I was much intrigued by the query in the "box" on page 56 of the April issue as I have been searching for the origin of the phrase myself. It is almost certain that Michelson did write it, as the phraseology of the last sentence (about "the sixth place of decimals") is identical with what he used in a speech four years earlier at the dedication of the Ryerson Laboratory.

What I would like to know is the identity of the "eminent physicist" whom Michelson was quoting.

George L. Trigg
Physical Review Letters

High ideals and hard data

In the PHYSICS TODAY interview with Jay Orear (May, page 87) a statement quoted from the council of the Federation of American Scientists begins, "Except in times of national emergency, the university should not . . ." This is the crux of the council's entire position, as I see it; for just *when* is a national emergency?

Are university researchers to wait for a formal declaration from Congress or the President? (Will they agree that an emergency really exists if it is declared?) Are they unscientifically not to anticipate or project possibilities of emergencies arising and wait for the fortunes of fate to accost them? Given the long lead times required to develop sophisticated defensive weapons of all sorts, can they in good conscience twiddle their thumbs until the emergency confronts them?

I do not know what university scientists may have in the way of magic that permits them to respond more efficiently to emergencies than other run-of-the-mill colleagues, but one thing the council might do in clarifying its position is to suggest some quantitative boundaries for its notion of "national emergency." How clear and present must the emergency be before conscience will permit council members to act? Something requiring response in five days? In five weeks? In five months? (Five years seems a long time for immediate responding.)

Or must we not hamstring high ideals with hard data?

W. L. Garner
Albuquerque, N. M.

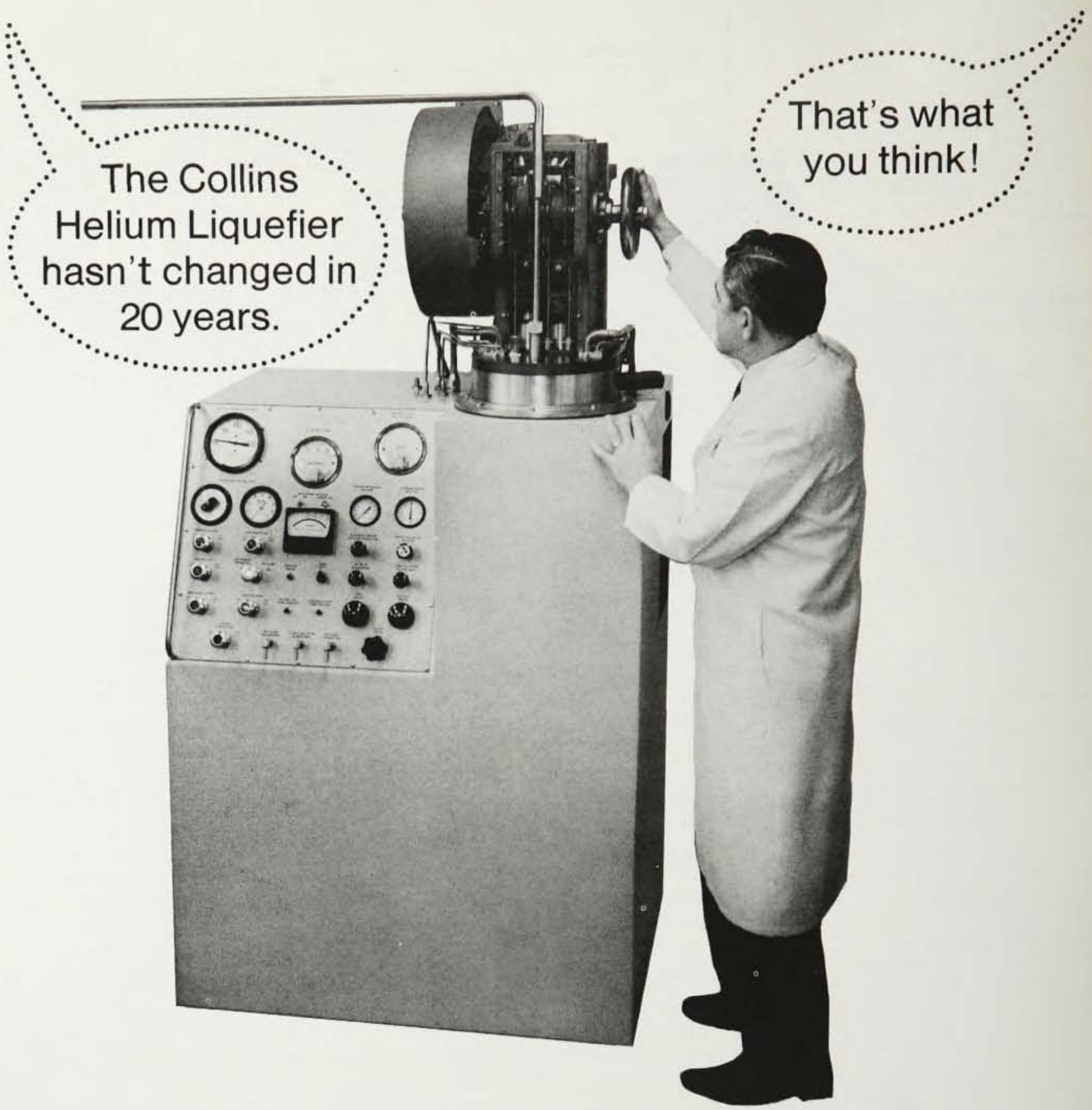
A REPLY FROM JAY OREAR: I feel it should be up to the individual university faculty to judge at what time a military threat is serious enough to justify the conversion of its university into a military training school and weapons laboratory. I remember during World War II my university was deeply involved in the training of military personnel and the development of new, secret weapons. At that time our nation was under military attack and defeat was a real possibility; so our entire country including its universities was under total mobilization. In brief, my answer to Dr Garner is that the university itself should decide "just when is a national emergency."

What bothers me about Garner's letter is the implication that the universities should be in the weapons business even in peacetime. Garner refers to the "long lead time required to develop sophisticated defensive weapons" and university scientists who "twiddle their thumbs until the emergency confronts them." Other than all-out mobilization, I see no need to make the universities a part of the defense establishment. Right now more than ample development of weapons is taking place outside of the university (and many university scientists do contribute on their own time).

However, in a larger sense the university itself does contribute to the national defense—perhaps even more than the military itself. A nation can be destroyed from within as well as from without. As long as the universities remain havens for open inquiry, debate and criticism free from outside influences, they can criticize and mobilize against tendencies that might destroy our nation from within. For example, one could make the case that the universities contributed significantly to saving the United States from McCarthyism. And today forces rising out of the universities are trying to reverse our path toward disaster in Vietnam and at home. The universities with their "childrens' crusades" have helped provide the electorate with real political alternatives. None of this could have happened if there had not been such a clear separation of the universities from the military establishment.

Jay Orear
Cornell University

ANOTHER REPLY: The FAS has made two policy statements, one recommending that universities should not


The Collins Helium Liquefier hasn't changed in 20 years.
That's what you think!

engage in the conduct or administration of classified research and the other recommending that universities should not engage in research and development of weapons or "devices designed to destroy human life or incapacitate human beings" nor should they "directly or indirectly take part in military operations or participate in the collection of military intelligence." Both of these statements were prefaced by "except in times of national emergency."

While we do not feel compelled to define emergency in detail, one might assume that if a situation becomes so dire that such measures as price and wage control, excess profits taxes, job freeze and mobilization of industry and manpower are justified, a relaxation of our guidelines might also be justified.

University scientists and universities are hardly twiddling their thumbs waiting for an emergency to confront them. They are constantly confronted with an emergency—the education of increasing numbers of young men and women and continual modernization of that education to meet the needs of society.

CAMERON B. SATTERTHWAITE
Chairman,
Federation of American Scientists

A question of credit

I feel obliged to compliment Hershel Markovitz for his extremely interesting historical documentation in "The Emergence of Rheology" (PHYSICS TODAY, April, page 23) and to respond to one point regarding the treatment of tube-flow data. Markovitz suggests that a derivation attributed to Karl Weissenberg finally made it possible to extract the invariant shear-stress–shear-rate relation for a rheologically complex fluid without invoking arbitrarily defined rheological models.

Presumably the following Poiseuille flow functions are the basis for these remarks

$$\frac{Q}{\pi R^3} \equiv F(\tau_w) = \frac{1}{\tau_w^3}\int_0^{\tau_w} \tau^2 f(\tau)\,d\tau \quad (1)$$

$$f(\tau_w) = 3F(\tau_w) + \tau_w \frac{dF(\tau_w)}{d\tau_w} \quad (2)$$

Here Q, R, and τ_w denote, respectively, flow rate, capillary radius and wall-shear stress, and $f(\tau)$ is the invariant rheological function relating shear stress and rate of shear.

We cannot agree with the implication that Weissenberg alone should be entitled to the credit for this significant development. According to the literature, equivalent forms of these expressions, *together with complete derivations,* appear in a classic paper written by R. Eisenschitz, B. Rabinowitsch, and Weissenberg (*Mitt d. deutsch. materialprufunganstalt,* **9**, 91, 1929). In their discussion, the authors emphasize that for a given fluid the dependence of Q on the pressure gradient and radius must be represented by a single function $F(\tau_w)$ if equation 2 is valid. Subsequent to this reference, these expressions are cited by Rabinowitsch (Z. Physik. Chem., **A145**, 1, 1929). He remarks that the underlying theory was presented by Herzog and Weissenberg at a meeting of the Colloid Society in Hamburg, 21 Sept. 1928 and reported in Kolloid-Z., **46**, 277, 1928).

Obviously, several *collaborators* were actively concerned with the problem of finding the unknown function $f(\tau)$ for colloidal solutions, described as "elastic fluids." It is indeed curious, and perhaps ironic in view of Markovitz's credit to Weissenberg, that this particular treatment of tube-flow data is also frequently credited in earlier rheological literature to either Rabinowitsch or Mooney.

J. G. SAVINS
Mobil Development
and Research Corp.

THE AUTHOR REPLIES: Savins is certainly correct in calling attention to the confusion concerning the origin of the formula for calculation of the viscosity or shear–stress function from tube-flow data. I cited only Weissenberg because both Rabinowitsch and Eisenschitz individually, in papers where they were the sole authors, state unambiguously that Weissenberg derived the equation (B. Rabinowitsch, Z. *Physik. Chem.* **A145**, 1 (1929), footnote on page 18, and R. Eisenschitz, *Kolloid-Z.* **64**, 184 (1933), p. 187. The latter paper also gives further historical details.)

There is an error in my paper that I would like to correct. B. D. Coleman has called my attention to the fact that "Young's modulus" was actually clearly and accurately introduced by Leonard Euler in 1727. See, for example, Clifford Truesdell, *Archive for History of Exact Sciences* **1**, 3, 1960, or G.A.E. Oravas in his Introduction to Carlo Castiagliano's *The Theory of Equilib-*