thermal statistics"

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It was recently argued [M. Nauenberg, Phys. Rev. E **67**, 036114 (2003)] that the theory sometimes referred to as nonextensive statistical mechanics has no physical basis, for a considerable variety of reasons, including the impossibility of measuring the temperature out of the Boltzmann-Gibbs (BG) theory. We comment here on virtually all the physically and mathematically relevant issues, and point out what we consider to be severe inadvertences contained in that paper. In particular, we factually argue, through computer simulations, the validity of the zeroth principle of thermodynamics, and of the basic rules of thermometry for nonextensive systems. This fact further supports the possible connection with the thermodynamics of nonextensive statistical mechanics, which is already known to be consistent with the first, second, and third principles. All the foundational steps (e.g., the uniqueness of the entropy and the stationary state distribution) have already been established for nonextensive thermostatistics on similar grounds than those long known for BG statistics, the former corresponding to power laws (expected for long-range interactions when size N diverges *before* time t), and the latter correspond to the BG exponential law (expected for long-range interactions when N diverges *after* t, as well as for short-range interactions in *any* diverging order for N and t). We conclude that the invalidating arguments made by Nauenberg by no means apply.

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Recently published [1] was a quite long list of objections about the physical validity for thermal statistics of the theory sometimes referred to in the literature as *nonextensive statistical mechanics*. This generalization of Boltzmann-Gibbs (BG) statistical mechanics is based on the following expression for the entropy:

$$S_q = k\,\frac{1-\sum_{i=1}^{W} p_i^q}{q-1} \quad \left(q \in \mathcal{R}; S_1 = S_{BG} \equiv -k\sum_{i=1}^{W} p_i \ln p_i \right). \tag{1}$$

For time and space economy, here I will address only a few selected points, hopefully the physically and/or logically most relevant ones, within the long list of objections and critical statements in Ref. [1].

The nonextensivity of the entropy S_q: The entropy S_q is nonextensive for independent systems [see Eq. (6) of Ref. [1]], *which by no means implies that it cannot be extensive in the presence of correlations at all scales*. In Ref. [1] there is no clear evidence of taking this fact into account in what concerns the validity of the q-thermostatistics. It is nevertheless of crucial importance, as we illustrate now for the simple case of equiprobability (i.e., $p_i = 1/W$, $\forall\ i$). In such a simple situation, Eq. (1) becomes

$$S_q = k \ln_q W \, [\ln_q x \equiv (x^{1-q}-1)/(1-q); \quad \ln_1 x = \ln x]. \tag{2}$$

If a system constituted by N elements is such that it can be divided into two or more essentially independent subsystems (e.g., independent dices, or spins interacting through short-range coupling), we generically have $W \sim \mu^N$ $(\mu > 1)$. Consequently, $S_q/k \sim \ln_q \mu^N$. There is a unique value of q, namely, $q=1$, for which we obtain the usual result $S_q \propto N$. But if the system is such that we have $W \sim N^\rho$ $(\rho > 0)$, then $S_q/k \sim \ln_q N^\rho$. Once again, there is a unique value of q, namely $q = 1 - 1/\rho$ for which, $S_q \propto N$ (see Refs. 5,6,14 of Ref. [1]). The same property holds in fact for $S_{\sigma(q)}$, $\sigma(q)$ being any smooth function of q such that $\sigma(1)=1$ (e.g., $\sigma = 1/q$, or $\sigma = 2-q$). For the correlated case, we have $S_{\sigma(q)} \propto N$ only for q satisfying $[1-\sigma(q)]\rho = 1$. The relevance of this property $(S \propto N)$ for thermodynamics needs, we believe, no further comment.

About the concept of "weak coupling" in Ref. [1]: Much of the criticism in Ref. [1] involves the concept of "weak coupling." To make this point clear through an illustration, let us think of the ground state of a Hamiltonian many-body classical system whose elements are localized on a d-dimensional lattice and have two-body interactions among them. Let us further assume that the (attractive) coupling constant is given by $C_{ij} = -c/r_{ij}^{\alpha}$ $(c>0,\ \alpha \geqslant 0,$ and $r_{ij} \geqslant 1)$. The potential energy $U(N)$ per particle generically satisfies $U(N)/N \propto -c\sum_{i\neq j} 1/r_{ij}^{\alpha} \simeq -c\int_1^{N^{1/d}} dr\, r^{d-1} r^{-\alpha} \propto -cN^*$ [with $N^* \equiv (N^{1-\alpha/d}-1)/(1-\alpha/d)$]. Therefore, for $\alpha/d > 1$ (*short-range* interactions in the present context), we have that $\lim_{N\to\infty} U(N)/N$ is finite, and BG statistical mechanics certainly provides the appropriate answer for the stationary state (thermal equilibrium) of the system. In this case, all the usual prescriptions of thermodynamics are satisfied, as well known [2]. If the interactions are, however, *long ranged* (i.e., $0 \leqslant \alpha/d \leqslant 1$), then $\lim_{N\to\infty} U(N)/N$ diverges, and the case needs further discussion. It might well happen that, dynamically speaking, the $N\to\infty$ and $t\to\infty$ limits do not commute. If so, only the $\lim_{N\to\infty}\lim_{t\to\infty}$ ordering corresponds to the BG stationary state, whereas the opposite ordering, $\lim_{t\to\infty}\lim_{N\to\infty}$, might be a complex one, *different* from the BG state, and in some occasions possibly related to the one obtained within the q formalism. It is clear then that, if we have long-range interactions and $N \gg 1$ (say of the order of the Avogadro number), it might very well happen that the

BG equilibrium is physically inaccessible, and the only physically relevant stationary or quasistationary (metastable) state is a non-Gibbsian one. Such a situation is indeed found in Ref. [3], as discussed below.

We can now address the manner used in Ref. [1] to refer to "weak coupling." It applies essentially in the simple manner stated in Ref. [1] only for $\alpha/d>1$, being *conceptually much more subtle* for $0\leqslant\alpha/d\leqslant 1$. For example, if $0\leqslant\alpha/d<1$, $U(N)/N$ *diverges* as $N^{1-\alpha/d}$ $(N\to\infty)$ for *any* nonvanishing value of *c*, even for *c* corresponding to . . . $10^{-10}eV$! Consistently, the *generic* use, without further considerations [such as the $(N,t)\to(\infty,\infty)$ limits, and the range of α/d], of relations such as Eqs. (5) and (7) of Ref. [1] seems irreducibly unjustified; as they stand, they trivially yield to no other possibility than $q=1$. In fact, Fermi transparently addressed this point in 1936 [4].

About the determination of q for a given system: The entropic parameter q is referred to in Ref. [1] as an "undetermined parameter." Moreover, the author claims, having proved that "q must be a *universal* constant, just like the Boltzmann constant k . . . ." I have difficulty in unambiguously finding in the paper whether this kind of statement would only apply to Hamiltonian systems, or perhaps also to dissipative ones; to systems whose phase space is high dimensional, or perhaps also to the low-dimensional ones. By "undetermined," it remains not totally clear whether the expression is used in the sense that q is "undeterminable," or in the sense of "not yet determined." However, if we put all this together, one might suspect that what is claimed in Ref. [1] is that it *can* be determined from first principles, and that the author has determined it to necessarily be $q=1$.

To make this point transparent, we may illustrate the factual *nonuniversality* of q by addressing the logisticlike family of maps $x_{t+1}=1-a|x_t|^z$, whose usefulness in physics can hardly be contested (at least for $z=2$). As conjectured since 1997 [5], numerically exhibited in many occasions (e.g., in Refs. [6–9]), and analytically proved recently [10,11] on renormalization group grounds, *q does depend on z*, and is therefore *not* universal, in neat contrast with what is claimed in Ref. [1]. Its value for $z=2$ (i.e., the standard logistic map), as given by the sensitivity to the initial conditions, is $q=0.244487\ldots$ at the edge of chaos (e.g., $a=1.401155\ldots$), whereas it is $q=1$ for all values of a for which the Lyapunov exponent is positive (e.g., for $a=2$). We have illustrated the nonuniversality of q for nonlinear dynamical systems with its value at the edge of chaos of the logistic map. It is perhaps worthy to notice that, since it has been proved to be analytically related to the Feigenbaum universal constant α_F [$1/(1-q)=\ln\alpha_F/\ln 2$], and since this constant is already known with not less than 1018 digits, we actually know this particular value of q with the same number of digits. Such a precision is self-explanatory with regard to the fact that q *can* be determined from first principles and that it *can* be different from unity (also see Refs. [12,13]).

Given the preceding illustrations of dissipative systems, and many others existing in the literature (e.g., Ref. [14]), it could hardly be a big surprise if, *also* for many-body Hamiltonian systems, q turned out to be a nonuniversal index essentially characterizing what we may consider as nonextensivity universality classes (in total analogy with the universality classes that emerge in the theory of critical phenomena). More precisely, one expects $q=1$ for short-range interactions ($\alpha/d>1$ in the example we used earlier), and q depending on (d,α) (perhaps only on α/d) for long-range interactions (i.e., $0\leqslant\alpha/d<1$), in the physically most important ordering $\lim_{t\to\infty}\lim_{N\to\infty}$. Although expected, uncontestable evidence has not yet been provided. It is not hard for the reader to imagine the analytic and computational difficulties that are involved. Nevertheless, the following points have already been established in the literature.

(i) The one-body marginal distribution of velocities during the well known longstanding quasi-stationary (metastable) state of the isolated classical inertial *XY* ferromagnetically coupled rotators localized on a *d*-dimensional lattice can be anomalous (i.e., non-Maxwellian). Indeed, it approaches, for a non-zero-measure class of initial conditions of the $\alpha=0$ ($\forall\ d$) model and not too high velocities, a q-exponential distribution (we recall that $e_q^x\equiv[1+(1-q)x]^{1/(1-q)}$, hence $e_1^x=e^x$) with $q>1$ [3]. *If* the energy distribution followed BG statistics, the one-body marginal distribution of velocities *ought* to be quasi-Maxwellian (*strictly* Maxwellian in the $N\to\infty$ limit since then the microcanonical-ensemble necessary cutoff in velocities diverges), but it is *not*. As specifically discussed in Ref. [3], the numerical results are incompatible with BG statistics. However, they do not yet prove that the one-body distribution of velocities precisely is, for the canonical ensemble, the one predicted by nonextensive statistics. Indeed, considering the appropriate limit $(N,M,N/M)\to(\infty,\infty,\infty)$ (N being the number of rotators of the isolated system, and M being that of a relatively small subsystem of it) is crucial. Work along this line is in progress.

(ii) In the same model, at high total energy, the largest Lyapunov exponent vanishes like $1/N^{\kappa}$ where κ depends on α/d [15,16]. Also during the longstanding state, the largest Lyapunov exponent vanishes, this time like $1/N^{\kappa/3}$ [17]. It is clear that, with a vanishing Lyapunov spectrum, the system will be seriously prevented from satisfying Boltzmann's "molecular chaos hypothesis," hence the (microcanonical) "equal probability" occupation of phase space.

(iii) In the longstanding regime of the $\alpha=0$ ($\forall d$) model, there is *aging* [18], something which is *totally incompatible with the usual notion of thermal equilibrium*. The correlation functions depend on the "waiting time," and are in all cases given by q-exponential functions. Even at high total energy, where the one-body distribution of velocities is Maxwellian, and where there is *no* aging, the time correlation functions are *still* given by q-exponentials with $q>1$, instead of exponentials, which is the standard expectation in BG statistics.

(iv) The temperature ($\propto$ mean kinetic energy per particle) relaxes, after the metastable state observed in the one-dimensional $0\leqslant\alpha<1$ model, onto the BG temperature through a q-exponential function with $q>1$ [19].

(v) In Lennard-Jones clusters of up to $N=14$ atoms, the distribution of the number of links per site has been numerically computed [20], where two local minima of the many-body potential energy are "linked" if and only if they are separated by no more than one saddle-point. This distribution is a q exponential with $q \simeq 2$, as can be checked through a direct fitting. The possible connection with our present discussion comes from the fact that the average diameter of the cluster is (in units of atomic size) of the order of $14^{1/3} \simeq 2.4$. Consequently, although the Lennard-Jones interaction is *not* a long-range one thermodynamically speaking (indeed, $\alpha/d = 6/3 = 2 > 1$), it can effectively be considered as such for small clusters, since all the atoms substantially interact with all the others.

(vi) The distribution of the number of links per node for the Albert-Barabasi growth model [21] yielding scale-free networks is analytically established to be, in the stationary state, a q exponential with $q = [2m(2-r)+1-p-r]/[m(3-2r)+1-p-r] \geqslant 1$, where (m,p,r) are microscopic parameters of the model. If we associate an $\alpha = 0$ interaction per link to this network, the just mentioned distribution also represents the distribution of energies per node. Although this is not the same distribution as that of the energy of microscopic states associated with a Hamiltonian, it is neither very far from it [22].

(vii) At this point let us mention some results, not many-body problem, that have been obtained with the $d=2$ standard map and with a $d=4$ set of two coupled standard maps. Both systems are *conservative* and *simplectic*, therefore having the dynamical setup of a standard Hamiltonian. The $d=4$ system undergoes Arnold diffusion as soon as the nonlinear coupling constant a is different from zero; this guarantees a chaotic sea which is singly connected in phase space (we may say that $a_c = 0$). The structure is more complex for the $d=2$ case because no such diffusion is present; consistently, unless a is sufficiently large, disconnected chaotic ‘‘lakes’’ are present in the phase space; below $a_c = 0.97\ldots$, closed KAM regions emerge in the problem. The remark that we wish to do here is that, in strong analogy with the many-body long-range Hamiltonian cases we have been discussing, both the $d=2$ and 4 maps present longstanding quasistationary states *before* crossing over to the stationary ones. The crossover time $t_{\mathrm{crossover}}$ diverges when a approaches a_c from above. This is very similar to what happens with the above (d,α) Hamiltonian, for which strong numerical evidence exists [3,17,23] suggesting that $t_{\mathrm{crossover}}$ diverges as $(N^{1-\alpha/d}-1)/(1-\alpha/d)$ when $N \to \infty$.

Although none of the (seven) factual arguments that we have just presented constitutes a proof, the set of them all does provide, in our understanding, a quite strong suggestion that the longstanding quasistationary states existing in long-range many-body Hamiltonians might be intimately connected to the nonextensive statistics, with q depending on basic model parameters such as d and α. The entropic index q would then characterize *universality classes of nonextensivity*, the most famous of them being naturally the $q=1$, extensive, universality class. Such a viewpoint is also consistent with the discussion of non-Gibbsian statistics presented in Ref. [24]. Last but by no means least, it is consistent with Einstein's 1910 criticism of the Boltzmann principle $S = k \ln W$ (lengthily commented upon in Ref. 6 of Ref. [1]).

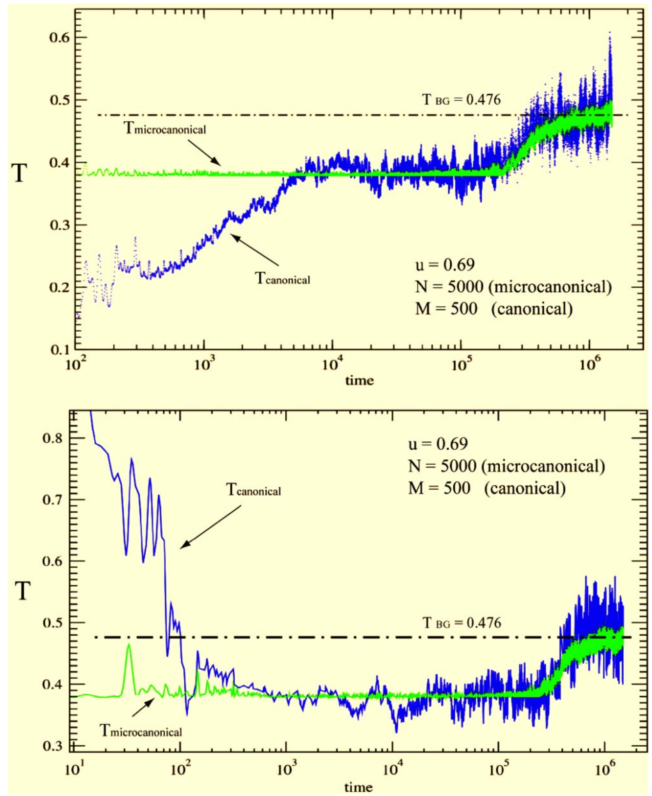

FIG. 1. Time evolution of the temperature $T_{\mathrm{microcanonical}} \equiv 2K(N)/N$ [$K(N) \equiv$ *total kinetic energy*] of one isolated system started with waterbag initial conditions at (conveniently scaled) energy per particle equal to 0.69 ($N=5000$ rotators; green line), and of the temperature $T_{\mathrm{canonical}} \equiv 2K(M)/M$ [$K(M) \equiv$ *subsystem total kinetic energy*] of a part of it ($M=500$ rotators; blue line). The M rotators were chosen such that their temperature $T_{\mathrm{canonical}}$ was initially below (a) or above (b) that of the whole system. It is particularly interesting the fact that, in case (b), the temperature of the subsystem of M rotators crosses the BG temperature $T_{BG} = 0.476$ without *any* particular detection of it.

About thermal contact between systems with different values of q and the zeroth principle of thermodynamics: We now focus on a strong and crucial statement in Ref. [1], namely ‘‘ . . . a Boltzmann-Gibbs thermometer would not be able to measure the temperature of a q-entropic system, and the laws of thermodynamics would therefore fail to have general validity.’’ [1]. Here we shall present the results [30] of molecular-dynamical simulations (using *only* $F = ma$ as microscopic dynamics) which will essentially exhibit what is claimed in Ref. [1] to be impossible. We shall illustrate this with the isolated $\alpha = 0$ model of planar rotators, and proceed through two steps.

We first show (Fig. 1) how the ‘‘temperature’’ (defined as twice the instantaneous kinetic energy per particle) of a relatively small part of a large system relaxes onto the ‘‘temperature’’ of the large system *while this is in the quasistationary regime* (where the system has been definitely shown to be *non*-Boltzmannian, and where it might well be described by the q statistics). We verify that the rest of the system acts for a generic small part of itself as a ‘‘thermostat,’’ *in total analogy with what happens in BG thermal equilibrium*. This is quite remarkable if we think that the system is in a state *so*

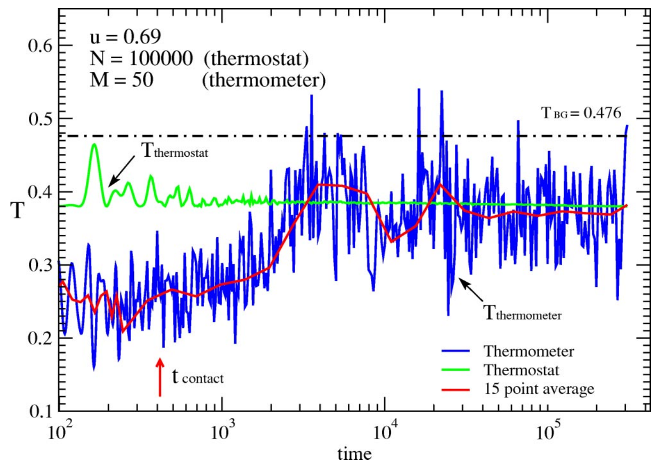

FIG. 2. Time evolution of the temperature $T_{\text{thermostat}} \equiv 2K(N)/N$ [$K(N) \equiv$ *thermostat total kinetic energy*] of one infinitely-range-coupled large system (thermostat) started with waterbag initial conditions ($N = 1000\,00$ rotators; green line) and of the temperature $T_{\text{thermometer}} \equiv 2K(M)/M$ [$K(M)$ $\equiv$ *thermometer total kinetic energy*] of one first-neighbor-coupled relatively small system (thermometer) started at Maxwellian equilibrium at a temperature below that of the thermostat ($M = 50$ rotators; blue and red lines). The large system is in a quasistationary state (where it is aging); its (conveniently scaled) energy per particle equals 0.69. The thermometer-thermostat contact is assured by only one bond per thermometer rotator, and starts at time t_{contact}. The intrathermostat and intrathermometer coupling constants equal unity; the thermostat-thermometer coupling constant equals 0.001. The thermalization of the thermometer occurs at the thermostat temperature, and up to time $t = 3 \times 10^5$, exhibits no detection of the BG equilibrium temperature $T_{BG} = 0.476$. The same phenomenon, with the thermometer initial temperature being *larger* than that of the thermostat, is not shown, because our numerical results suggest that the $N \gg M \gg 1$ limit has to be satisfied in an even more stringent manner due to the relatively large fluctuations of $T_{\text{thermometer}}$. For clarity, not all the points of the curves have been represented, but they have been instead logarithmically decimated.

different from thermal equilibrium that it even has aging!

We then show (Fig. 2) how a BG thermometer (its internal degrees of freedom are those of *first-neighbor-coupled* inertial rotators, hence definitively a $q = 1$ system) *does* measure the "temperature" of the *infinitely-range-coupled* inertial rotators during their quasistationary state, where the statistics is definitely *non*-Boltzmannian. In light of this evidence, it appears that the zeroth principle of thermodynamics is even more general than the already important role that BG statistical mechanics reserves for it. Naturally, the fluctuations that we observe in both figures are expected to disappear in the $(N, M, N/M) \to (\infty, \infty, \infty)$ limit.

The facts that we have mentioned up to this point heavily disqualify the essence of the critique presented in Ref. [1]. I believe, nevertheless, that it is instructive to further analyze it.

About the existing mathematical foundations of nonextensive statistical mechanics: It is essentially claimed in Ref. [1] that it can be proved, from the very foundations of statistical mechanics, that the only physically admissible method is that of BG. It is, however, intriguing how such a strong statement may be made without clearly pointing out the mathematical errors that should then exist in the available proofs of the q-exponential distribution. Such proofs have been provided by Abe and Rajagopal [25,26]; they are multiple, mutually consistent, and generalize the well known proofs done, for BG statistics, by Darwin and Fowler (in 1922), Khinchin (in 1949) and Balian and Balazs (in 1987), respectively, using the steepest-descent method, the laws of large numbers, and the counting for the microcanonical ensemble [25]. All these proofs are ignored in Ref. [1]. The critique therein developed outcomes severely diminished.

Similarly, no mention at all is made in Ref. [1] of the q generalizations of the Shannon (1948) theorem, and of the Khinchin (1953) theorem, which are universally considered as parts of the foundations of BG statistical mechanics since they prove under what conditions S_{BG} is unique. These two q generalizations [27] analogously exhibit the necessary and sufficient conditions associated with the uniqueness of S_q.

Finally, no mention at all is made of the fact that S_q $(\forall q > 0)$ shares with S_{BG} three remarkable mathematical properties that are quite hard to satisfy, *especially simultaneously*. These three properties are *concavity* (Ref. 1 of Ref. [1]), *stability* [28], and *finiteness of entropy production per unit time* (see Ref. [29], among others). The difficulty of having such agreeable mathematical features can be perceived by the fact that Renyi entropy (Eq. (19) of Ref. [1]), for instance, satisfies *none* of them for abitrary $q > 0$.

It is perhaps for not paying due attention to all these theorems that the cyclic argument involving Eqs. (22)–(26) of Ref. [1] has been included in the critique. Indeed, that argument uses Eq. (22) to "prove" Eq. (26). Such a consistency can hardly be considered as surprising since the distribution in Eq. (22) is currently established *precisely using the BG entropy*, i.e., the form of Eq. (26). By the way, immediately after Eq. (26) we read "provided $f(1) = f(0) = 0$, which corresponds to the requirement that the entropy vanishes at $T = 0$." It is in fact only $f(1) = 0$ which is related to the vanishing entropy at $T = 0$. The property $f(0) = 0$ has in general nothing to do with it; it is instead related to the *expansibility* of the entropy, i.e., the fact that $S(p_1, p_2, \ldots, p_W, 0) = S(p_1, p_2, \ldots, p_W)$.

About the escort distribution: The precise formulation of nonextensive statistical mechanics has, since 1988, evolved along time in what concerns the way of imposing the auxiliary constraints under which S_q is optimized (see Refs. 1–3 of [1]). The paradigmatic case occurs for the canonical ensemble, where one must decide how to generalize the traditional energy constraint. The correct manner is nowadays accepted to be that indicated in Ref. 3 of [1], i.e., Eq. (3) of Ref. [1], namely,

$$\frac{\sum_{i=1}^{W} p_i^q \epsilon_i}{\sum_{j=1}^{W} p_j^q} = U_q \tag{3}$$

This particular writing of the energy constraint has various interesting features. Let us mention three of them here (further convenient features can be found in Ref. 3 of Ref. [1]).

(i) It is precisely this form which emerges naturally within the steepest-descent proof [25] of the q statistics. It is a trivial consequence of the fact that $de_q^x/dx=(e_q^x)^q$.

(ii) This particular form causes the theory to be, in what concerns the energy distribution, valid up to a *single* value of q, namely, *precisely* that determined by the trivial constraint $\Sigma_{i=1}^{W}p_i=1$. Let us illustrate this in the continuum limit, for a typical example where the density of states $g(\epsilon)\propto\epsilon^{\gamma}$ for $\epsilon\to\infty$ ($\gamma\in\mathcal{R}$). Since we wish $p(\epsilon)$ to be normalizable, we must impose that $\int_{\text{constant}}^{\infty}d\epsilon g(\epsilon)p(\epsilon)$. Since $p(\epsilon)\propto\epsilon^{1/(1-q)}$ for $\epsilon\to\infty$, it must be $q<(2+\gamma)/(1+\gamma)$ ($q<2$ for the simple case of an asymptotically constant density of states, i.e., $\gamma=0$). *Precisely the same upper bound* for q is obtained by imposing the finiteness of constraint (3), as can be seen by analyzing $\int_{\text{constant}}^{\infty}d\epsilon g(\epsilon)\epsilon[p(\epsilon)]^q$. In other words, and interestingly enough, the use of escort distributions causes *both* constraints (norm and energy) to be mathematically well defined in the theory (i.e., given by *finite* numbers) *all the way up to a single upper bound for* q.

(iii) This structure (based on escort distributions) for the energy constraint allows the construction of a quite general entropic form [31] which is extremized by the Beck-Cohen superstatistics [32], and which, quite remarkably, is stable (like S_q, and in variance with Renyi entropy).

Conclusion: We have essentially argued here that the basis of the critique in Ref. [1] appears to be inconsistent with very many, and by now well established, physical and mathematical facts. We have addressed not all but only the main mispaths and inadvertences in Ref. [1]. Let us now summarize the main points of the present Comment.

(i) In what concerns a crucial difference between short- and long-range interactions, we stress that, if $\alpha/d>1$, $\lim_{N\to\infty}\lim_{c\to 0}cN^*=\lim_{c\to 0}\lim_{N\to\infty}cN^*=0$, whereas, if $0\leqslant\alpha/d\leqslant 1$, $\lim_{N\to\infty}\lim_{c\to 0}cN^*=0$ but $\lim_{c\to 0}\lim_{N\to\infty}cN^*\to\infty$. This is the basic point which is missed in Ref. [1]. It appears that this simple mathematical feature is deeply related to the fact that, for short-range interactions, we expect BG statistics *independent* of the $N\to\infty$ and $t\to\infty$ ordering, whereas, for long-range interactions, we still expect BG statistics *only* if t diverges first, but we expect something different, for example nonextensive statistics, if N diverges first. *For large systems, only the last possibility is physically achievable.*

(ii) A remarkable foundational work (uniqueness of the entropy S_q and its optimizing distribution, stability of S_q, and others) is available in the literature (e.g., Refs. [25,27,28]) which generalizes step by step the available foundational work available for BG statistical mechanics. It yields a power law for the stationary-state energy distribution, instead of the usual BG exponential law.

(iii) Figures 1 and 2 exhibit that, in striking contrast with what is stated in Ref. [1], the zeroth principle of thermodynamics *does* appear to emerge in a indisputably *non*-BG (metastable) state. This fact further supports the possible thermodynamical connection of nonextensive statistical mechanics, which is already known [33] to be consistent with the first, second, and third thermodynamical principles.

Our overall conclusion is that, although several important and/or interesting points related to nonextensive statistical mechanics still need further clarification, this theory undoubtedly nowadays exhibits a sensible number of physically and mathematically consistent results. Of course, as it has always been, only time will establish its degree of scientific utility in theoretical physics and elsewhere.

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