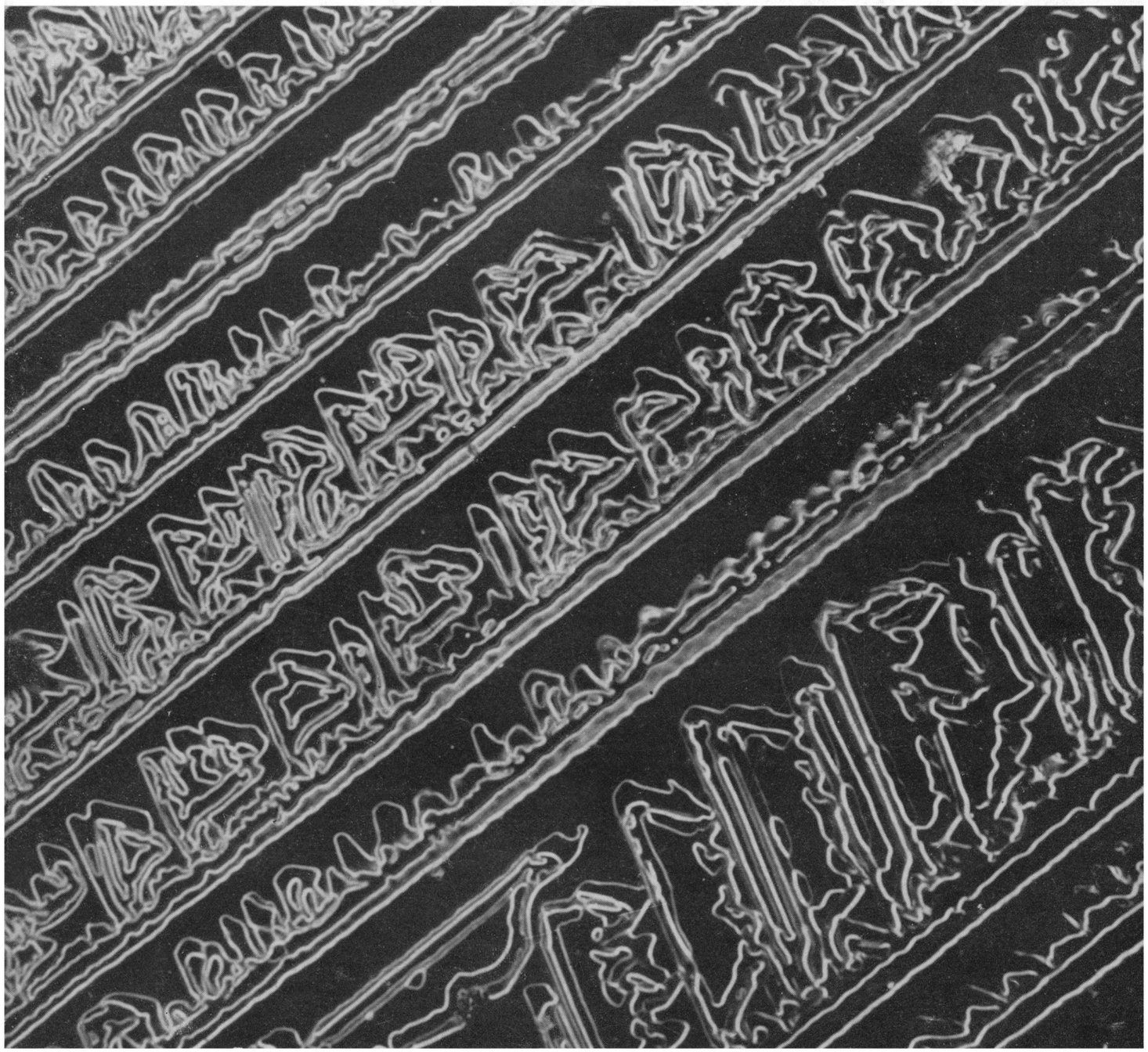
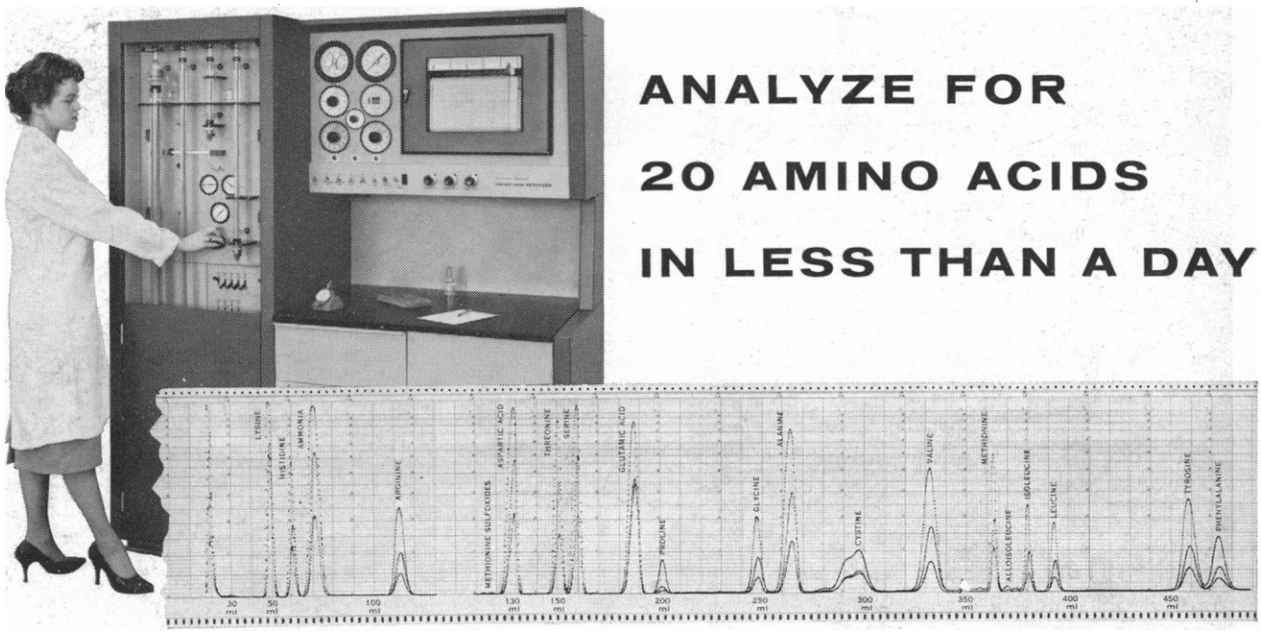
SCIENCE

10 June 1960

Vol. 131, No. 3415

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE





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19WS

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10 June 1960, Volume 131, Number 3415

SCIENCE

Cover

Thiocarbamide crystal. Dark-field illumination (× 575). A drop of solution is kept under observation until a suitable arrangement appears. The solvent determines the pattern produced during crystallization. Rapid crystallization produces a coarse pattern, slow crystallization a fine one. [L. C. Massopust, Marquette University School of Medicine]

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Letters

Chronology of the Last Glaciation

James B. Griffin in his extremely interesting article, "Some prehistoric connections between Siberia and America" [*Science* **131**, 801 (1960)], states that "the Wisconsin ice advance is thought by some Pleistocene students to have begun about 50,000 B.C., followed by a warmer period corresponding to the Würm interstadial in Europe. This may have provided an ice-free corridor east of the Rockies some 30,000 years ago."

I have no doubt that Griffin is right about the thinking of some, if not many, Pleistocene students. Apparently a short but important paper by H. Tauber and H. de Vries [*Eiszeitalter und Gegenwart* **9**, 69 (1958)] has received less attention than it deserved. According to these authors, samples for radiocarbon dating from the Würm interstadial deposit at Brörup, Jutland, showed no significant activity after thorough decontamination. "This means," they write, "that the interstadial at Brörup and the preceding cold period are older than 50,000 B.C." And, one may add, perhaps much older.

It has been [D. B. Ericson and G. Wollin, *Micropaleontol.* **2**, 257 (1956)] and still is my guess that the Würm I-II or Brörup interstadial is represented in the deep-sea sediments of the North Atlantic by a well-defined faunal zone containing low-latitude species of planktonic foraminifera among which *Globorotalia menardii flexuosa* is especially abundant. From the stratigraphical position of this zone, at the base of a relatively thick layer of sediment with cold-water species throughout, which lies, in turn, directly beneath postglacial sediment, it is difficult to see how it could be anything else than the Würm interstadial. Now, extrapolation of rates of sediment accumulation determined by radiocarbon dating of many samples from long sediment cores from various parts of the Atlantic, Caribbean, and Gulf of Mexico has shown that the time interval represented by the zone containing *G. menardii flexuosa*, or the Flexuosa zone, came to an end about 65,000 years ago.

Of course, my correlation of the Flexuosa zone with the Würm interstadial may be wrong in spite of its apparent plausibility. Even so, the important fact remains that Tauber and de Vries have shown conclusively that the climatic amelioration which separated the early and late Wisconsin glaciations occurred more than 50,000 years ago, and that therefore the short chronology of the last glaciation must be abandoned. Accordingly we conclude that if early man entered America during the Würm interstadial, he must have done so at least 50,000 years ago, and perhaps no less than 65,000 years ago. This does not impair Griffin's argument regarding the time of man's appearance in America. If anything, this longer chronology strengthens his conclusion that man did not enter America during the interstadial between the early and late Wisconsin glaciations.

DAVID B. ERICSON
Lamont Geological Observatory, Palisades, New York

Emotionality and Fear

Harlow and Zimmermann's description of "Affectional responses in the infant monkeys" [*Science* **130**, 421 (1959)] was a gem, but it did, I believe, contain a minor flaw.

Although it clearly described "affectional responses," which are indeed emotional responses, when the term *emotionality* was used, it seemed that its meaning was limited to the disruptive emotion of fear. This can be seen in the use of the term *emotionality index* rather than *fear index*, and in the following statement (p. 425): "Children in the first group (mother present) were much less *emotional* [italics mine] and participated much more fully in the play activity than those in the second group (mother absent)." This first group was not less "emotional" but less fearful; presumably, if the first group was happier, it could also be called *more* emotional than the second.

Harlow and Zimmermann seem implicitly and inaccurately to equate emotionality with fear, an equation which would lead us to see courage, for example, as equivalent to emotionlessness. A very fine critique of the theory "according to which emotions are disorganized or disruptive states" is to be found in V. J. McGill's *Emotions and Reason* (Thomas, Springfield, Ill., 1954).

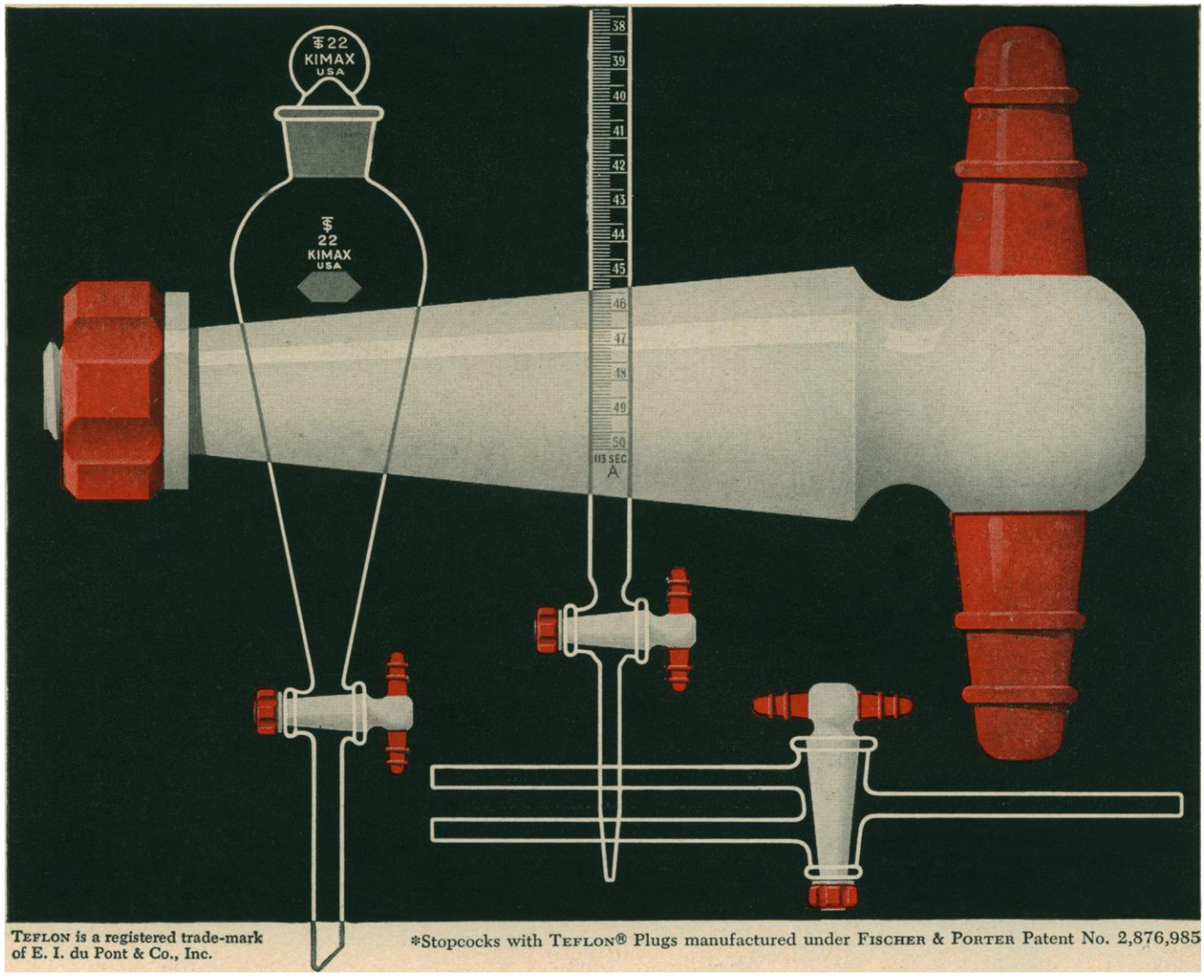
Hence I think their fine article would be even finer if this inaccuracy in the use of the concept "emotionality" were clarified.

NATHANIEL S. LEHRMAN
15 Canterbury Road, Great Neck, New York

I am in agreement with Lehrman's position and have long been an opponent of those psychologists who would identify and define emotion as "disorganized and disruptive states." In an earlier paper [*Am. Psychologist* **12**, 673 (1958)], I unequivocally define love as an emotion, and I still subscribe to this theoretical position.

The term *fear index* would have been

(Continued on page 1740)



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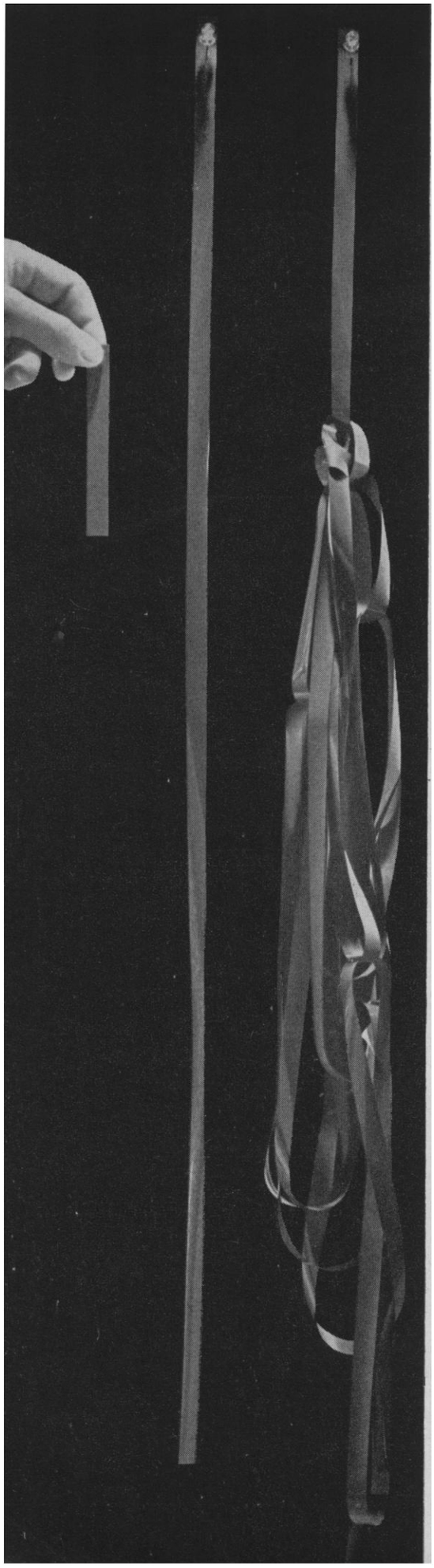

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10 June 1960, Volume 131, Number 3415 **SCIENCE**

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Meanwhile, Down in the Valley

Last month saw the collapse of negotiations at the summit, with accusations from Moscow that Washington had wrecked the Paris meeting by staging the 1 May flight of the U-2 reconnaissance plane, and accusations from Washington that Moscow had been determined all along that nothing should come of the Big Four conference. At a lesser height, the quarrel moved into the United Nations Security Council where the Soviet Union sought unsuccessfully to get the United States indicted for "aggression." Down in the valley, however, at the level of physics, medical science, and the arts, the world was treated to the sight of another aspect of the East-West dialogue continuing undisturbed.

Premier Khrushchev may have withdrawn his invitation to President Eisenhower to visit him in the Kremlin, but exchange programs that had started before the crisis proceeded right through it. American physicists visited various high-energy research centers in the Soviet Union, American medical scientists attended a conference in Moscow on poliomyelitis, and the American violinist Isaac Stern played encores to Russian applause. In this country, Soviet scientists specializing in thermonuclear research visited the James Forrestal Laboratory at Princeton University and other laboratories, and Soviet medical scientists attended conferences at Johns Hopkins and elsewhere on heart disease and related problems.

The hope that exchange programs will promote mutual trust is equally strong when relations on the political level are not so favorable as when they are favorable. An increase in mutual trust at the top can lead to greater opportunities for exchange below, and bringing people together at lower levels in activities that command common loyalties can affect relations above. Exchange may promote mutual confidence by correcting the distorted images that East and West may have of each other. We do not expect that at the mere sight of our magical presence all Russia will break out into "The Stars and Stripes Forever," but we can hope that those with eyes to see will learn that we are not a nation of hungry, exploited workers ripe for revolt against our secretary-chasing, cigar-chomping masters. And, for our part, in observing the Soviet way of life, we will never tire of watching demonstrations that the Russia of Khrushchev is not that of Stalin.

An increase in mutual trust generated by exchange programs can also bear on more specific American goals such as carrying out a step-by-step disarmament program. Part of the theory underlying this program is that the confidence between East and West developed in one step will furnish the confidence needed for the next step. But since new negotiations can also be new sources of suspicion, independent methods for building mutual trust are also necessary.

President Eisenhower has called for a continuation of cultural contacts and "businesslike dealings" with Soviet leaders, while Premier Khrushchev has found this statement of "positive value." This is good news. In the technical portion of exchange, our hope is that scientists, in seeking broader avenues of communication, will help reduce the tensions that their own achievements have made so dangerous.—J.T.

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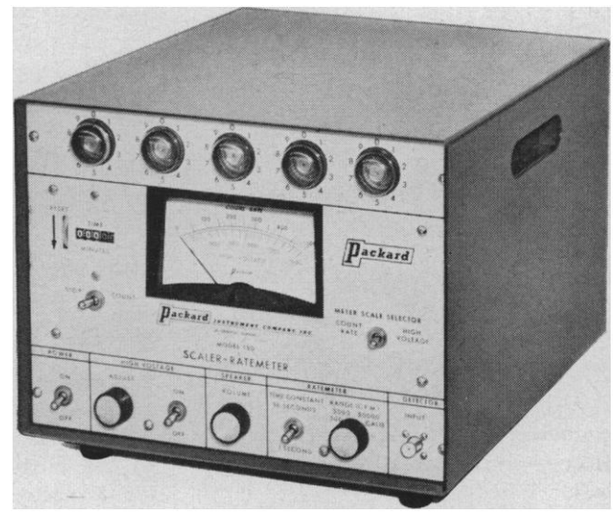

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Letters

(Continued from page 1700)

improper, since many of the objective check-list items measured behavior more adequately designated as distress, disturbance, yearning, and displaced aggression. The term *disturbance index* could be substituted for *emotionality index* without semantic loss and, probably, without semantic gain.

HARRY F. HARLOW
Department of Psychology, University of Wisconsin, Madison

Small Colleges and Small Minds

The lack of interest of some teachers in research, discussed in the editorial of 8 January [*Science* **131**, 71 (1960)], is disturbing, but so also are some aspects of the editorial itself.

The heading "Small colleges and small minds" implies that these go together. This guilt-by-association technique is used several times in the editorial, though the man whose views are cited as the basis for the editorial is the president of a "small college." Is there evidence for a larger proportion of so-called small minds in small colleges?

The following statement is perhaps the worst part: "The core of the argument for scientific research . . . is that while there may be good research scientists who are not good teachers, the evidence is that there are no good teachers whose competence is not increased by good scholarship." Ostensibly this places the research scientist above reproach and leaves the incubus on the teacher. Actually, the statement is a *non sequitur,* and its converse is equally true—and unfair, in turn, to the research scientist. It is suggested that the reader substitute the word *editors* for *teachers* (he will find it equally correct). Competence in any profession would be increased by good scholarship, as the editorial in question seems to illustrate.

After this unfair statement the editor changes from "good scholarship" to "research" in the next sentence, which again helps to put the research scientist beyond criticism and implies criticism of the teacher. Had he chosen to make a straightforward statement of what he implies it might have read something like this: "While there may be good research scientists who are not good teachers, the evidence is that there are no good teachers who are not good research scientists." This is, I suggest, rather untenable.

In the next paragraph we are told: "A prominent figure on many campuses is the instructor who is forever marking exams, grading papers, and drawing curves representing his students' performance." With our present grading system instructors are inevitably marking exams, and so on, but the editor is depreciating the teacher with the guilt-by-association technique again, for he indicates that these instructors have "schemes" of a detrimental nature. But the scientist could be given the same unjust treatment, in very similar phrases: "A prominent figure in many research laboratories is the scientist who is forever looking at figures, evaluating data, and drawing curves representing his results. He is full of schemes . . . that if instituted would require the assistance of all his colleagues." Though, curiously, in this case involving one's colleagues becomes a virtue and is extolled under the name of "scientific teamwork."

But this is not all. Having implied that no research means no scholarship, the editor completes his degradation of the teacher in the next sentence by suggesting that those concerned with students' performance are even against reading books!

In the last paragraph the editor proposes the right question, but for the wrong schools, when he asks: "But why in small colleges should some instructors oppose the recognition of good research as a consideration second to good teaching?" In so far as this opposition exists, it is typical not of the small school but of the large school with an extensive graduate program, where some instructors want recognition of good research first and of good teaching second, if at all. In large measure the apparent hostility toward research in the small college is manifested by instructors who do not oppose research as such, but oppose the evaluation of good teaching as a consideration second to good research, because they have seen the unfortunate results of this practice, especially since all too often a department finds itself with "research scientists who are not good teachers."

WILLIAM K. NOYCE
University of Arkansas, Fayetteville

It was with considerable interest that we read your editorial "Small colleges and small minds," for here at Wilkes College the subject of research in small colleges has been of more than academic interest. Wilkes, I believe, would fulfill your criteria for a small independent liberal arts college. Our experience in the initiation and conduct of a research-teaching program has been satisfactory and rewarding. A summary of our findings may be of interest

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color lore that might have bored Goethe to death . . . what to do with wiggle-bearing paper . . . correspondence concerning dimerization of cyanamide

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Amid rampant intellectuality, it has behooved us to tread lightly and confine our thinking to such *farbenlehre* as will fit us the better to flood the earth with color photography, myriad-colored Tenite plastics, color-locked Chromspun fibers, and Eastman textile dyes. Plus another field of dye art, tiny in economic comparison and disproportionately demanding in technical patience but important to those who, whatever their theories or purposes, wish to modify spectral distribution or overall intensity of light in systematic, quantitative, reproducible, simple, and inexpensive fashion. We refer to the celebrated little marvel of precision dye chemistry, the *Kodak Wratten Filter* of uniform gelatin, with or without glass mounting.

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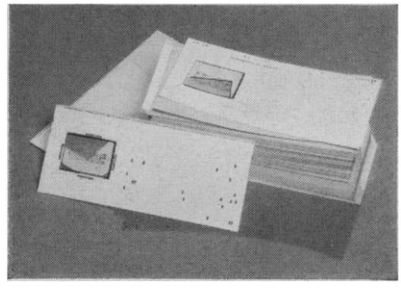

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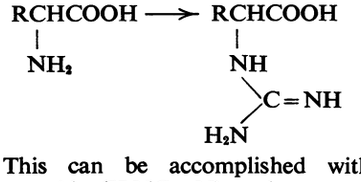
filming with punched cards. They say they would be willing to help you, too. They suggest you write them at 415 Madison Avenue, New York 17, N. Y., for a copy of "The Use of Aperture Cards for the Consolidation of Spectrophotometric Data."

Suggestion to Ames, Iowa

Cyanamide (note the "e"; very, *very*, VERY important) is not stable. On that, Walter R. Hearn of Iowa State University and we agree. Dr. Hearn is interested in guanidation of amino groups in peptides and protein, i.e.



This can be accomplished with cyanamide ($H_2NC{\equiv}N$) and some of its derivatives. There was a problem. In thinking of well-known chemical houses with whom to take up a problem involving cyanamide, one doesn't necessarily think first of us, but Dr. Hearn had somehow formed the impression that we were friendly fellows. Another factor which might have contributed to his decision to write us was the fact that six bottles of cyanamide in his stockroom, which showed melting points as much as 150° higher than they were supposed to, happened to bear our P1995 label.

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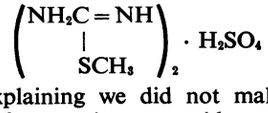

by explaining we did not make the latter from pesky cyanamide.

We further suggested how he might obtain the oxygen analog of this isothiouronium salt which he preferred for some of his guanidation operations. We referred him to a paper by one of our cousins at Kodak Limited in England (*Journal of the Chemical Society, 1955*, 3551), where cyanamide is by-passed by smooth methylation of urea with methyl toluene-p-sulfonate to give a good yield of readily isolated product. Finally we proposed that if he did not want to try this himself we would, for 96 bucks, cash on the barrelhead, deliver to him 500 grams of O-methyl-2-pseudourea sulfate.

That ought to teach Walter R. Hearn of Ames, Iowa, not to begin a letter with, "Since you have not acknowledged my letter of September 22, I thought perhaps you had gone out of business."

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to a general scientific audience. We published a fuller description in the *AIBS Bulletin* [**8**, 16 (1958)].

We found at Wilkes that the minimum prerequisites for establishing a research program are: teachers with an interest in conducting investigations and with the ability to communicate that interest to students; a sympathetic attitude on the part of the college administration; realization that the merit of a given piece of research is not measured only by the magnitude of the study or by the amount of technical apparatus it requires; selection of a problem suitable for investigation by a group; adaptation of student laboratory equipment for special needs and utilization of existing classroom space by appropriate planning; enlistment of the librarians of the college in seeking the cooperation of local hospital and industrial libraries and that of university and governmental library loan and microfilming services; use of undergraduate assistants—under supervision they have often proved as valuable as the average technician working solely for a salary; and, finally, invitation of scientists residing in the community to participate in the research-teaching program.

SHELDON G. COHEN
CHARLES B. REIF
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From my limited experience it would seem that problems of opposition to research on the part of any college staff member are minor. The real problem, as usual, is one of finance or stimulation of interest. Small colleges are not even in the running when it comes to the money spent by the larger institutions just to line up federal grants, to lobby the legislature, or to secure research money from industry or philanthropical organizations.

The faculty and students in most colleges are usually a step or two ahead of the administration and sources of income both as to the desirability of research and the time and minor facilities necessary to be devoted to studies.

A. D. MOINAT
Colorado State College, Greeley

It is easy to agree with Wiggers [*Science* **131**, 942 (25 March 1960)] that "larger colleges do not have a monopoly on students with ability, curiosity, and desire." This is a truism. What he overlooks in asserting that smaller colleges do not supply their share of the scientific talent of the country is that they have, in fact, provided a disproportionate share of scientific personnel. As reported in the October 1948 issue of *Fortune* and in the study of the origins of American scientists by R. H. Knapp (1952), one of the anomalous conclusions was that most Ph.D.'s in science received their undergraduate training in small and even obscure colleges. The productivity of these smaller colleges, measured as a proportion of the number of graduates, contrasts strangely with the low productivity of larger institutions famed for research and staffed by prominent scientists. Most of the faculty members responsible for stimulating these undergraduates to pursue scientific careers were not themselves well known as researchers.

Reportedly, in recent years the larger and wealthier institutions have performed somewhat better in fulfilling their obligation to provide inspiration, challenges, and opportunities leading to scientific careers.

It may be that the commonly overworked science teacher in the smaller college somehow transferred his own thwarted research ambitions and motivation to students who showed promise and interest in a scientific career. Whatever the explanation, it is unfair to castigate these persons and institutions for not producing scientists when they have produced scientists and scholars in larger proportion than their numbers, faculty, facilities, or financial status would seem to warrant.

ROBERT P. MCINTOSH
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The responses of Wiggers and of Allen to the editorial "Small colleges and small minds" reflect two views on the subject of teaching and research in the small college neither of which are entirely realistic. I am in a position to know that research of a serious nature not only can be pursued in a small college but serves as a potent stimulus to student curiosity and interest and gives the staff member a sense of fulfillment which teaching alone seldom does. Allen's rather cynical comment that "teaching should be more than a meal ticket for researchers" should not be considered a universal attitude among college scientists.

At this college and in this department the research program during the academic year is necessarily curtailed because of teaching duties. But there are virtually 4 months of summer during which research is pursued without interruption. In the early stages support must be had from the college itself, but if the caliber of the research is sufficiently high, outside support in the form of grants is available.

No matter how much this subject of college research is kicked around,

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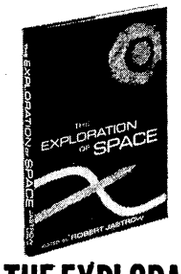

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however, the fact remains that, wherever he may find himself, the scientist who pursues research out of a deep inner urge to do so will find ways and means of satisfying his needs. It is this individual and not the opportunist who does research for what he may get out of it in the way of promotion who will, with some support, reveal the now almost hidden sources of research talent that are present in our small colleges.

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Obesity and Steroid Hormones

In the 5 June 1959 issue of *Science* [**129**, 1546 (1959)] there appeared a report on "Storage of steroid hormones by adipose tissue in two experimental obesities," by Zomzely, Asti, and Mayer.

This otherwise admirable article contains an inaccuracy in its reference to the studies of Gallagher, Fukushima, Barry, and Dobriner [*Recent Progr. in Hormone Research* **6**, 131 (1951)]. In the last three sentences of the article by Zomzely *et al.* it is stated that a large amount of fat in obese individuals may favor retention of their own steroid hormones, and that storage of administered hormones in fat depots might have therapeutic significance. The work of Gallagher *et al.* is cited in connection with this statement.

I did not detect any reference to this point of view in my examination of the article by Gallagher *et al.* However, an explicit prediction of the findings contained in the *Science* article, and of the probable importance of this in the control of obese patients, was adumbrated by S. G. Margolin in a communication to me in 1953.

With Margolin's permission, his hypothesis was summarized in a chapter of a book in the editing of which I collaborated at about that time [E. D. Wittkower and R. A. Cleghorn, Eds., *Recent Developments in Psychosomatic Medicine* (Lippincott, Philadelphia, 1954)]. It may be of some importance to draw attention to this, particularly because of one suggestion contained therein—namely, that in the dieting of obese patients, a reinforcement of the biological instinctual appetite to eat may occur with the release of steroid hormones as the patient reduces weight. No attention seems to have been paid to this point in the literature, and it is a suggestion of sufficient interest to be entertained.

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Meetings

Pacific Division, AAAS

The 41st annual meeting of the Pacific Division of the AAAS will be held at the University of Oregon, Eugene, 13–18 June 1960. Arrangements are in charge of a local committee headed by Bradley T. Scheer, head of the department of biology, to whom inquiries regarding the meeting should be addressed.

The president of the Pacific Division is Henry P. Hansen, dean of the Graduate School, Oregon State College, Corvallis. The retiring president is Henry Eyring, dean of the Graduate School of the University of Utah, Salt Lake City. The president elect is Wilbert A. Clemens, professor of zoology, emeritus, University of British Columbia, Vancouver.

Among highlights of the Eugene meeting will be the divisional symposium on man's exploration of space and the presidential address, entitled "Cycles and Geochronology."

The 40th annual meeting, held in San Diego, 15–19 June 1959, was hosted by five local institutions: San Diego State College, the Scripps Institution of Oceanography (University of California), the U.S. Navy Electronics Laboratory, the Zoological Society of San Diego, and the San Diego Society of Natural History. Local arrangements were ably handled by a committee headed by George E. Lindsay, director of the San Diego Museum of Natural History. Most of the sessions were held on the campus of San Diego State College.

Outstanding among the many excellent programs were the divisional symposium on results of the International Geophysical Year, moderated by Joseph Kaplan, chairman of the U.S. Committee for the IGY, and the presidential address of Henry Eyring entitled, "The Chemist Looks into the Future."

The total registered attendance of 1448 included representatives of 37 societies affiliated with the Pacific Division, a number of societies not affiliated, and 207 persons who failed to note their society affiliations. The registrants represented 17 states, the District of Columbia, the Virgin Islands, the Canal Zone, and 13 countries other than the United States and Canada.

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