

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.

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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. Townes¹ 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).

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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

$$\begin{aligned} |R_i|^2 &\equiv \left(\frac{\partial R_i}{\partial X_i} \right)_{X_1, \dots, X_{i-1}, X_{i+1}, \dots, X_{c+2}} \\ &\equiv \left(\frac{\partial^2 U}{\partial X_i^2} \right)_{X_1, \dots, X_{i-1}, X_{i+1}, \dots, X_{c+2}} \geq 0 \quad (1) \end{aligned}$$

as stated in the article (page 26).

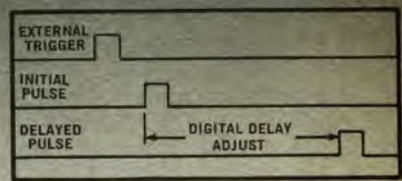
In general, the stability criterion (equation 1) is necessary only if one assumes a strong version of the zeroth law of thermodynamics, namely that two isolated systems each in stable equilibrium at the same temperature (or another intensive variable R_i) will be in stable equilibrium if placed in thermal 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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