

states. Each of these has the smallest (or largest) energy of all the states with the same values of S , n and a , or, alternatively, the largest entropy of all the states with the same values of E , n and a . (Spin systems have energies that are bounded both from below and from above. For each set of values S , n and a , the state with the smallest energy corresponds to a positive temperature, and the state with the largest to a negative temperature.) For other states, E depends on more variables, and equation 1 is inappropriate.

Of course, one might consider states with more independent variables and define a T by either a similar derivative or some other procedure. However, such a definition would be useless for the thermodynamics of equilibrium. For a combination of two systems A and B to be in a thermodynamic equilibrium state, the entropy of that state must be the largest of all the states with the same values of $E^A + E^B$, $n^A + n^B$ and $a^A + a^B$. A necessary condition for this to be true is that

$$[(\partial E / \partial S)_{n,a}]^A = [(\partial E / \partial S)_{n,a}]^B$$

or

$$T^A = T^B \quad (2)$$

that is, that A and B be in temperature equality. Because only the thermodynamic temperature satisfies equation 2, any definition of T different from equation 1 would be irrelevant to the most important and unique operational meaning we assign to temperature measurements.

In probabilistic interpretations, the question arises of whether temperature is restricted to systems with particular values of either the amounts or the constraints, or both. Since the time of Josiah Willard Gibbs,¹ it has been recognized that no such restriction is necessary. Temperature is defined for systems with one degree of freedom and any values of the constraints. To see this, consider a system with a Hamiltonian operator $\hat{H}$, and the energy eigenvalue problem

$$\hat{H}u_i = \epsilon_i u_i, \quad i = 1, 2, \dots$$

where the spectrum ϵ_i is discrete, and each ϵ_i is a function of n and a . No restrictions are imposed on n and a because none are required by the eigenvalue problem; that is, the system may be one spin with two eigenstates, one harmonic oscillator or many particles in a box (large or small) with many eigenstates.

Regardless of the system, an equilibrium state has values of energy and entropy

librium state has values of energy and entropy

$$E = \sum_i x_i \epsilon_i$$

$$S = -k \sum_i x_i \ln x_i$$

where

$$x_i = \frac{\exp(-\epsilon_i/kT)}{\sum_i \exp(-\epsilon_i/kT)}$$

and is found² by maximizing S subject to fixed values of E , n and a . Here, the value of T is determined by E , and satisfies the relation

$$T = (\partial E / \partial S)_{\epsilon_i} = (\partial E / \partial S)_{n,a}$$

That is, T is the temperature, where the subscript ϵ_i denotes that all ϵ_i are kept fixed. [For this elegant and concise derivation of the canonical distribution, we need not invoke either a heat bath or interactions between subsystems of the overall system. All we need is to admit the existence of quantum theoretic probability distributions associated with a homogeneous ensemble, even though its density operator may have the form traditionally associated with inhomogeneous quantal ensembles (mixtures).³ Because no restrictions are imposed on n and a , temperature is defined for thermodynamic equilibrium states of systems with any number of degrees of freedom, including 1, any number of energy eigenvalues greater than or equal to 2, and any values of constraints.

Concerns about the applicability of the concept of temperature are sometimes expressed in terms of fluctuations. (This is how Feshbach expressed his concerns.) Some of these concerns, however, are misplaced. One meaning of the term "fluctuation" relates to the variance of a probability distribution associated with measurement results of an observable, such as the variances entering the Heisenberg uncertainty principle. Regardless of the magnitude of this fluctuation, the (expectation) value of the corresponding observable is well defined, and no restriction is imposed on its validity. So the fluctuation of temperature discussed by Feshbach, on the basis of which he concludes that "for small systems thermodynamics is not always quantitatively valid," cannot be variance-like. Besides, temperature is not a quantum observable.

The other meaning relates to changes over time. Such fluctuations imply that a system is not in equilibrium. But then the concept of tempera-

ture does not apply, not because the number of degrees of freedom is small, but because the state is not equilibrium.

I am thankful to Feshbach, Gian Paolo Beretta and Marvin Miller for helpful discussions.

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FESHBACH REPLIES: It is always possible to devise a precise definition of temperature. Such precision does not preclude the existence of fluctuations. In my Reference Frame column I called attention to the dependence of these fluctuations for isolated systems on the number of particles the system contains.

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Cluster Credit: Who Seconded the Motion?

I enjoyed reading Per Andersen's news story (October 1987, page 17) on recent work on nonuniformities in the Hubble flow. However, the first study to indicate large-scale motion was not that of Vera Rubin, Norbert Thonnard, W. Kent Ford Jr and Morton Roberts¹; in 1973, I found² that the Virgo cluster was receding faster than its expected Hubble velocity by several hundred kilometers per second. I used two independent methods of gauging relative cluster distances, the radius-magnitude relation and the m^* method of George Abell.³ Five years later, with improved data for the m^* method, and the first application of the microwave background correction⁴ to cluster redshifts, I refined⁵ the Virgo peculiar velocity to 658 ± 96 km/sec. There was also preliminary evidence for a peculiar velocity for cluster Abell 1367 of 683 km/sec. Rubin and her colleagues interpreted their results as showing motion of our Galaxy, not motion of

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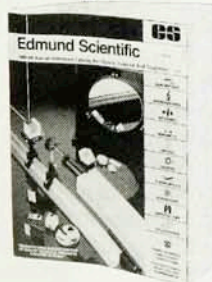
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external galaxies. It was only after an accurate determination of the Galaxy's true motion from the microwave background radiation that their results were interpreted (by a few) as showing a large-scale motion. The Rubin results and mine were disbelieved by most astronomers for over a decade. It is thus nice to see the recent flurry of interest in peculiar velocities and the birth of new buzzwords.

Work on cluster luminosity functions carried out at McGraw-Hill Observatory with an imaging charge-coupled diode system built by Dennis Hegyi and me⁶ indicates⁷ that m^* can yield relative cluster distances to $\pm 3\%$. Thus clusters at redshifts greater than that of A1367 can be tested. The data from my 1978 paper yield peculiar velocities of -8 km/sec for Coma and -504 km/sec for cluster A2199. I have plotted the longitudinal positions and peculiar motions of the four clusters projected on the best-fitting plane through their positions on the sky, adding to each cluster a tangential velocity equal to the Galaxy's projected motion relative to the microwave background in order to reference the clusters' velocities to the microwave background. The vector sum of these motions points toward Galactic longitude 258° and Galactic latitude 17° , not far from the constellation Hydra. In 1978, I obtained at Cerro Tololo data on the Hydra I cluster, from which I had hoped to obtain a new m^* and a new peculiar velocity. Unfortunately, these data have still not been analyzed; I hope they will receive more attention in the coming months.

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RUBIN REPLIES: It was with pleasure that I read Donald Gudehus's letter concerning his 1973 work on the Virgo cluster. As we are all aware, assigning credit in science can be a complex, subjective and often distasteful procedure. Discoveries are

rarely independent; what matters is that each study should advance the field. Even Gudehus would agree that there were earlier studies of large-scale motions, some referenced in his own work.

Strikingly early is the brilliant insight of William Herschel, based on a knowledge of the motions of a dozen stars. He wrote in 1783 in the *Philosophical Transactions of the Royal Society* that "there is not, in strictness of speaking, one fixed star in the heavens... when once it is known that some of them are in motion: for the change that must arise by such motion, in the value of a power which acts inversely as the squares of the distances, must be felt in all the neighbouring stars; and if these be influenced by the motion of the former, they will again affect those that are next to them, and so on till all are in motion."

Even earlier, perhaps 300 BC, Aristotle wrote in his *Metaphysics* that "the search for truth is in one way hard and in another easy. For it is evident that no one can master it fully nor miss it wholly. But each adds a little to our knowledge of nature, and from all the facts assembled there arises a certain grandeur." It is this certain grandeur, not the credit for being first, that we seek.

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Shevchenko on Soviet Science

In a public lecture on "The Future of American-Soviet Relations," given at Los Alamos on 15 September 1987, Arkady Shevchenko made several informative points. Three are of particular interest to physicists:

▷ Speaking of Soviet scientists traveling abroad, Shevchenko stated, "One thing I do know is that any scientist allowed to leave the Soviet Union will be instructed to collect specific information" from his scientific hosts. Shevchenko said that scientists had one chance to come back from a trip abroad without the information requested. However, if a second trip abroad also turned up empty, the scientist would never be permitted to leave the Soviet Union again. "This is not an assumption or a guess. I know."

▷ In respect to the Soviet version of the Strategic Defense Initiative, Shevchenko noted: "I can assure you that the Soviets have been working on lasers and other defensive systems long before the United States. The

estimates are they've spent \$150 billion on strategic defense."

▷ Shevchenko said that any nuclear arms control agreement made now between the United States and the Soviet Union would just be a starting point. He stated that it would be "absolutely impossible" to expect total nuclear disarmament. "Relations will never be that stable. The United States and the Soviet Union don't trust or understand each other and are not the world's only nuclear powers."

It is important to note briefly Shevchenko's wide knowledge and experience in these matters. He graduated in 1954 from the Moscow State Institute of International Relations, where he later earned a doctorate in international law. Shevchenko is a 22-year veteran of the Soviet foreign service. He rose to become actively involved in a number of sensitive issues, including disarmament and Soviet-American relations. He worked for Andrei Gromyko, Nikita Khrushchev and Leonid Brezhnev, and also knew Yuri Andropov, Konstantin Chernenko and Mikhail Gorbachev. He was a UN under secretary general, stationed in New York. He is the author of many Soviet publications on international affairs. He broke with the Soviets in 1978 and became a US citizen in 1986.

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An Educational Knotion from Knudsen

One of the delights in examining the letters section in certain physics journals used to be the possibility of encountering a letter written by Julius Sumner Miller. In a letter in the issue of *PHYSICS TODAY* devoted to the Michelson-Morley centennial (May 1987, page 132), Miller referred to "the essential ingredient in the education of a physicist... learning how to think physics." An Opinion column by Donald F. Holcomb, Robert Resnick and John S. Rigden that also appeared in the issue (page 87) focused on the longevity of the concepts and ideas of physics as a testimony not only to their basic nature but also to the fundamental nature of physics itself. Those authors also observed that "revolutions in physics, such as those that occurred during the 20th century, seldom invalidate older ideas" but tend to "establish boundary regions for older theories and open up new areas of physical investigation. Old physics does not die... It is simply integrated into a more