letters

nik under the chairmanship of James R. Killian Jr, and second as the first Director of Defense Research and Engineering, a new position created in 1958 as another part of the response to Sputnik. In these positions, I was directly concerned with precisely those scientific and technological programs in which the President himself was most involved and my own view of the world gradually changed as I came to see and understand the overall situation in which we found ourselves. I had gone to Washington a technological optimist, full of confidence in the technological fix. I came away three and a half years later gravely concerned about the all too common practice of seeking and using technological palliatives to cover over serious persistent underlying political and social problems. In particular, I became convinced of the futility of always devoting our main efforts to finding a technical solution to the problem posed by the steady decrease in our national security that was being brought about by the spread of high technology weapons throughout the world. This, it seemed to me, was not only futile but basically absurd, because nearly all of the weapons which in the hands of others were (and are) threatening our national security, and indeed our very existence, had been invented or perfected by us in the first place. In sum, my views on the relationship between technology and security did not arise out of Eisenhower's warnings; rather his warnings and my views both arose out of the same set of circumstances, but his formal warnings did very much help to crystallize my views on the subject. I found it very reassuring that the Commander-in-Chief, a professional military man himself, shared my own growing doubts about the value and efficacy of placing such a relatively high priority on finding technical solutions to what were really political problems.

Eisenhower's warnings, which were based largely on his remarkable intuition, pointed up very real and extremely serious problems. If we forget or downgrade his warnings, it will be to our peril.

HERBERT F. YORK
University of California, San Diego
La Jolla, California

* * *

Abridged version of the author's response on receipt of the Forum on Physics and Society Award on 27 April 1976. Further discussion of this subject may be found in York's recently published book "Race to Oblivion" reviewed in December (page 49).

Ether drift tested

This letter is in response to the letters of H. C. Dudley (February 1975, page 73) and Dale C. Scheetz (March 1976, page 15). Both letters address themselves to the question of the detectability versus the non-detectability of "ether-drift." The first letter suggests the possibility of using lasers or masers to provide useful data regarding "ether-drift." I should like to point to the paper by T. S. Jaseja, A. Javan, J. Murray and C. H. Townes[1] as a possible candidate. The fact that neither of the above-mentioned correspondents mentioned the work of Jaseja *et al* might be viewed as an oversight. This experiment used "one-way" light paths of two cross-fired infrared masers and drew the conclusion that there was no effect greater than 1/1000 of the v^2/c^2 term, over a period of six consecutive hours.

Once again you are vindicated, Albert!

Reference

1. T. S. Jaseja, A. Javan, J. Murray and C. H. Townes, Test of Special Relativity or of the Isotropy of Space by Use of Infrared Masers, Physical Review, 133, A1221 (1964).

J. W. HASLETT
University of Illinois at Chicago Circle
Chicago

3/25/76

Thermodynamic paradoxes

The article by Frank Weinhold on "Thermodynamics and Geometry" (March, page 23) gives an interesting new representation of an old branch of physics. However, I wish to point out that the formulation in terms of Riemannian geometry with a positive-definite metric is somewhat more restrictive than the second law of thermodynamics. The second law states that the entropy of an isolated system is maximized at equilibrium.[1] Since the energy of an isolated system cannot vary, the second law by itself says nothing about how the energy U varies with entropy, or with any other extrinsic variable X_i. Therefore, the second law does not require

$$|\mathcal{R}_i|^2 \equiv \left(\frac{\partial R_i}{\partial X_i}\right)_{X_1 \ldots X_{i-1} X_{i+1} \ldots X_{\ell+2}} \equiv \left(\frac{\partial^2 U}{\partial X_i^2}\right)_{X_1 \ldots X_{i-1} X_{i+1} \ldots X_{\ell+2}} \geq 0 \quad (1)$$

as stated in the article (page 26).

In general, the stability criterion (equation 1) is necessary only if one assumes a strong version of the zeroth law of thermodynamics, namely that two isolated systems each in stable equilibrium at the same temperature (or another intensive variable R_i) will be in stable equilibrium if placed in thermal contact (or contact for exchange of another extensive variable X_i). However, if equation 1 is violated, the two systems may be unstable to the transfer of entropy (or another X_i) when in contact, without there being an instability for either system when in isolation with the extensive

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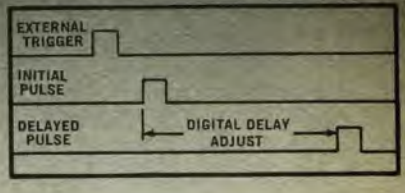

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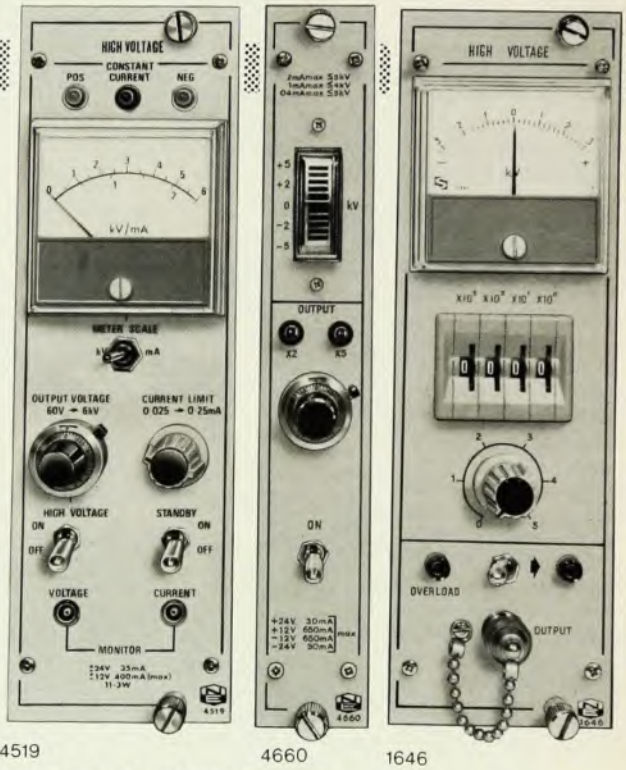

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variables fixed. This "contact instability" for systems stable in isolation cannot occur for single-component simple systems,[2] such as the homogenous fluids considered by Weinhold,[3] but the restrictions on microscopic interactions that are required to prevent contact instabilities and thus preserve equation 1 are not trivial.[4] In fact, long-range forces that produce clumping in the equilibrium state violate those restrictions and can thereby cause a breakdown in the contact-stability condition equation 1.

Gravitational forces are an important example. V. A. Antonov[5] has found in Newtonian gravity that a system of point particles of fixed total energy inside a box of fixed volume does not have a global entropy-maximum, since clumping of some of the particles can release an arbitrarily large amount of gravitational potential energy to increase the entropy of the other particles. However, there is a local entropy-maximum when the box is smaller (for a given number of particles and energy) than a certain critical radius (which is of the order of the Jeans length of the system). D. Lynden-Bell and R. Wood[6] show that for a certain range of radii below the critical value, there are configurations which are locally stable when isolated but which have negative specific heats (so that $\partial T/\partial S = \partial^2 U/\partial S^2 < 0$, violating equation 1) and thus are unstable when placed in contact with a heat bath.

When the box is larger than the critical size, there is no local entropy-maximum and no Newtonian equilibrium state. The system will evolve toward higher and higher entropy as one or more condensations form and grow. In Newtonian theory there is no limit to the entropy, but general relativistic effects will cause the condensations to form black holes, which have finite entropy.[7] The system ultimately attains a state of maximum entropy, which for box volume V, not too large (see below), consists of one nonrotating black hole of mass M surrounded by thermally distributed particles of total energy $U - Mc^2$ (where U is the total energy in the box) and chemical potentials zero.

In this equilibrium state, as Stephen Hawking has shown,[7] particles are continually falling into the hole, and the hole is continually creating and expelling new particles to replace them. The entropy S of the system is approximately that of the hole plus the surrounding radiation:

$$S = \frac{4\pi kG}{\hbar c} M^2 + \left(\frac{64\pi^2 k^4}{81\hbar^3 c^3}\right)^{1/4} V^{1/4}(U - Mc^2)^{3/4} \quad (2)$$

assuming that the black hole is much smaller than the box and that the external particles have a temperature low enough that they consist only of the zero-rest-mass particles: gravitons, photons, and muon and electron neutrinos and antineutrinos. Varying M gives a local extremum for S when the temperature of the hole equals that of the radiation

$$\frac{\hbar c^3}{8\pi kGM} = \left[\frac{4\hbar^3 c^2}{\pi^2 k^4}\left(\frac{U - Mc^2}{V}\right)\right]^{1/4} = T \quad (3)$$

There is no local extremum with a black hole present unless

$$V \le V_h = \frac{2^{22}\pi^2 G^4}{5^5 \hbar c^{17}} U^5 = \left(\frac{U}{1.39 \times 10^{35}\ \text{erg}}\right)^5 \text{cm}^3 \quad (4)$$

so (as Hawking has shown) if $V > V_h$, any black hole present will completely decay away into thermal radiation. For $V < V_h$, there are two roots of equation 3 for M given U and V; the smaller one gives a local minimum in S and so represents unstable equilibrium, and the larger one (for which $Mc^2 > 0.8U$) gives a local maximum in S and thus stable equilibrium by the second law of thermodynamics. (In order for the stable equilibrium configuration to be a global entropy-maximum, one must have $V < V_g = 0.256\ V_h$ so that the configuration with $M = 0$ does not have more entropy. In terms of the average energy density $\rho c^2 \equiv U/V$, the critical volume V_g is

$$V_g = \left(\frac{\rho}{1\ \text{g cm}^{-3}}\right)^{-5/4} (9.14\ \text{km})^3$$

Thus, a completely impervious box filled with $(9.14\ \text{km})^3$ or more of water would have its greatest entropy possible once a black hole of 7.47×10^{17} g or more formed inside it in thermal equilibrium with radiation of temperature 1.64×10^8 K = 14.2 keV or less.)

Once the larger root of M in equation 3 is chosen, equation 2 defines an implicit relationship between U and the extensive variables $X_1 \equiv S$ and $X_2 \equiv V$. The conjugate intensive variables are then

$$R_1 \equiv \left(\frac{\partial U}{\partial S}\right)_V = T$$

(which has the value of the expressions in equation 3), and

$$R_2 \equiv \left(\frac{\partial U}{\partial V}\right)_S = -P = -\frac{U - Mc^2}{3V} = -\frac{\pi^2 k^4}{12\hbar^3 c^3} T^4$$

One can then show that this system has

$$|\mathcal{R}_1|^2 \equiv \left(\frac{\partial R_1}{\partial X_1}\right)_{X_2} \equiv \left(\frac{\partial^2 U}{\partial S^2}\right)_V = -\frac{T^2}{5Mc^2 - 4U} < 0$$

$$|\mathcal{R}_2|^2 \equiv \left(\frac{\partial R_2}{\partial X_2}\right)_{X_1} \equiv \left(\frac{\partial^2 U}{\partial V^2}\right)_S = -\frac{16P^2}{5Mc^2 - 4U} < 0$$

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The responses are *negative*, contrary to equation 1 because more than 80% of the energy is in the black hole, which has a temperature that decreases with mass. As more heat is added to the cavity at constant volume, the black hole absorbs more radiation to become bigger and make the system cooler. Compressing the cavity adiabatically likewise puts more energy into the black hole, reducing the overall temperature and pressure. Thus the cavity would be unstable if placed in contact with a heat bath or with another cavity containing a black hole, even though such systems obey the second law of thermodynamics and are in equilibrium when isolated from one another.

In summary, one might say that just as special relativity with its light cones shows that spacetime does not have a positive-definite metric, so general relativity with its black holes and its gravitational fields (even in the Newtonian limit) shows that thermodynamics does not have a positive-definite metric either.

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DON N. PAGE
California Institute of Technology
4/16/76 *Pasadena*

REPLY BY THE AUTHOR: Several misstatements in Page's letter (such as that of the fourth sentence) indicate a misunderstanding of the technical sense of the term "isolated system" (1) or of the constrained minimization/maximization procedure that underlies the second law, or both. His deductions concerning black holes (which I do not presume to assess) do not relate specifically to the geometric representation of equilibrium thermodynamics which I discussed, for the paradoxical properties he describes—negative specific heats, negative compressibilities, and so on—are equally at odds with stability conditions of the *classical* formalism of macroscopic equilibrium thermodynamics. The "clumping" phenomena he discusses are of course incompatible with basic homogeneity assumptions that underlie ordinary thermodynamic theory, and which I believe were made adequately clear both in my PHYSICS TODAY article and the earlier technical papers.

FRANK WEINHOLD
Stanford University
5/10/76 *Stanford, California*

•

Frank Weinhold's article (March, page 23) on the geometrization of thermodynamics is interesting especially for the additional insights on the approach due to J. W. Gibbs. It would be helpful for the reader interested in pursuing the subject in more detail if the author would explain the relation of his work to the axiomatic formulation of general thermodynamics recently achieved by Walter Noll and Bernard David Coleman as recently explained by C. Truesdell.[1]

A nonspecialist reader might not know of the basic controversies in the field of the axioms of thermodynamics, because the referenced book by Truesdell was not reviewed by PHYSICS TODAY. The review of a second book by the same author[2] gave it credit for its clarity of expression, but appeared to be neutral regarding its pungent comments on the then current status of the axioms of thermodynamics.

References

1. C. Truesdell, *Rational Thermodynamics*, New York, 1969.
2. C. Truesdell, *Essays in the History of Mechanics*, New York, 1968, reviewed July 1970, page 71.

RICHARD L. MOORE
5/14/76 *Davenport, Iowa*

THE AUTHOR COMMENTS: In reply to Richard Moore, I can only state that my intent was neither to "axiomatize" thermodynamics, nor to encompass irreversible phenomena. Rather, I have sought to show how an underlying geometric structure of the classical formalism (already implicit in the empirical laws, and thus preserving the empirical spirit of the subject) brings useful techniques to bear on the analysis of thermal equilibrium.

FRANK WEINHOLD
Stanford University
5/20/76 *California*

•

In a recent interesting paper, Weinhold[1] has discussed the geometrization of equilibrium thermodynamics. One central feature of his approach is the observation that the generalized "triangle inequality" ($d_{ij} \leq d_{ik} + d_{jk}$ for all sets of points i, j, k) corresponds to the second law of thermodynamics whenever the "distances" d_{ij} are identified with the differences between "thermodynamic response vectors" $\mathcal{R}_i$ and $\mathcal{R}_j$ (i.e. $d_{ij} = |\mathcal{R}_i - \mathcal{R}_j|$), it being further necessary to de-

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fine $\mathcal{R}_i$ by the following expression:

$$|\mathcal{R}_i|^2 \equiv (\partial R_i/\partial X_i)x_j \ldots x_{c+2}$$

where (R_i,X_i) is a conjugate pair of thermodynamics variables such as $(-P,V)$ or (T,S). Weinhold's papers[1,2,3] make it amply evident that this is an important alternative way of expressing the content of classical thermodynamics, providing very illuminating ways to view well known relationships and often providing generalizations that would be much more awkward in the conventional formalism. His approach is in the general spirit of recent important work by H. B. Callen[4] to show that the conceptual basis of thermodynamics is the restrictions on the behavior of the matter that follow from the symmetries of the fundamental laws of physics.

I would like to point out some related geometrical relations, which although not nearly so sweeping in scope or utility as those of Weinhold and of Callen are nevertheless rather interesting and readily accessible to anyone who has a grasp of elementary analytical geometry.

The first law of thermodynamics, in the form $\Delta U = Q + W'$, where U is the internal energy, Q the heat added, and W' the work done *on* the system, can be seen to conform to the definition of an ellipse as the locus of points such that the sum of the distances from the two foci is constant. A given internal energy corresponds to an infinite family of ellipses but a specific system and/or process would correspond to an ellipse with a given eccentricity and spacing of foci. If the usual convention of treating work done by a system as positive ($W = -W'$) is used, then the First Law can be represented as a hyperbola.

A different and somewhat more interesting approach, which also introduces some new angle variables, is to consider the First Law as

$$Q = \Delta U + W \text{ (general process)}$$

$$\int TdS = \Delta U + W_r \text{ (reversible process)}$$

and the second law as $W_r \geq W$, or $Q \leq \int TdS$. Now imagine a set of generalized coordinates such that velocities squared are proportional to energies, or such that velocities are the square roots of energies with the proper choice of the "mass." Thus $(\Delta U)^{1/2}$ acquires the interpretation of a velocity or a momentum, as do $Q^{1/2}$ and $W^{1/2}$. With this interpretation, the first law is seen as a relation between the squares of vectors, and in fact, as the condition for a right triangle with sides $(\Delta U)^{1/2}$ and $W^{1/2}$, and hypotenuse $Q^{1/2}$. The figure shows the triangle for the first law and neatly reduces the second law to the statement that the triangle for a general process must lie within or on the one for a reversible process, or that the angle α_r between $(\Delta U)^{1/2}$ and $(\int \tau dS)^{1/2}$ for the reversible case must equal or exceed α for the general case. For a specified system and process, the irreversibility is characterized by the angle $\Delta\alpha = \alpha_r - \alpha$. The law of cosines applied to the upper triangle provides an interesting new relation

$$\frac{1}{2}(W_r + W) - (W_r W)^{1/2} = \frac{1}{2}\left(Q + \int \tau dS\right) - \left(Q \int \tau dS\right)^{1/2} \cos \Delta\alpha$$

which implies that the arithmetic mean of the absorbed heats exceeds their geometric mean by a larger amount than the arithmetic mean of output works exceeds their geometric mean. When either U, Q, or W is negative, the interpretations are not as simple.

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R. E. BARKER, JR
University of Virginia
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5/14/76

UFO journal

In connection with the recent correspondence in your columns about the UFO observations, your readers should be informed of the formation of The Committee for the Scientific Investigation of Claims of Paranormal Phenomena, under the sponsorship of the American Humanist Association. This Committee will publish a biannual journal, *The Zetetic*, this Fall, at $10.00 per year. The Managing Editor may be addressed at 923 Kensington Ave., Buffalo, N.Y. 14215.

FRANK MORTYN
San Diego, California

6/16/76

Debunking astrology

I wish to comment briefly on your review of *Objections to Astrology* (March, page 54). Your reviewer, Robert H. March, asks: "To whom is this book addressed and what end does it serve?" As far as I am concerned, I made the effort of organizing the articles for the book for two primary purposes, which have been achieved. The first one was to warn young people against accepting astrological predictions without question. The second was to provide them with a clearly written statement showing that astrology totally lacks a scientific foundation.

From 1930 to 1974, I taught regularly at least one half of a beginning course in astronomy, first at Harvard (1930–1956), later at the University of Arizona (1966–1974). During the past few years, I again became quite disturbed by the many requests from my students regarding the astronomical foundations for astrology (or lack of the same) that came my way. I would answer my students' questions, but I could not refer them to any published, up-to-date, concisely written answers to their inquiries. With the recent increase of interest in astrology, and the increase in astrological publications, young folks simply lack a firm statement about the astronomical evidence against astrology and they naturally turn to their astronomy teachers for advice and information. My article was specifically aimed at enlightening the young people who want this sort of information. There are old people who need this information as well, but I do not care as much myself how those folks think. However, it is most important that young people should realize that they must learn to use fully their God-given rights. They must learn to think out their own problems and make decisions wisely themselves, instead of becoming dependent on astrological predictions, as many are today. They must be "tubs that stand on their own bottoms."

As I see it, we have no hope of reforming the dyed-in-the-wool believers in astrology. For some, at least, astrology is almost like a religion, and all I can say is that they hold their beliefs on poor basic grounds. There are others who take advantage of astrology simply for financial personal gain. It does seem to me that I, as an astronomer, trained and supported in my work by public funds, have a duty to inform the general public and warn them that there is no foundation for astrology. I shall continue to do this as best I can, hopefully paralleling my activities with those of many of my scientific colleagues.

Some of my correspondents ask what harm is done by having people believe in astrology—it is all fun and games! Some of the letters I receive show the way in which real harm is being done. The daily columns on astrology in the newspapers (1300 of them) are not just fun. To take an example: One morning our otherwise excellent local morning paper in Tucson omitted the daily column on astrology. There were dozens of telephoned complaints. One came from a woman who was most upset for, because of the omission of the daily column on astrology, she did not know how to organize her day. She was apparently quite desperate about it all. I note that in several letters from astrologers the daily columns on astrology are shown to be of no value. And yet, vast amounts of money are earned by astrologers, and lots of papers are sold because of this admittedly fraudulent practice of serving this daily brew to gullible customers.