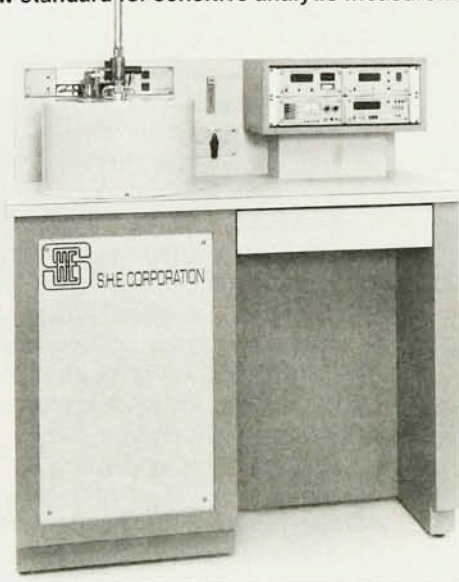


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letters

military waste production; that could only come from destruction of the weapons.

I am sorry that Ravi Rau finds me "laughable," "parochial," "racist," "astounding," "naive," "foolish," "arrogant" and "pontificating." It must be wonderful to be such a good judge of character. One misstatement does require correction. Rau equates non-proliferation with imperialism, and he states that "an essential component" of the treaty is a commitment on the part of the nuclear powers to stop production ("vertical proliferation," in his words). My copy of the treaty contains no such item to provide a convenient excuse for clandestine proliferation of nuclear weapons.

HAROLD W. LEWIS

University of California

Santa Barbara, California

2/83

Berkeley in the 1930s

Contrary to the statement by Robert Varney (October, page 24) that Gilbert Lewis terminated his experimental research career in 1937, may I point out that Lewis carried out experiments, starting in 1937, on general acids and bases (with G. T. Seaborg), the absorption and emission of light by aromatic substances (with J. Bigeleisen, M. Kasha, D. Lipkin and T. T. Magel), photochemistry in condensed media (with J. Bigeleisen, D. Lipkin and T. T. Magel), and the identification of triplet states in organic compounds (with M. Calvin and M. Kasha). Each of these research areas has flowered in the last forty years. A more detailed historical perspective of Lewis' contributions in these and other areas of chemistry, based on a symposium held at the March 1981 meeting of the American Chemical Society, will be published in the forthcoming issue of the *Journal of Chemical Education*.

JACOB BIGELEISEN

State University of New York

Stony Brook, New York

2/83

More Russian refuseniks

Jacov L. Al'pert was formerly deputy director of the Institute of Terrestrial Magnetism, Ionosphere, and Radiowave Propagation of the Academy of Sciences, USSR. As a pioneer of space physics, as a major contributor to the theory of satellite-plasma interaction and of magnetospheric wave propagation, and as the author of several books that have been translated into English and published in the US, he is certainly known by name to many members of AIP. But only few are likely to have

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met him, for he has not been permitted to attend any scientific meetings outside the USSR since 1959. Because of this and other harassment, he and his wife applied in 1975 for visas to emigrate to the West, whereupon he was summarily demoted to a part-time position and she was dismissed from her employment. Since that time, they have been living in Moscow in difficult circumstances and constantly renewing their visa applications in spite of repeated refusals. Meanwhile, he has been most active in helping unemployed scientists who are also waiting for their exit visas, notably by organizing a series of seminars through which they try to maintain their professional skills.

While living his refusenik life in Moscow, Al'pert has written a new book on space plasma physics, titled *The Near-Earth and Interplanetary Plasma*, which will be published, in two volumes, by the Cambridge University Press (England) in the spring of 1983. All royalties will accrue to a trust fund. If the Al'perts continue to have their exit visa applications rejected, the fund will be used to help them in every way possible and also, with their approval, to help other Russian refusenik scientists. Should they receive an exit visa, the fund would be put at their disposal.

People who have read any of Al'pert's previous books are familiar with the high standard of his writing, and this will be their prime incentive to purchase his new one. But in addition, as Al'pert's trustees, we urge AIP members to purchase it and to make sure that it is acquired by the libraries of the institutions to which they belong, as a practical way of helping an eminent colleague who has succeeded in continuing his professional work despite so many obstacles, and at the same time has done so much to help others.

FRANÇOISE STOREY
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2/83

Response to book review

I wish to thank you and David Owen for the review of my book "An Approach to Rheology Through Multivariable Thermodynamics" in November (page 77). However, I object to several statements in the review.

Owen indicts "the traditional approaches to thermodynamics... because of the high frequencies of vague statements, inconsistencies, in this and other books..." The existence of poor books in thermodynamics is no reason to condemn all the books. Examples of

good books are *Thermodynamics* by Lewis and Randall, revised by Pitzer and Brewer; *Thermodynamics* by Guggenheim; and *Chemical Thermodynamics* by Prigogine and Defay, translated by D. H. Everett. If Owen intends to place my book in a class with these books, I consider it a compliment.

Owen criticizes my book because it does not include the continuum mechanics and continuum thermodynamics approach. My book takes the opposite approach—that matter is made up of molecules and atoms. Then the rheological behavior and the thermodynamic free energies are shown to be related to the kinetic energy and the intermolecular potential energies of these atoms and molecules. In fact, the terms in the free-energy equations are shown to correspond to these energies. I have just written a short addendum in which I expand on the above ideas in my book. I hope that the explanations in the addendum are clearer. I also describe the sources of reversibility and irreversibility and connect these to the thermodynamic equations for free energy.

The reader can obtain a copy of the addendum by writing to me at the address below.

HARRY H. HULL
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2/83 Sun City Center, Florida 33570

Amateur scientists

In his excellent article on the great amateur scientist Alfred Lee Loomis (January, page 25), Luis Alvarez has emphasized that the twentieth-century scientists are usually professionals, whereas before that time, most scientists were independently wealthy gentlemen who could devote their lives to scientific research. However, there appear to be too many exceptions to this general rule. Among the historical greats, Newton, Lagrange, Laplace, Gauss, Faraday, and many others were not particularly wealthy. Lord Kelvin became wealthy as a physics professor from his consultation fees (the equivalent of at least a quarter of million dollars annually).

In more recent times, amateur scientists can be more productive than the professionals. The airplane was invented by two bicycle mechanics, not by Professor Samuel Langley. Xerography was invented by a New York City lawyer working in his Brooklyn apartment, not at any well-financed and well-organized research laboratory. (He had tried but failed to get any financial backing from the large industrial corporations. Finally he had success from an obscure small corporation in Rochester, New York.) Gregor Mendel was a monk. The important geolo-

gical concept of continental drift was introduced by Alfred Wegener, a meteorologist who received his doctorate in astronomy. Thomas Edison never went to college. Similarly, the founders of great high-technology companies (such as Polaroid or Apple Computers) are often college dropouts.

May I propose an alternate rule: "First generation founding fathers are often amateurs." In fact, professionalism can often be a hindrance. Concepts such as continental drift or viral origin of cancer were refused (and often despised) by the orthodox scientific community for fifty years as a consequence of professional inertia. Had Wegener been a professional geologist, his drift theory might have brought him the real risk of a ruined career and unemployment, and so he might not have dared to propose the drift theory. Peyton Rous discovered the viral origin of cancer when he was a young man, but did not receive the Nobel prize until he was 87. The scientist who discovered that DNA (rather than protein) was the gene lived "only" into his sixties and never received the Nobel prize.

It is obvious that few high-school dropouts can invent like Edison and that the university professor is usually more creative than the patent-office clerk. Nevertheless, history of physics may be quite different had Albert Einstein been working in a university rather than in the patent office. Bold hypothesis ("In order to have constant light velocity, it may be necessary to postulate the non-universality of time") is the key step in relativity. Despite (and perhaps hampered by) his superior knowledge of physics, Lorentz did not dare to take such a drastic step. Had Einstein not been isolated in the patent office (or some similar place), he might follow the footsteps of Lorentz. Today, we may have Einstein-Lorentz transformation rather than relativity!

As the field matures and progresses, the founding fathers are naturally followed by the academically oriented professionals, where their successes can be enhanced by large staffs, large budgets, good organizations, enlightened managements and so on. However, systematic enhancements at the preliminary stages may not be obvious. The almost disappearance of the true amateurs, as pointed out by Alvarez, is unfortunate and may have contributed to lackluster performance of US innovation in recent years.

TUNG TSANG
Howard University
Washington, DC

1/83

Development of radar

I am writing in connection with the excellent paper in January (page 25) by