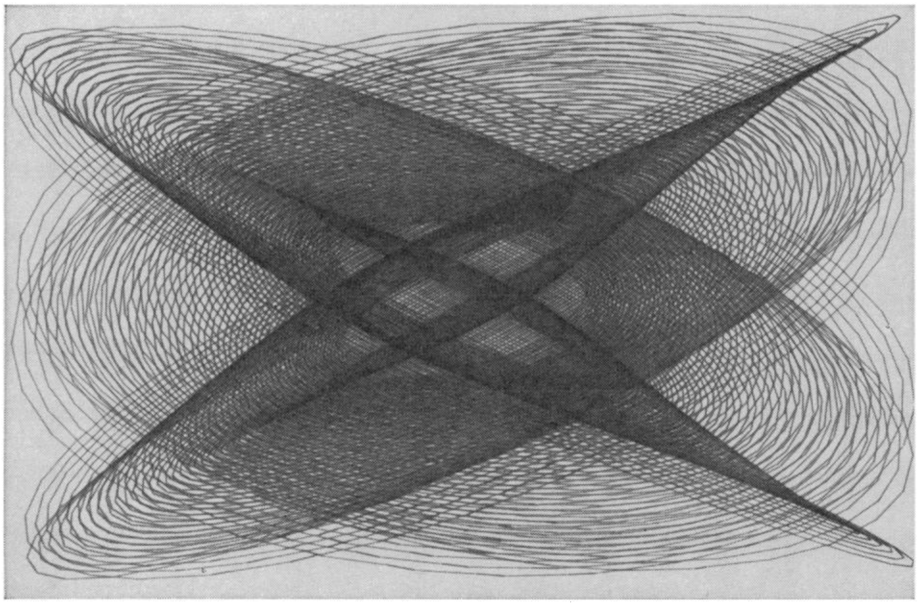

Fig. 1. A Lissajous figure produced by computer. The input data were $T_1 = 3.04$, $T = 3.65$, $\alpha = .7854$, $a = .900$, $b = .600$, $\delta t = .100$, total time = 912.0, and $\psi = 1.40 \times 10^{-3}$. Another example appears on the cover.

son 4020 printer-plotter. The 4020 is a device programmed to draw straight lines from one point to another on the face of a cathode-ray tube; it then photographs the output. The starting-point and end-point data of each line segment are fed into the 4020 on a magnetic tape produced by the 7044 from a program written in Fortran IV.

The equations describing the path of a particle undergoing the motion of a damped double pendulum are

$$x = ae^{-\psi t} \sin \left\{ \left[\left(\frac{2\pi}{T_1} \right)^2 - \psi^2 \right]^{\frac{1}{2}} t + \alpha \right\}$$

$$y = be^{-\psi t} \sin \left\{ \left[\left(\frac{2\pi}{T} \right)^2 - \psi^2 \right]^{\frac{1}{2}} t \right\}$$

where $\psi = \mu/2m$, μ is the coefficient of air friction, m the mass of the pendulum bob, α the phase angle, a and b the amplitudes, and

$$T_1 = 2\pi(l/g)^{\frac{1}{2}}$$

and

$$T = 2\pi(L/g)^{\frac{1}{2}}$$

are the fundamental periods.

Inputs to the program consist of T_1, T, α, a, b, δt, total time, and ψ; δt is the interval at which points (x, y) are calculated. The time t is initially set to zero, then increased by δt, until the time exceeds total time. Input values were chosen with Hales's figures as a guide.

The results compare well with those previously reported, although the pictures are not as esthetically pleasing because of the necessity of approximating a curve by drawing straight-line segments from point to point (Fig. 1). Smoother results could be obtained by choosing a smaller time increment, but this would increase the computing time and the expense. The total time required to produce a completed picture is about one minute.

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Conference Literature: Rebuttal

I take strong exception to the views of E. H. Ahrens, editor of the *Journal of Lipid Research* (16 Apr., p. 313), when he praises *Biological Abstracts* for its decision not to abstract (or even list and index by title) individual papers from "conference literature." Perhaps this term suggests the 10-minute papers presented by beginning investigators, but in actual fact it refers to substantial papers presented by distinguished, hand-picked, senior investigators at major conferences and symposiums.

Abstracts of these important papers have two purposes: (i) They call attention to work in progress, even if it is in preliminary form and not accompanied by experimental detail. (Unfortunately the lag in publishing abstracts detracts from this function.) (ii) They are a tool for retrospective searches. Scientists seldom use abstracts to find out what is happening in their own field of specialization, but when they are preparing bibliographies or looking for reviews in a peripheral field, abstracts serve a valuable function. For these needs, conference literature is probably the most useful type of published work. It summarizes and reviews results scattered in different journals over a period of years. It describes the most recent and significant findings as well as work in progress. It presents views, perspectives, and interpretations which often will not pass the severe restrictions imposed on regular journal articles by limited space and conservative editorial policy. Apparently the value of conference literature to those who *use* abstracts is not fully appreciated by the editors and advisers of primary journals and hard-pressed abstracting services.

A major error in Ahrens position is his "main objection" that "the individual contributions are seldom subjected to critical review" and that "there is no check whatever on [the] scientific quality [of the work]." Has it been overlooked that these papers are presented to the most critical peer group possible —a group whose collective impression will determine the professional future of the speaker? Could one or more isolated referees be more critical than a roomful of scientists, many of them working in the same field, who are ready to catch the speaker on the slightest error in his procedure or conclusions? What scientist would dare to present results or speculation in such circumstances without adequate experimental data and without the most careful, critical self-appraisal?

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"America" Defined

At the turn of the century, as I collected iguanas in the shadow of the active volcano Colima, an Indian told me that I was not an American but he was.

Science (7 May, p. 787) repeats the claim of the University of Pennsylvania that its medical school, "founded a decade before the Revolutionary War, ranks as America's oldest school of medicine." In fact, the Medical School at San Marcos University, Lima, Peru, antedates the one in Philadelphia. Juan de la Fuente, M.D., was professor of medicine in the Real Universidad de México in 1580. It may be that the first medical work in America was the Badianus Manuscript, which was buried for 400 years in the Vatican Library and is the only surviving evidence of medical practice by Maya or Aztec, whose great libraries were burned.

Since we spend billions of dollars on our Good Neighbor Policy, would it not be good policy to remember our neighbors now and then?

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Fast-Reactor Programs Here and Abroad

In his review of the four volumes listed under the heading "Peaceful uses of atomic energy" (12 Feb., p. 721), J. D. Cockcroft expresses two opinions which I would question.

The first is one in which he refers to the fuel and fuel cycle of the Experimental Breeder Reactor 2 as "unconventional." The nuclear-power business is only about 15 years old. Within this time span, it is difficult to argue that any phase of reactor work has become conventional. Perhaps the closest to conventional power-reactor fuels are the uranium oxide fuels for water reactors and the Magnox fuels for the British gas reactors. The only fuel-processing method that is in any sense "conventional" is the aqueous method. Both the fluoride-volatility and pyroprocessing (used with EBR-2) methods are receiving serious consideration for commercial application by U.S. industry, but at present these are obviously "unconventional" because they are untried on a large scale. Studies currently under way at Argonne National Laboratory on both these new processing methods indicate that the processes are technically and economically sound. And the metallic-fuel systems which can be accommodated by the pyroprocessing method can be uranium-plutonium with or without any desired alloying addition. The fissium alloying elements need not be used. The decision which resulted in the fissium fuel for EBR-2 was made at least seven years ago. Certainly progressive changes take place.

The second opinion is expressed in the sentence "The United States effort in fast reactors, after leading with Experimental Breeder Reactor 1, has fallen several years behind that of Britain and Russia." It is true that Britain has had the Dounreay fast reactor and the Russians the BR-5 fast reactor operating at power for some time, whereas the EBR-2 and the Fermi are not at design power. The EBR-2 has operated to 45 megawatts and has demonstrated very stable conditions. But the total fast-reactor program is not tied up in the reactors themselves. The United States has significant development efforts in uranium-plutonium fuels of the metal, oxide, and carbide types, in sodium-system components such as boilers and pumps, in fast-reactor physics and safety, in the reprocessing of fuels of high plutonium content, and in the engineering studies of large (1000-electrical-megawatt) fast-reactor systems. Most of this work is being funded by the United States Atomic Energy Commission, and all such work is reported in open literature which is available to Britain and Russia. While I believe the U.S. has not "fallen several years behind," it is not possible to substantiate my belief because the comparable developmental efforts in Britain and Russia, particularly in fuel- and reactor-system studies, are not reported for general distribution.

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Silicone Producer

The excellent article "Chemical background of silicone" by J. F. Hyde (19 Feb., p. 829) mentions the three older silicone producers in the United States. Your readers may be interested to know that Stauffer Chemical Company's Silicone Division, in Adrian, Michigan, is on the threshold of becoming a fourth major silicone producer. Its first integrated plant will start producing a complete line of silicone fluids, rubbers, and resins by the end of summer, 1965. The research and development laboratories, moreover, have been working on various innovations which are expected to contribute to further significant advance in organosilicone technology.

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Information Exchange Group No. 1

In early 1961 the first Information Exchange Group (IEG No. 1) was set up as an experimental venture under the aegis of the National Institutes of Health and the initiative of Errett C. Albritton. It covered the field of electron transfer and oxidative phosphorylation. The starting premise was that the usual exchange of information among workers in an active field is highly inefficient and that this inefficiency is a major deterrent to rapid progress. The IEG was designed to maximize exchange of information in a given field of science. It is now possible to evaluate accurately what has been achieved in one field in the course of a four-year trial period.

The membership of IEG No. 1 includes every active worker in its designated field in this country and abroad. At least 90 percent of the important papers published anywhere in the world on electron transfer, oxidative phosphorylation, and related topics are submitted to the IEG, and these reach the membership 3 to 12 months before the same papers can be read in the usual journals. Despite the absence of any editorial screening, the papers submitted to the exchange (a total of over 300 at the time of writing) have been of uniformly high quality. The judgment of one's peers serves as a major deterrent to the submission of marginal papers of the potboiler variety.

The IEG has been of special assistance to research scientists in foreign laboratories who previously have been isolated from the mainstreams of meetings and word-of-mouth reports. The IEG has, in fact, equalized the opportunities for everybody in its research area, worldwide, to be "in the know" and to share in the rapid dissemination of information.

The IEG offers a forum for discussion of controversial matters, and this forum has made it possible to air differences almost as soon as the triggering paper is published. Controversy and discussion have been rehabilitated as necessary and desirable instruments of scientific progress.

In replies to a recent questionnaire sent to 50 members of the IEG, there was complete unanimity that the IEG is fulfilling a unique and indispensable function.

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