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
L. R. OWEN STOREY
Marcilly-en-Villette, France
KENNETH G. BUDDEN
Cavendish Laboratory
Cambridge, England
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 critizes 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
1710 Dell Webb Boulevard
Sun City Center, Florida 83570
2/83

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 geological 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

Luis W. Alvarez in which he gave an account of the scientific accomplishments of the late Alfred Lee Loomis.

Near the bottom of page 31 the statement is made that "the dismal state of US radar was demonstrated at Pearl Harbor, a year and a half after the Tizard Mission had revealed all the British successes to the US armed forces."

This statement would imply that the US armed forces had done little if anything to solve the aircraft early-warning and tracking problem before the British information became available on their developments. In the 1930s and until near the end of WWII the US Army Signal Corps was responsible for the development of detection equipment to satisfy the needs of the Army and the Air Corps. In 1937, after abandoning the use of heat detection, which did not give range and could not look through clouds, a program was started to develop an early warning radar at the request of General H. H. Arnold of the US Army Air Corps.

At Ft. Monmouth and at Ft. Hancock the Signal Corps engineers developed the SCR-268 gun and search light laying radar, which operated at 205 MHz, and the SCR270-271 early-warning aircraft-detection radar, which operated at 110 MHz. The development model of the SCR-270 was demonstrated to the Army and Air Corps brass in 1939. The SCR-268 was manufactured by Western Electric with the help of Bell Laboratories engineers and the SCR-270 and 271 were manufactured by Westinghouse with some items from RCA. The first models of the SCR-270 were sent to guard the Panama Canal in June 1940.

The SCR-270 installed at Opana, Oahu (Hawaii) before the Japanese attack on Pearl Harbor on 7 December detected the raid at a range of 132 miles and tracked it into the ground clutter. The failure, therefore, was not of the design of the equipment but of the chain of command.

When I became technical executive officer and later director of Evans Signal Laboratory in Belmar, New Jersey, in 1944, all this history was fully brought home to me; I can assure you that the SCR-270-271 was a well designed and functioning piece of equipment before and during WWII.

The Signal Corps went on to develop 600 MHz radar using the Zahl tube which delivered 250 kW peak power. Portable equipment using this technique was manufactured by Zenith and saw much use in the field.

Radiation Lab developments using the British high-power microwave magnetron gave the US a great advantage over the Germans both in airborne and antiaircraft-gun laying equipment. The Signal Corps was responsible for procurement of a large number of equipments developed there including the GCA invented by Alvarez.

JOHN H. DEWITT JR
Consulting engineer
1/83 *Nashville, Tennessee*

•

The article by Luis Alvarez contains several rather dubious statements about radar development on which I would like to comment, and it also it fails to mention considerable pioneering work closely related to the Loomis activities.

The development of radar has a long and complicated history, and its invention, involving many people,[1] raises some questions about the Alvarez statement that radar was "invented independently in the United States by the Navy and the Army, and in England by Robert Watson-Watt."

As early as 1932, Irving Wolff and I, working at RCA, Camden, New Jersey, undertook a research program on 9-centimeter electromagnetic waves. The end-plate magnetron[2] was invented, and we used it to demonstrate to the US Army Signal Corps in August 1934 some transmission and reflection tests across New York Bay.[3] A ship entering the bay was detected, and other objects, such as a water tank and large trees were located.[4] The Army, in a Signal Corps history published in 1956, said that these tests "may well have been the first successful use in the United States of microwave radar, or of what eventually became microwave radar."

The New York Bay tests were made with continuous tone modulation. Subsequently, work was started on a pulsed radar system, and this was demonstrated to the Army and Navy in 1936.

By 1940, when Loomis and others undertook the establishment of the MIT Radiation Laboratory, the RCA work was well known in scientific and radio engineering communities. Numerous papers had been published,[2–7] and our 9-centimeter wave equipment had been demonstrated at several IRE meetings. The magnetron research had been discussed at MIT in seminars organized by Wayne Nottingham and W. L. Barrow. Lee A. Dubridge had visited us at Camden and knew of our progress. Our representative on the NDRC Microwave Committee, Loren F. Jones, had discussed our work with members of that Committee, including Loomis, who was chairman. Further discussions were held with Loomis at the latter's New York apartment. Furthermore, in 1940 a group headed by Karl T. Compton and including, to the best of my recollection, Loomis and Vannevar Bush, visited us to discuss our research and see our apparatus. As a result of this visit we supplied the new-born Radiation Laboratory with two 3000–4000 MHz signal generators, extra end-plate magnetrons, wavemeters, output power meters, attenuators, square-wave modulators, 9-cm filters, and so on—in short, basic equipment needed to initiate a microwave radar research and development program in the 3000–4000 MHz region. At that time, such equipment was not available elsewhere in the US.

Thus it is obvious that by 1940 microwave technology was well advanced at RCA, and it played a significant role in the establishment of the Radiation Laboratory; and furthermore, the two klystron radar sets, mentioned by Alvarez, were not the only microwave radar sets then existing in the US.

In continuing his discussion of the history of radar development, Alvarez states also that "The dismal state of US radar was demonstrated at Pearl Harbor." This statement is contrary to records with which I am familiar.[8] The Signal Corps radar at Pearl Harbor on 7 December 1941 was manned by Private Joseph L. Lockard. He detected planes approaching about a half hour distant from the Hawaiian Islands, and reported his sighting to his superior officer, who, knowing that a group of American planes was due from the mainland, believed the detected planes to be American. He took no action. Thus the Pearl Harbor disaster can not be blamed on a "dismal state" of US radar. US radar had done its duty, as had Private Lockard. He was awarded the Distinguished Service Medal, and was enrolled in the Signal Corps Officer Candidate School at Fort Monmouth, New Jersey, from which he later graduated as a lieutenant.

On the whole, I found the Alvarez article a fascinating contribution to the history of physics, and regret only that it was not more comprehensive.

References

1. T. G. Greene, RCA Engineer **23**, No. 5, 6 (1978).
2. E.G. Linder, Proc. I.R.E. **24**, No. 4, 633 (1936).
3. Irving Wolff and E. G. Linder, RCA Institutes, Inc. Radio at Ultra-High Frequencies **1**, 421 (1940).
4. I. Wolff, RCA Engineer **23**, No. 5, 11 (1978).
5. I. Wolff, E. G. Linder and R. A. Braden, Proc. I.R.E. **23**, No. 1, 11 (1935).
6. E. G. Linder, Proc. I.R.E. **26**, No. 3, 346 (1938).
7. E. G. Linder, Proc. I.R.E. **27**, No. 11, 732 (1939).
8. Samuel E. Morison, *History of United States Naval Operations in World War II*, Vol. 3, Little Brown, Boston, page 138 (1948).

ERNEST G. LINDER
2/83 *Boynton Beach, Florida*

THE AUTHOR COMMENTS: I agree completely with John DeWitt that the US Army Signal Corps Radar sets number 268 and 270 were of high quality, and that they were developed independently of any inputs from the British. My rationale for the apparent disagreement with my reference to the "dismal state of US radar (as) demonstrated at Pearl Harbor. . . " is one of semantics; as technical people, DeWitt and I are accustomed to thinking of radar in terms of a "device," such as the SCR-270, that did in fact detect the incoming Japanese aircraft at 132 miles, as DeWitt says. But the main message of the Tizard Mission, in the summer of 1940, was that a radar device was of no operational use unless it was integrated as a radar *system* into the command and control network. I agree with DeWitt that the failure of US radar was "not in the design of the equipment but in the chain of command." But I stand by my statement that Pearl Harbor showed the dismal state of US radar, 18 months after the Tizard Mission told us that the key to the usefulness of radar was in command and control. Doctors have an expression that illustrates this situation: "The operation was a success but the patient died."

In response to Ernest Linder, I must agree that I wasn't correct in saying that the two klystron radar sets I mentioned in my article—those of Loomis and Hansen—were the only microwave sets then existing in the US. But one should remember that the subject of my article was not the history of radar, but the life of Alfred Loomis. In recounting his appointment, by Bush, to head all NDRC supported work in microwave radar, it seemed appropriate to mention one of his qualifications to hold that important job—the fact that his laboratory had built one of the few then-existing microwave radar sets. If I had been describing the history of US radar, I would not have mentioned the Loomis or Hansen devices, or that of RCA, because in my opinion, none of the three had any appreciable effect on the development of microwave radar, as it was practiced by the Allies in WWII. The program derived directly from the invention by John Randall and Henry Boot of the cavity magnetron, which immediately raised magnetron peak powers from Linder's 13 watts[1,2] to 50 000 watts, and within two years to 10^6 watts. To physicists, accustomed to sudden changes by factors of 10^5, a simple way to appreciate the magnitude of Randall and Boots' engineering "quantum jump" of a factor of 3000 in power, is to compare it to the way aircraft engine power has increased from the 16 hp of the Wright brothers' first plane, to the roughly 25 000 equivalent hp of a modern jet engine. That development—of less than a factor of 3000 in power—took 75 years. Milestones along the way included the 1918 Liberty engine, at 420 hp, the versatile Rolls-Royce Merlin engine of 1938, at 1100 hp, raised to 2700 hp by 1945, the Double Wasp, used by US bombers in 1945, at 3400 hp, and the Pratt and Whitney T57 turboprop of 1960, at 15 000 hp. These numbers should help those who weren't involved in the US radar effort of WWII to understand the awe in which those of us who were still hold the achievements of Randall and Boot.

And as I admit my mistake in omitting mention of the RCA radar set, I must point out that Linder's first reference[3] is similarly incorrect when it says, "At the time of our entry into World War II, the twenty (RCA CXAM) sets installed on the most important ships of the fleet were the only radars in use by the Navy." (The CXAM was definitely not a microwave radar set.) The official history of the MIT Radiation Laboratory[4] has this to say in its chronological account of what happened in June 1941: "[A Rad Lab-built s-band radar had been] installed on the USS Semmes, and signals were seen on a PPI, display. Production contract for the SG radar set was awarded to Raytheon—the first US order for microwave radar. Becomes the Navy's most widely-used microwave set." So Linder, MIT, RCA and I all show that we, or our writers, are human enough to remember best the things with which we were associated.

References

1. R. I. Sarbacher and N. A. Edson, *Hyper and Ultrahigh Frequency Engineering*, Wiley, New York, p. 575 (1943).
2. E. G. Linder, Proc. IRE, **27**, 732 (1939).
3. T. G. Greene, RCA Engineer **23**, No. 5, 6 (1978).
4. *Five Years* (The MIT Rad Lab Memory Book), Andover Press, Andover, Mass. (1946).

Luis W. Alvarez
University of California
Berkeley, California

4/83

Using nuclear waste

One of your writers on high-level waste disposal (December, page 36) remarks that waste from one year's power-plant operation would fit under the kitchen table; it might just be a little hot. The other writer remarks that it was once thought that nuclear waste might be useful, but the experts soon changed their tune.

In truth, waste is waste only if it isn't useful. In the days of cheap energy, one couldn't afford much capital invest-