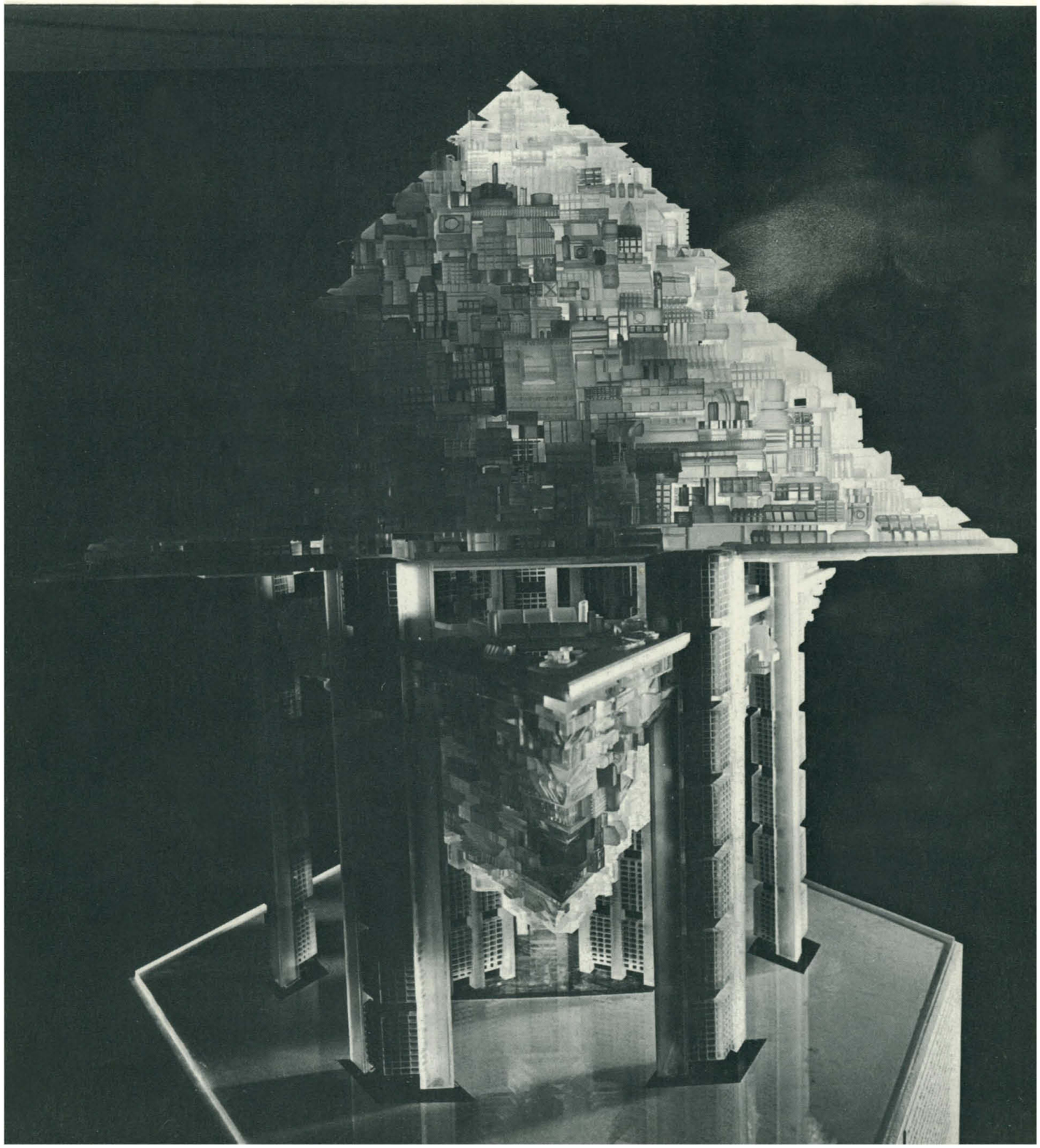
SCIENCE

6 November 1970

Vol. 170, No. 3958

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE



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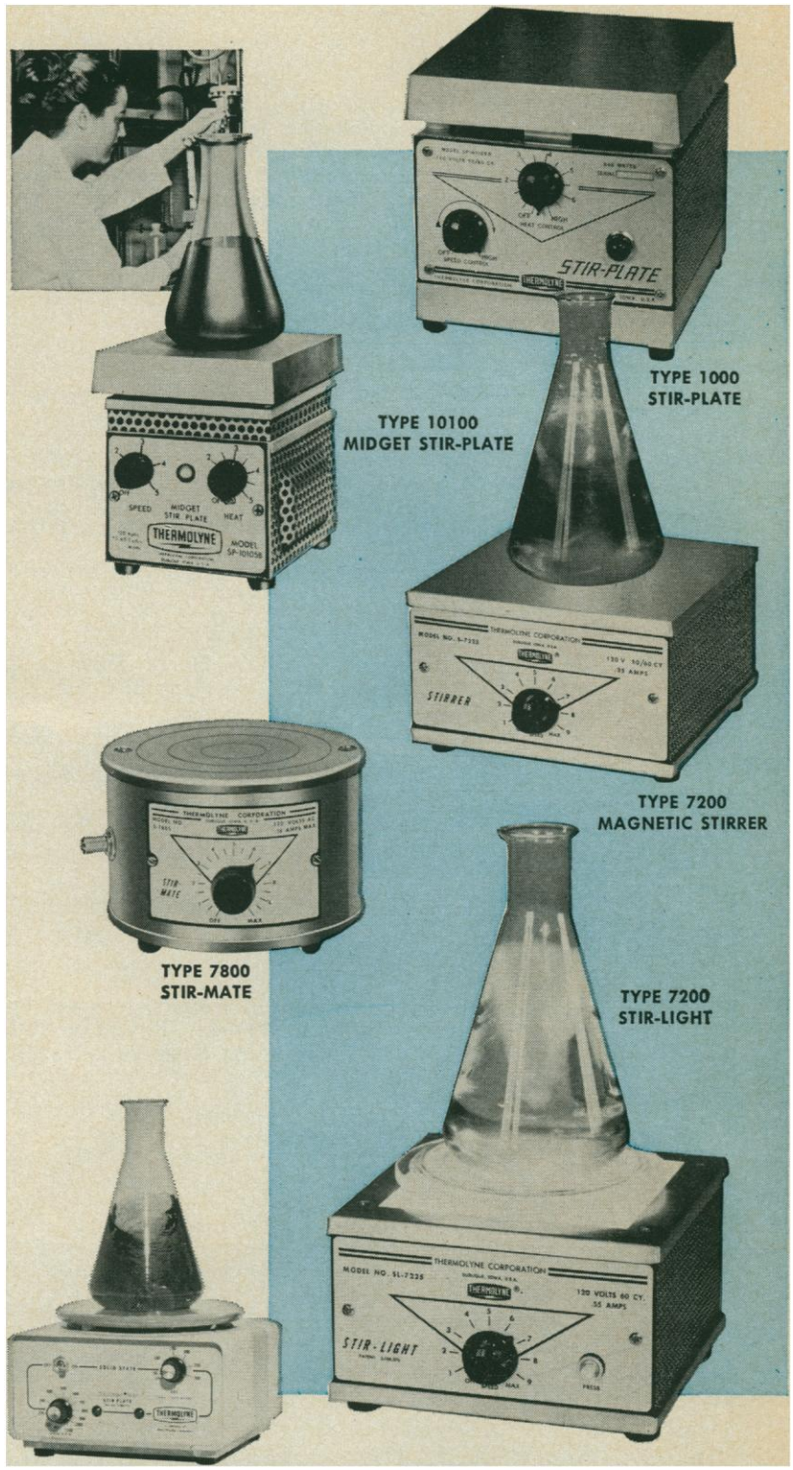

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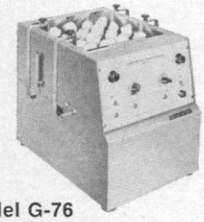

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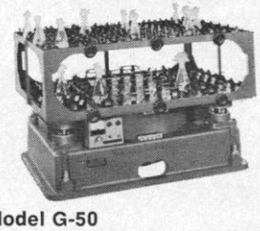

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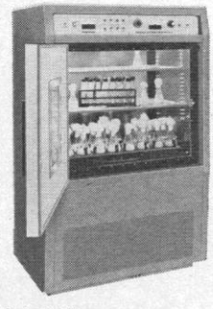

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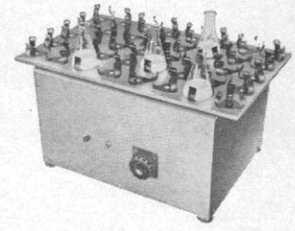

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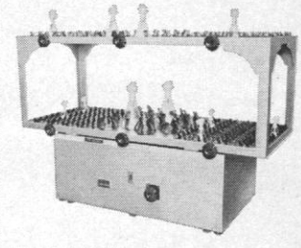

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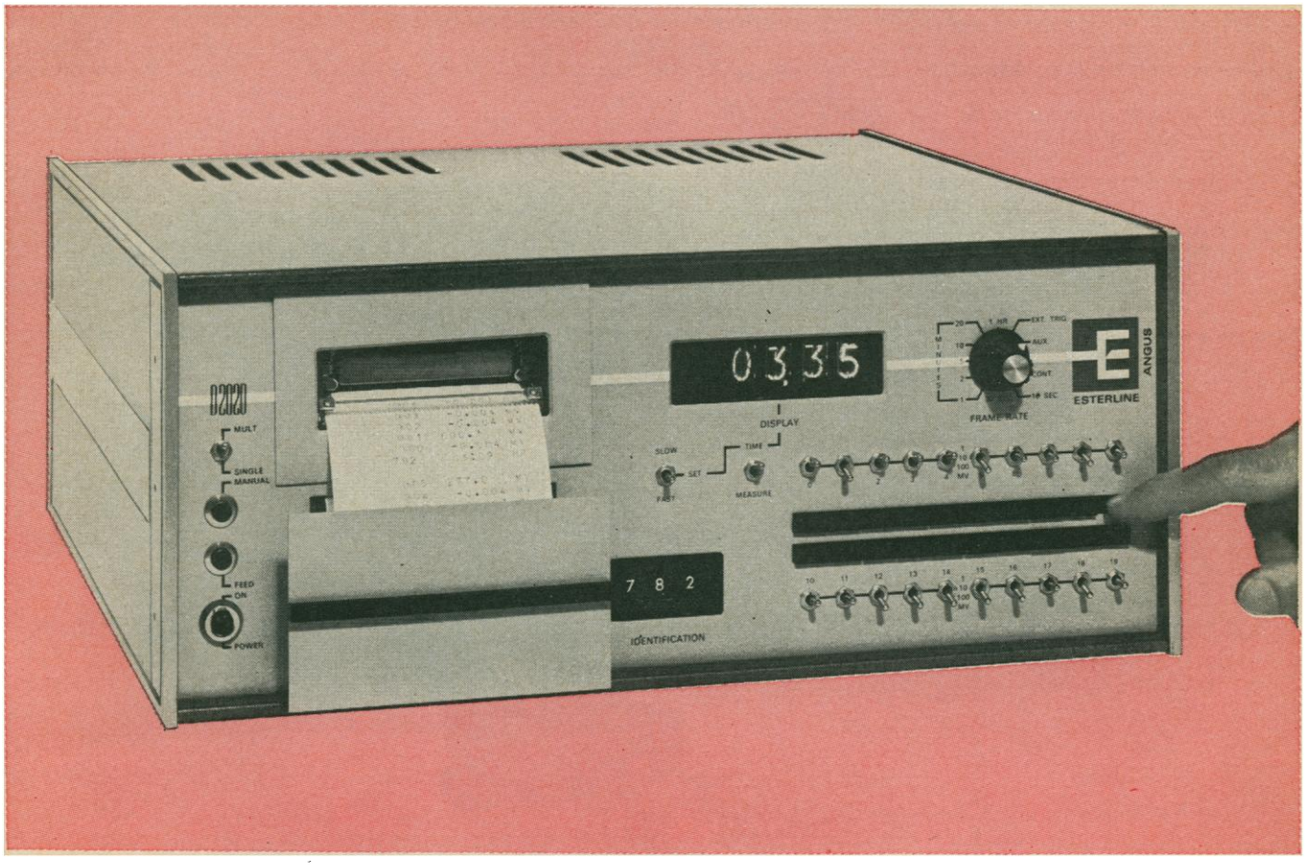

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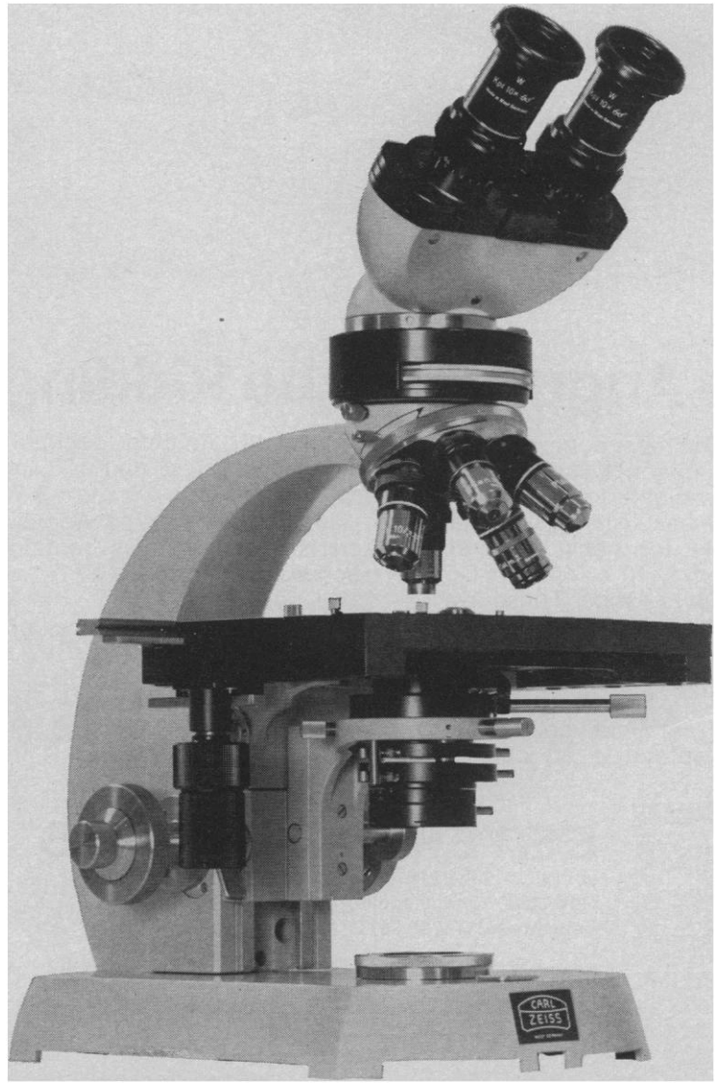

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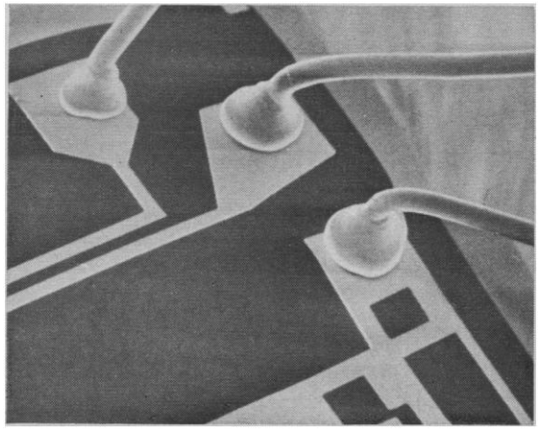

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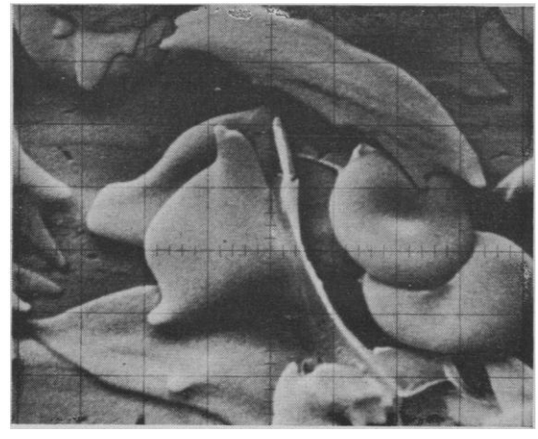

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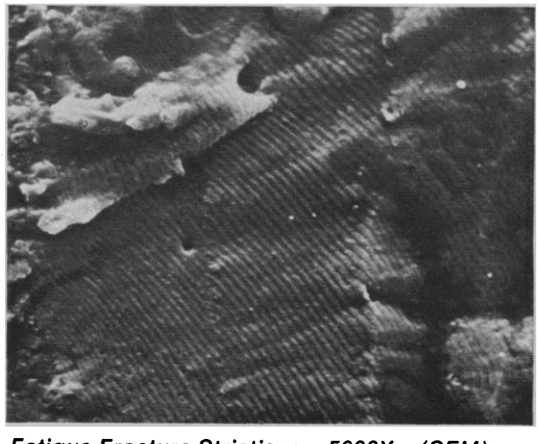

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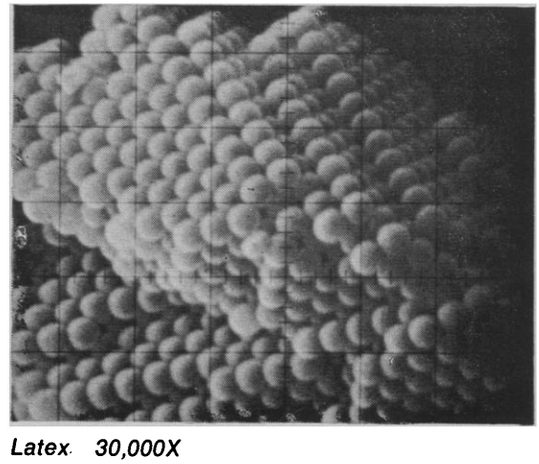

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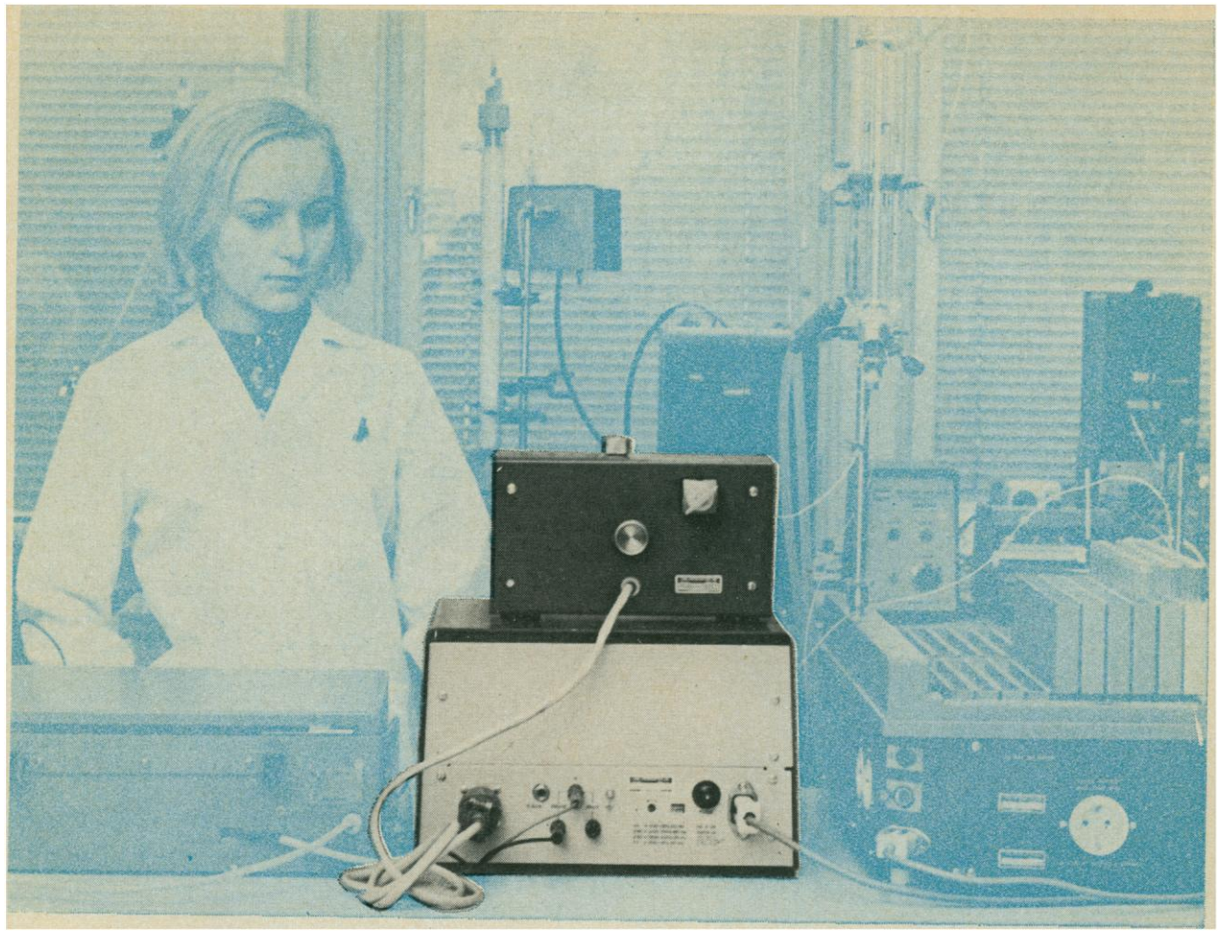

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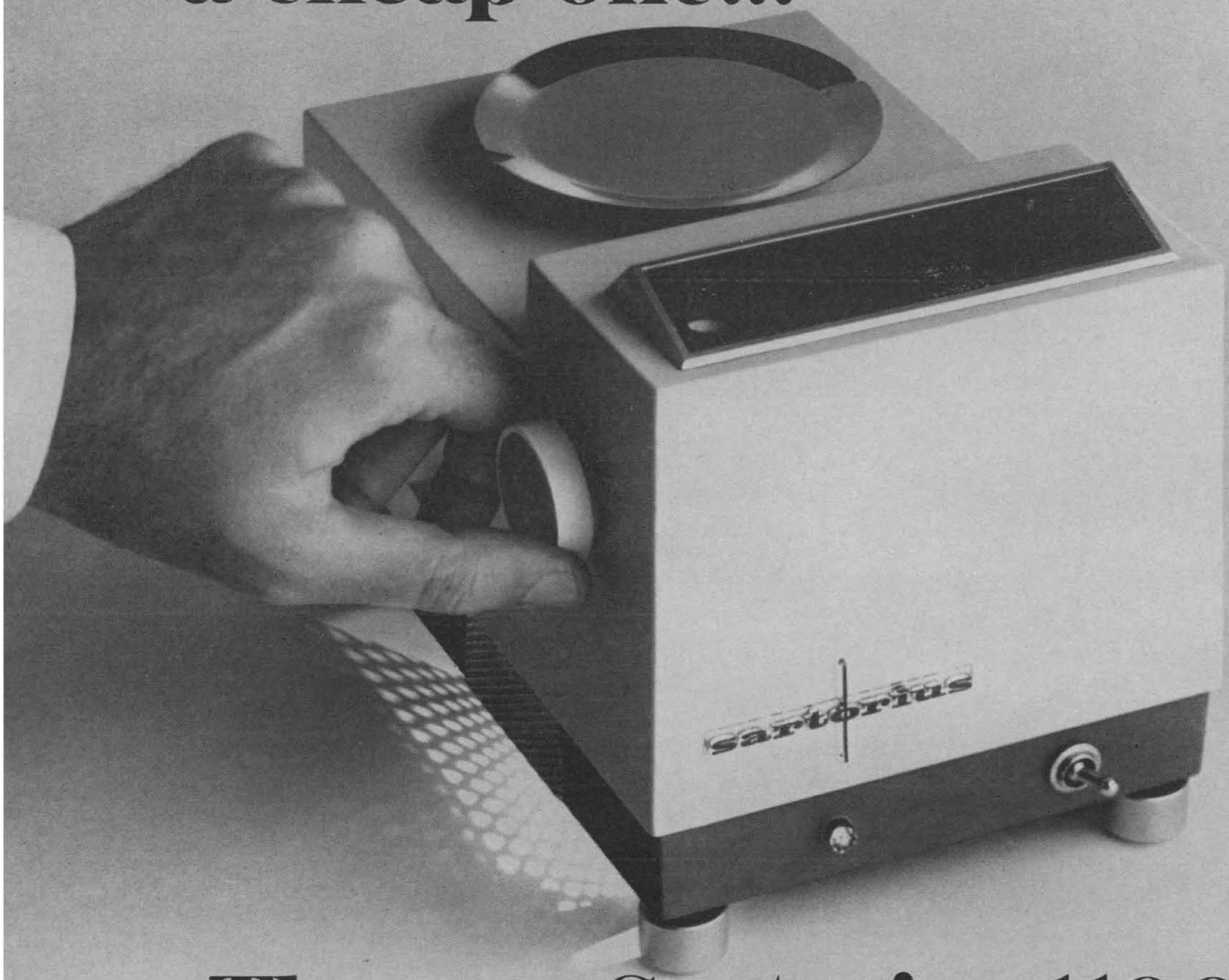

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Recalcitrant Honey Bees

The fine discussion by Gould *et al.* (*1*) is a worthy continuation of the series by Von Frisch (*2*) and Wenner *et al.* (*3*) which I have followed in *Science* with fascination. Gould points up the ever greater ambiguity of honey bee behavior, an ambiguity that may be unresolvable, considering that we scarcely understand rat behavior, much less our own.

The following experiment in the genre is unusually clear-cut, however. I placed a hive of honey bees (*Apis*) in the middle of a large open field (1 hectare, or 10 meters × 1000 meters), then walked to one end of the field and constructed a feeding station. No sooner (5 to 30 seconds) had I placed the feeding solution in the dish, than the entire swarm of bees descended and began to feed. They apparently had been waiting nearby in a tree. I was unable to repeat the experiment, because, when they finished, the bees left and have not been seen since (by me), no doubt being impatient of such pointless (to them) games, or afraid of being "discarded" (*1*, p. 547). This may or may not be true.

Several things are immediately clear. Bees always seem to know a little better than the experimenter just what they are about. They seem to think along somewhat the same lines that we do, but to a little better effect. And they pursue their ends doggedly, altering their response to fit the situation, being altogether humorless, at least under scrutiny.

My own experiments in this field have thus ended. I should advocate, however, that the funds for ABM and SST be diverted to other scholars interested in apiology, a pleasant occupation and a less harmful one (except to bees).

FRED G. HILL

14 Cottage Grove Avenue, South Burlington, Vermont 05401

References

1. J. L. Gould, M. Henerey, M. C. MacLeod, *Science* **169**, 544 (1970).
2. K. von Frisch, *ibid.* **158**, 1072 (1967).
3. A. M. Wenner, *ibid.* **155**, 847 (1967); D. L. Johnson, *ibid.*, p. 844; A. M. Wenner and D. L. Johnson, *ibid.* **158**, 1076 (1967); A. M. Wenner, P. H. Wells, D. L. Johnson, *ibid.* **164**, 84 (1969).

Brutus Struck Down

In his editorial "Science: Attack and defense" (14 Aug., p. 633), Thimann's logic slipped a little with his conclusion, "No, the only effective defense of science is through strengthening science itself." And by this he means, among other things, "our devotion to our research and teaching . . . and lively awareness of useful and humane applications." I doubt if one can teach science to alert college students today without extending the boundary of science to include, as an intrinsic part of each discipline, the value judgments concerning the place of one's subspecialty within the societal perspective. Useful and humane application is mainly what the nonscience community is asking for. Science shares the responsibility for the utilization of knowledge and its best defense is to attack the two-valued, but obsolete, split between science and society.

STANFORD C. ERICKSEN

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Look here, Professor Thimann, you and I are in our 60's—not far from death. We may die like animals, not knowing where we came from or where we are going. It cannot be too soon for us to demand that science and religion find out why we are here and where we are going. In your "only effective defense of science," you completely overlook the idea that science is the means and method for discovering's man destiny. Science uninvolved in a search and conquest of man's place in the universe is a science beheaded.

Science has failed to integrate with religion. Instead, theology, the authoritative type of thinking of Aristotle, is still the method of religion. Science is actually inseparable from religion; science should be the method, and religion the goal in the search for the meaning of life. But currently in the United States, science is mostly the basis for technological progress and industrial wealth. Nowhere is science basically the means for discovering the creativity of our universe. . . .

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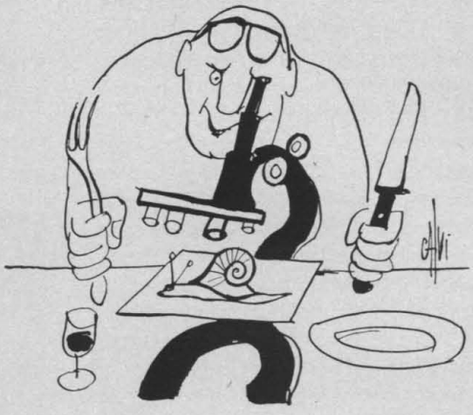
Thimann's fine message on behalf of science should be taken to heart by all who read it. His recommendation, however, that we emulate Brutus in disregarding threats is rather dangerous. Let us review what happened to "the noblest Roman of them all."

An impractical visionary of lofty

Tiens, une revue scientifique française.



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ideals and great courage, Brutus could not make a lasting impression on his listeners in the populace, who were easily swayed against him by the demagoguery of Mark Antony, a practical politician. So they turned furiously on Brutus, who was termed a traitor (Act III, Scene ii, 158 *et seq.*). He was overwhelmed in battle, and committed suicide by falling on his sword, a day after making the speech quoted by Thimann. Although "arm'd so strong in honesty," Brutus failed to persuade the public of the validity of his struggle against tyranny.

The play ends with Antony and Octavius shedding a crocodile tear on the corpse of Brutus, whose death they have encompassed. They then depart to consolidate their political victory.

THOMAS H. JUKES
Space Sciences Laboratory,
University of California,
Berkeley 94720

Thimann likens the upright scientist to Brutus, saying,

> There is no terror, Cassius, in your threats,
> For I am arm'd so strong in honesty,
> That they pass me by as the idle wind.

Thimann evidently is of those who believe Brutus was an honorable man. Let us all hope that scientists, including Thimann, realize that they can be and are being used by societies and groups for political ends. They, like all men, are political, and have opinions with "precisely the same worth as those of other citizens." The moment any person or group becomes so assured of his indubitable honesty, his objectivity, his claim to act in Truth that he forgets his responsibility for his creations and for their use, the time is ripe for him to do much worse than be "politicized," or retreat into a monastic cell—it would be time for him to follow in the steps of Brutus.

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I never intended to put up Brutus as a patron saint of scientists. Cassius was almost equally wise. However . . .

> . . . Brutus,
> Made I an error when I quoted you?
> 'Twas but for your fine words, not for your fate,
> For men have long enjoyed Will Shakespeare's words,
> So pithy and so true. But tell me, pray,
> What would you do if you could live again
> Those fearsome days? Would you your best ideals

Forswear, and let your critics have their way,
Till you demean your calling in vain hope
Of sharing theirs?

Brutus:
I would not if I could,
For I can strike a stronger blow for Man
By doing what I best have learned to do
And making it my life, than giving heed
To ev'ry thoughtless wight who may reproach
Me and my science, with false cries of shame
And guilt we feel not. Did not Cassius say,*
"Of your philosophy you make no use
If you give place to accidental evils"?

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* *Julius Caesar*, Act IV, Scene iii

France: Reducing the Ratio

According to a letter addressed by Prime Minister Jacques Chaban-Delmas to the French University Teachers Union (see *Le Monde*, 19 Sept.), there are this year, in the public higher education system, 38,102 teachers for 679,000 students. That makes a student-faculty ratio of about 18 to 1 and not the preposterous figure of 80 to 1 cited by D. S. Greenberg in "Academic finance . . ." (14 Aug., p. 658).

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Molecular Biology: Origin of the Term

In the September 1970 issue of *Physics Today*, in an article "The future of physics," Freeman J. Dyson, of the Institute for Advanced Study at Princeton, speaks of the disapproval on the part of his physicist friends of the fact that Sir Lawrence Bragg, the director of the Cavendish Laboratory, was interested in supporting activities in a field which, Dyson states, did not "even have a name when Bragg was appointed in 1938."

Dyson is here referring, as his article makes clear, to molecular biology. When I pointed out to Dyson that the term "molecular biology" had in fact been used in 1938, he wrote me that he had based his remark on the fact

that an article by E. L. Hess, "Origins of molecular biology" [*Science* **168**, 664 (1970)] "fails entirely to mention any use of the name earlier than that by Astbury in 1945"; and Dyson suggested that I "write a letter to the editor of *Science* to set the record straight."

My letter to Dyson contained the following paragraphs:

> Very shortly after I was appointed, in 1932, the director for the natural sciences in the Rockefeller Foundation I urged the Trustees, with the full backing of the then president of the Rockefeller Foundation, Max Mason, that the science program of the foundation be shifted from its previous preoccupation with the physical sciences, to an interest in stimulating and aiding the application, to basic biological problems, of the techniques, experimental procedures, and methods of analysis so effectively developed in the physical sciences.
>
> This proposal was accepted and approved by the Rockefeller Foundation Trustees, and progress in the program was sufficiently prompt and promising so that when I drafted the "natural science" section of the Annual Report of the Rockefeller Foundation for 1938 this section began with a sixteen-page portion, pages 203–219, which was headed, in large type, MOLECULAR BIOLOGY, the first sentence being "Among the studies to which the Foundation is giving support is a series in a relatively new field, which may be called molecular biology, in which delicate modern techniques are being used to investigate ever more minute details of certain life processes."
>
> Thus I think that it must be conceded that this field did have a name when Sir Lawrence was made the Director of the Cavendish in 1938.

WARREN WEAVER
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Yogurt Fit for an Armenian

I have read with shock the improprieties urged upon your readers by letter writers Segal and Goodman for the making of yogurt (31 July and 9 Oct.). In all generosity, I must admit that Evalyn Segal has genuine insights into portions of the process. She uses whole milk and she understands the application of yogurt to cucumber salad. But she recommends a dangerously imprecise method of measuring preincubation temperature of the boiled milk and goes on to say that the milk container should cool slowly wrapped in "two heavy Turkish towels." Jerome Goodman, on the other hand, writes that he uses "powdered skim milk" (may my sainted grandmother forgive my repeating such things on my own typewriter), and then prescribes incubation at 43°C using a "thermostatically controlled electric frying pan."

Such things cannot go uncorrected in a journal of science and humanism. Whole milk is used, of course. It is placed in a crockery bowl and mixed with the culture. But the proper temperature for the start of the incubation period is ascertained by dipping the elbow into the milk until one feels a sensation neither of hot nor of cold. Then the covered bowl is wrapped, first, in the second section of *The New York Times*, and, over that, a baby blanket made of virgin wool given in a layette 12 years earlier by an aunt in Watertown, Massachusetts. The coverings are kept in place by a flat granitic rock weighing approximately 8 pounds. The next morning you can expect yogurt fit for an Armenian.

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Livermore Lab Response

Boffey's article on the Gofman-Tamplin charges against this Laboratory (28 Aug., p. 838) quotes me only through the intermediary of Gofman. The quotations in the article are not in accordance with my memory of the conversations.

Thus, I did not tell Gofman that "it would be a 'grave mistake' for him and Tamplin to testify before Senator Muskie's subcommittee last fall." To the best of my recollection, I did not see Gofman between the time Senator Muskie called him to testify and the time he returned from the hearings.

I do not recall saying that the Laboratory was a "fragile institution," and that we couldn't "take a chance on injuring relationships between the lab and the commission." I thought, and believe I said, that the Laboratory's (or anyone else's) ability to get an objective hearing for controversial recommendations is "fragile," and depends on the willingness of many people to look dispassionately at views to which they have given support. In particular, I thought that coupling technical recommendations with attacks on individuals and institutions could only damage this ability.

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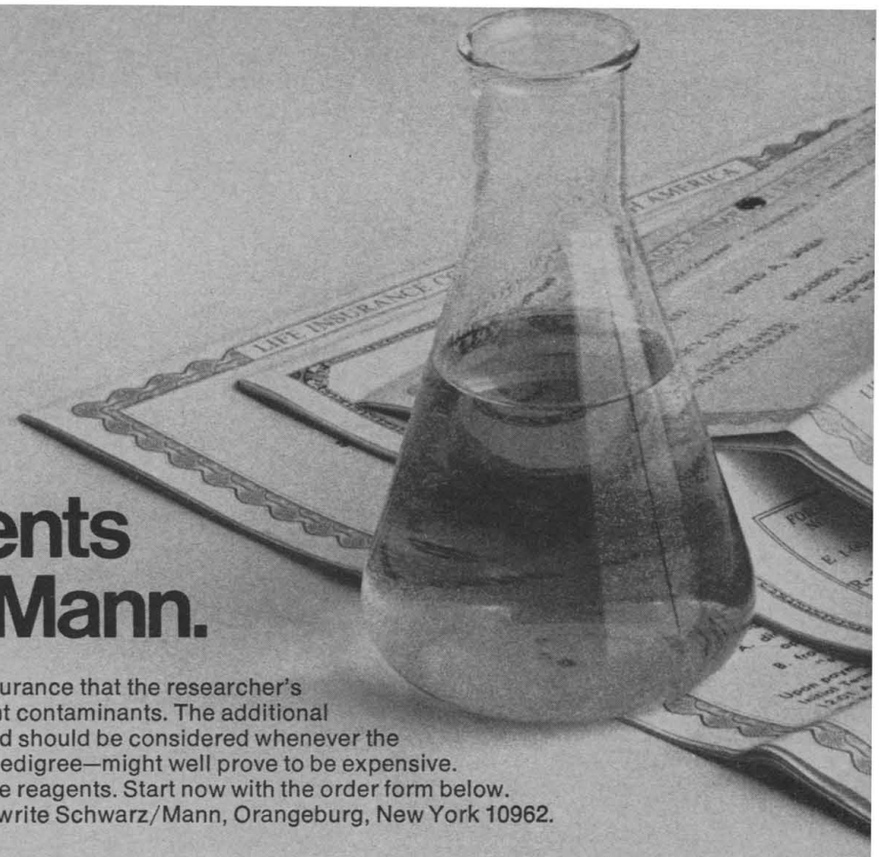

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Vietnamese Pacification Program

Orians and Pfeiffer strayed far from their defoliation brief in their letter of 11 September . . . There was never any "deliberate policy of the U.S. government to win the war by moving people from the countryside, which we cannot control, to the cities, which we can control." Quite the contrary, the aim was to extend security throughout the countryside, and it was to this aim that the so-called "90 percent relatively secure goal of 1969" referred. I should know, as I was chief U.S. adviser to the Vietnamese pacification program in 1967–68 when that goal was set.

Also untrue is the assertion of Haseltine, Carter, and Long (Letters, 3 July) that "the defoliation program is in large part purposefully directed against the rural population of South Vietnam." Not in my time it wasn't (though I had nothing to do with the program). Their quote from Huntington is lifted out of context to imply that it was—and still is. Whatever else one may say about the Vietnam tragedy, I object to assertions that the pacification program was in any way associated with destroying Vietnam's ecological balance or society. Its purpose—and impact—has been quite the reverse, as is now becoming clear.

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Nerve Gas: Judging the Risks

In his recent article ("Nerve gas disposal: How the AEC refused to take Army off hook," 25 Sept., p. 1296) Carter was being something less than objective. It is incredible, not that the AEC refused to take on the nerve gas disposal job, but that anyone ever seriously considered using a nuclear explosion for that purpose. The report of the Gross committee was speculative; Carter cited no experimental data or evidence to back up their claim that "the results would be completely predictable."

Carter ignores some obvious questions. Why is hauling a trainload of nerve gas coffins 2000 or so miles west to Nevada less of a hazard than hauling them 600 miles east to Sunny Point, North Carolina? Why is loading a ship in North Carolina more hazardous than threading coffins one-by-one down a 6-foot diameter shot hole at Yucca Flats, where an accidental explosion (before the shot hole could be closed) would spray a highly lethal geyser plume of nerve gas upwind of at least three-fourths of the people in the United States? Why is hydrolysis of nerve gas under 16,000 feet of salt water, the kinetics of which have been thoroughly studied, more hazardous than blowing it up in a nuclear explosion underground at one-tenth the depth, in an operation whose outcome is purely speculative? Carter has let his anti-Army bias hang out in clear view.

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Misdirected Attribution

On page 32 of the excellent *Short History of Botany in the United States*, edited by Joseph Ewan for the Eleventh International Botanical Congress, I find that I am erroneously credited with proposing the word "palynology" which is in general use to replace the older term "pollen analysis." Actually the proposal was made by H. A. Hyde and D. A. Williams, assisted by L. J. D. Richardson of University College, Cardiff, Wales. It was published 28 October 1944 in the informal mimeographed "Pollen Analysis Circular" (Oberlin College, 1943–53) which has since been absorbed by the journal *Micropaleontology*.

PAUL BIGELOW SEARS
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Coitus-Induced Ovulation

May I urge W. Jöchle (Letters, 21 Aug.) to publish all pertinent information on any case he may have of coitus-induced ovulation in women. If such evidence can be provided, we will have to completely revise our present concepts on the functional and timely interdependence of the pituitary-ovarian-endometrial physiology in women. Not a single documented case of induced ovulation in women has been published in the world literature during the past 100 years even though the literature is replete with case reports based on "old wives tales" (male and female).

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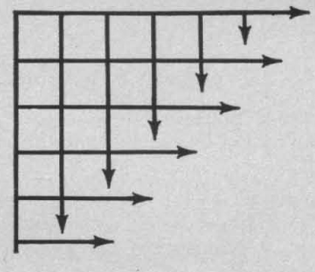

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6 November 1970, Volume 170, Number 3958

SCIENCE

Doctor of Arts Degree

In the last few years science has been under fire because it has not prepared to meet the changing demands of society. Higher education has been similarly challenged on many fronts, one of the points at issue being the narrowness of the traditional research-oriented Ph.D. program. This challenge has been intensified recently because of a sudden and rather dramatic tightening of the job market for Ph.D. graduates, especially in physics and chemistry.

Although this phenomenon is obviously related to the gloomy economic outlook, it stems from the academic world's resistance to change. But the monopoly of the standard Ph.D. may be starting to change.

Several years ago Carnegie-Mellon University established a program leading to the Doctor of Arts degree in the fields of history, English, art, music, and mathematics. It is not an attempt to alter the concept or value of the Ph.D., for the research orientation of the Ph.D. will, we believe, keep it always as the cornerstone of scientific progress. Rather, it supplements the traditional program with a new program designed to prepare people for certain teaching roles and to prepare them better. The first of these new degrees were awarded by Carnegie-Mellon University in 1968.

While acceptance of the new degree was relatively slow at first, recently there has been a sudden upsurge of interest. In 1969 the American Association of State Colleges and Universities and the Council of Graduate Schools in the United States formally endorsed the degree. In October 1970 these new programs were the subject of a national conference, which was sponsored by the Council of Graduate Schools in cooperation with Carnegie-Mellon University and attended by 80 invited participants.

Fifteen persons have received Doctor of Arts degrees from Carnegie-Mellon thus far, and approximately 100 are currently enrolled in the program. The program grew out of the university's deep involvement, over the past decade, in curriculum development and other innovative projects with personnel from secondary schools, junior colleges, and 4-year colleges.

The program is the university's considered answer to alternative graduate degree programs for training teachers and future leaders in secondary schools, junior colleges, and possibly some liberal arts colleges. The concept grew out of the Carnegie Education Center, which was established in 1966 by a grant of $1 million from the Carnegie Corporation.

Unlike the Ph.D. program, the new Doctor of Arts program essentially eliminates the requirement to discover new knowledge by "a piece of original research," but requires each candidate instead to develop classroom teaching materials as a thesis project. The candidate writes a rationale for his project, evolves teaching materials, devises appropriate teaching strategies, develops evaluating instruments, and tests his materials in class. This work is carried out in the major-subject department rather than in a department of education.

I believe that this new Doctor of Arts program is an important development, and hope it will gain broader support not only in the humanities, fine arts, and social sciences but in the physical sciences as well.

—H. GUYFORD STEVER, *President, Carnegie-Mellon University*

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