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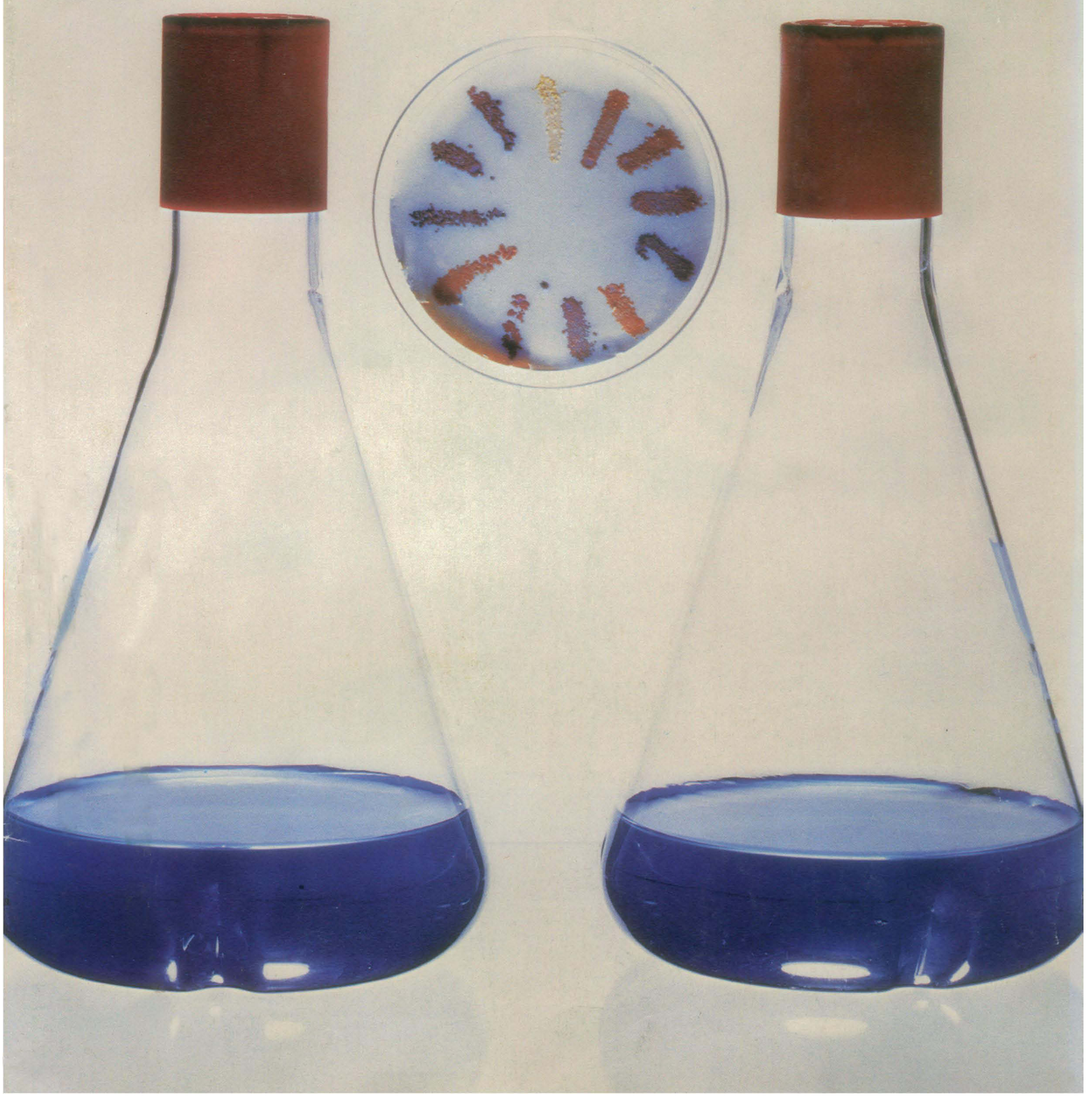

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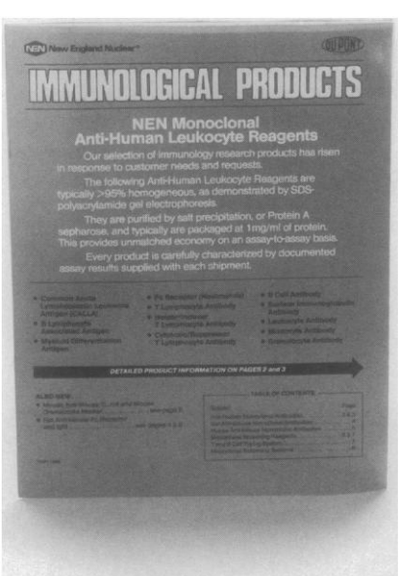

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COVER

Cloning of the naphthalene oxygenase genes from *Pseudomonas* into *Escherichia coli* results in the production of a number of pigments including indigo. The plate contains pigmented colonies produced by growth of the cells on solid medium. Unpigmented wild-type cells are at the top. Indigo is also produced in liquid medium and can be extracted from the cells with organic solvents. See page 167. [B. D. Ensley *et al.*, Amgen, Thousand Oaks, California 91320]

SCIENCE/SCOPE

A Very High Speed Integrated Circuit chip built for the U.S. military uses technology that makes it inherently hardened against radiation. The chip, produced after less than two years of development, draws on complementary metal oxide semiconductor/silicon on sapphire technology. It has circuit dimensions of 1.25 micrometers, or about 50 millionths of an inch. The VHSIC program is being conducted by the Department of Defense to develop chips that will give electronic systems a tenfold increase in signal processing capability. The high-speed, compact VHSIC chips will be more reliable and will require less power than integrated circuits now in use. Hughes Aircraft Company is the only contractor in the tri-service program pursuing CMOS/SOS technology.

Studies have begun to see how an advanced airborne surveillance radar might serve military forces late in this century. The radar would have a large phased-array antenna capable of generating many pencil-shaped beams and would complement the Airborne Warning and Control System (AWACS). One use of the new radar might be to listen in directions other than that of its transmitted beam. If it were to detect another active radar transmitter, the radar could turn its transmitter off (thus foiling an enemy's antiradiation missile) and do its surveillance by using the other radar's transmitted pulse. These concepts are being investigated by Hughes under several study contracts for the U.S. Air Force.

Among many innovations built into the new AMRAAM missile are a special safety mechanism and a high-power coaxial cable. The safety device will prevent the missile from exploding when subjected to fire, yet will not be activated by the high temperatures generated by burning fuel when the missile is launched. The new cable handles much more power than conventional cables and yet costs about one-tenth as much. Hughes designed and developed the Advanced Medium-Range Air-to-Air Missile for the U.S. Air Force and Navy.

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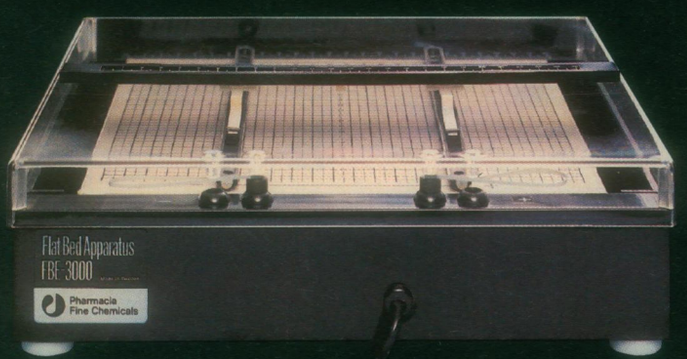

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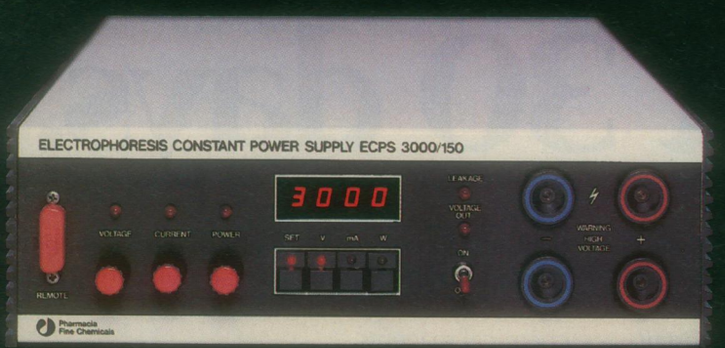

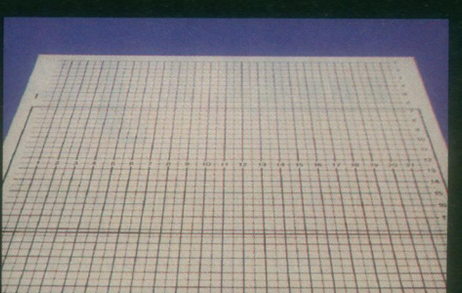

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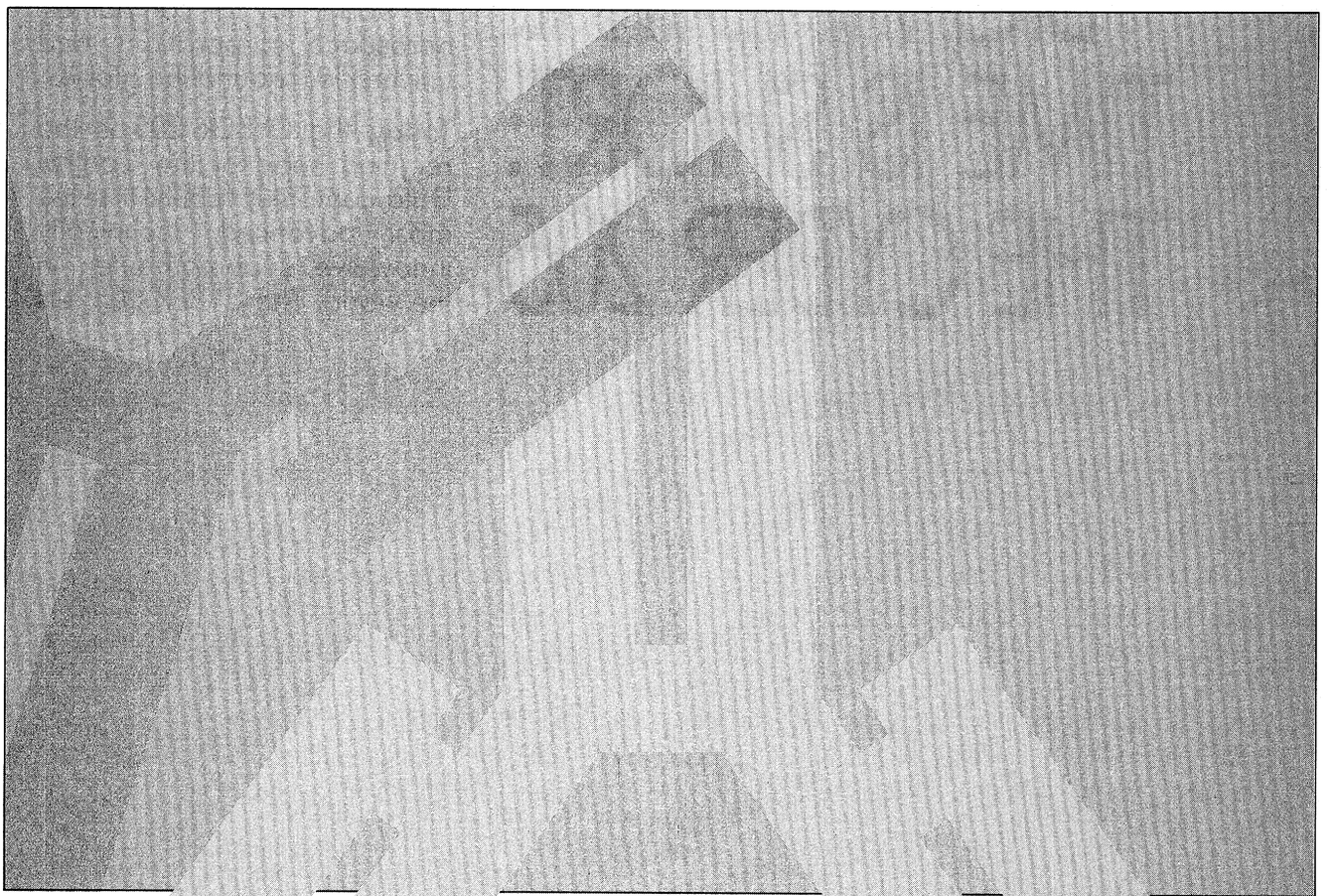

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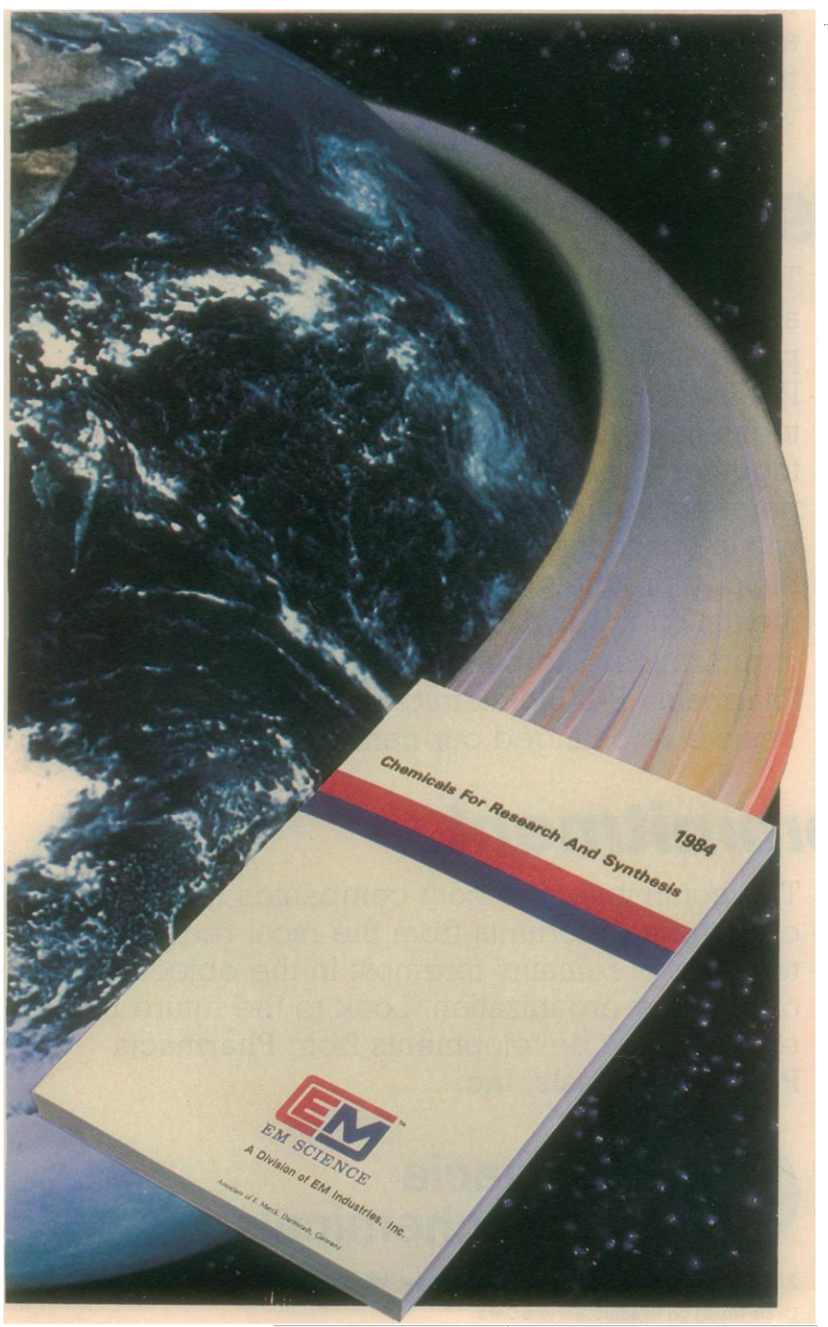

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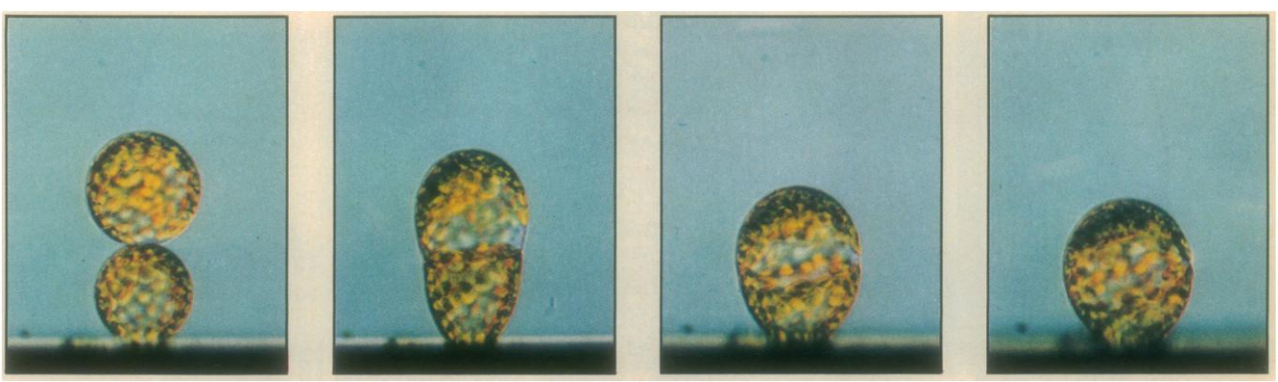

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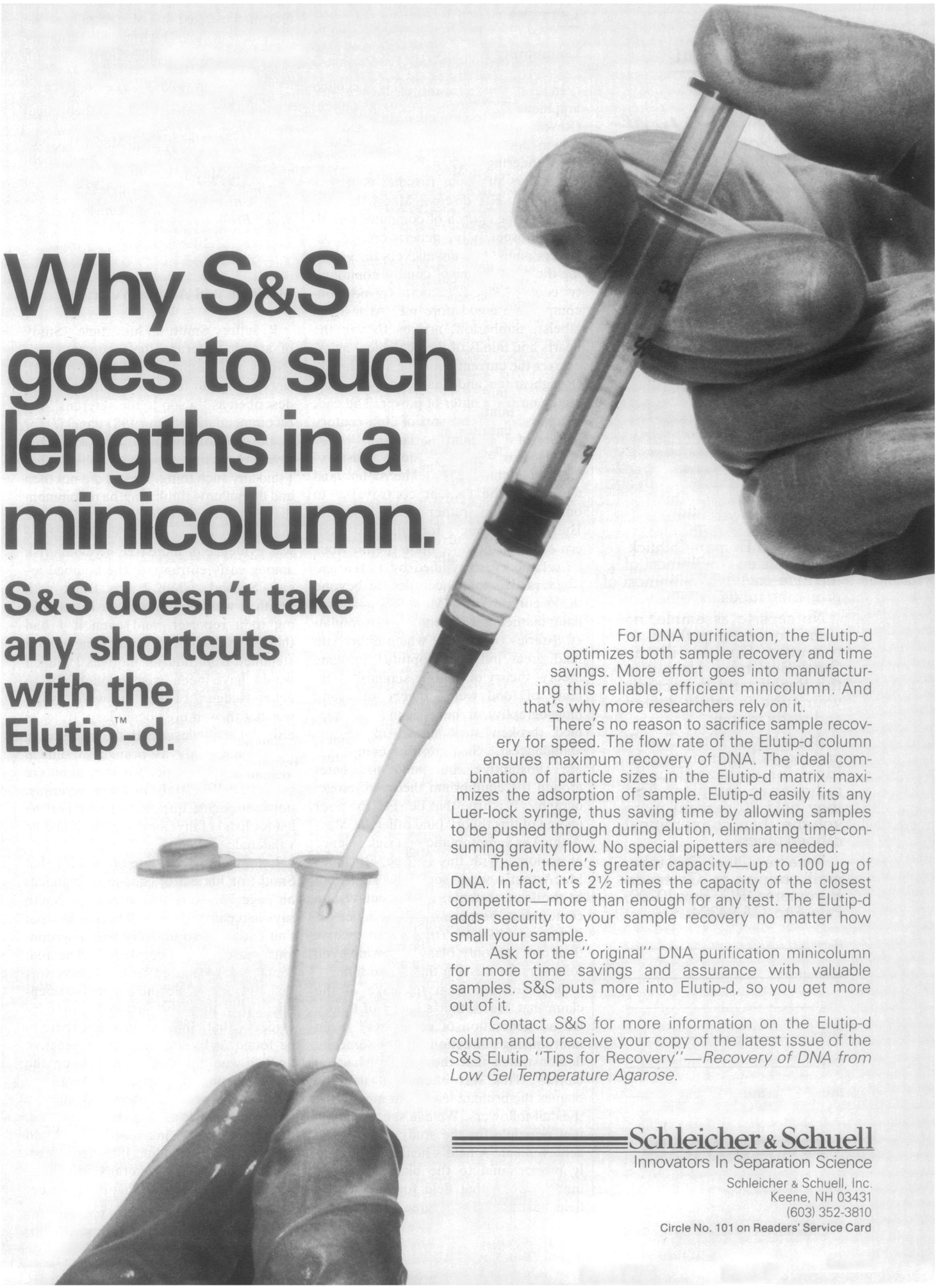

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LETTERS

Community Ecology

In a recent account of the current argument raging in community ecology (Research News, 12 Aug., p. 636), Roger Lewin portrays Evelyn Hutchinson as the wandering pilgrim who, upon tasting the waters of Santa Rosalia, is reborn spiritually. His disciple MacArthur establishes the church of community ecology. Subsequently, generations of believers punish the unbelievers for violating the first commandment of community ecology: thou shalt keep no noncompetitive god before me. At last, an atheist, Simberloff, arrives to win the hearts and minds of the ecological masses over the current church establishment (Roughgarden and Diamond) who now clutch onto the miter of power. The end, I suppose, will be a sort of 20th-century history of nullist totalitarianism.

This characterization does a disservice to Hutchinson and MacArthur and diminishes the current controversy to one of religion, rather than substance. Hutchinson brought formalism to modern ecology and built upon the previous era of theory so typified by Lotka and Volterra. His influence goes far beyond those corixid bugs. MacArthur—a brilliant mathematical ecologist—formulated a series of theories which either still hold great influence (optimal foraging theory, theory of limiting similarity, stability of food webs, theory of island biogeography) or have been since toppled (broken stick model of species abundances). Simberloff's complaints are substantive, but tend to center around the equilibrium theory of biogeography. It is true that he sees the poor testing of this theory (and others of MacArthur) as symptomatic of a sick science of ecology. With this I agree. But does MacArthur's work somehow stand out as the least testable, or is it merely on center stage because of its brilliance? If it was so obviously the wrong theory, then one can only blame the wide-eyed followers for missing this for so long.

One wonders what to make of the claim that MacArthur's brand of theory led a "generation of ecologists" in an unpromising direction until someone demonstrated that the emperor had no clothes. This sort of curious thinking blames the brilliant leader for misleading the dull followers. We can see an important principle for the study of scientific achievement. A field's health is inversely proportional to the blame given to innovators of that field for leading the field "astray." The degree to which we feel disillusioned by MacArthur is the very degree to which we have either shirked our duty or allied ourselves with a rather sick science. I am sure that ecologists are more to blame than MacArthur for the current state of theoretical community ecology.

JEFFREY S. LEVINTON
Ecology and Evolution Department, Division of Biological Sciences, State University of New York, Stony Brook 11794

Myeloma and Atomic Veterans

R. Jeffrey Smith, in his article "Study of atomic veterans fuels controversy" (News and Comment, 19 Aug., p. 733), says that in our telephone conversation I described as "a sop to the veterans" the recommendation of our 1981 panel (*1*) for a closer scrutiny of a list of alleged myeloma victims. It is most unlikely that I said any such thing. First, I did not then and do not now think that the recommendation stemmed from any other motive than the wish to see whether or not there was evidence of increased myeloma risk among early entrants to the bombed areas. Second, while I was frank with Smith, I was also aware that I was talking to a reporter, and even if I had thought that the recommendation stemmed from ulterior motives I think I would have been wise enough not to acknowledge it. Third, the word "sop" was not then in my vocabulary. If, as he also states, I described the motivation behind the study as primarily political rather than scientific, I had in mind the broad issue of study of these veterans, not the specific question of whether the list (or lists) of myeloma victims could be validated.

Let me clarify the line of thinking that Smith, or his editor, chose to highlight on page 734. I did not argue, as Smith says in a paraphrase of my remarks, that "an excess is so unlikely that a scrupulous search is unnecessary." The reasoning which I tried to get across was that any large-scale and expensive scientific study must be justified either by evidence that there is something there to be found or by the fact that a negative finding would be of value. Since the overwhelming consensus is that one would not expect an observable increase in myeloma risk among these veterans, the finding of no increased risk would have no scientific value. If the lists informally collected by veterans' organizations led one to believe that there was an excess—despite what one expected—a

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full-scale epidemiologic study would be justified. Since a closer perusal of these lists does not support this belief (*2*), the full-scale study is not justified. Our 1981 report specifically states that "This [the investigation of multiple myeloma in these veterans] should not *at first* involve a full-scale epidemiologic study" (emphasis added). The implication is clear that the full-scale study might follow if evidence of increased risk were found.

In my opinion, the 1983 multiple myeloma panel carried out fully the recommendation of the 1981 panel.

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References

1. "Panel on Feasibility and Desirability of Performing Epidemiological Studies on U.S. Veterans of Hiroshima and Nagasaki, Final Report to Director, Defense Nuclear Agency" (National Research Council, Washington, D.C., 1981).
2. "Panel on Multiple Myeloma among Hiroshima/Nagasaki Veterans. Final Report" (National Research Council, Washington, D.C., 1983).

R. Jeffrey Smith writes about a recent National Research Council (NRC) report assessing the evidence for allegations concerning an unusually high incidence of multiple myeloma in U.S. veterans who participated in the occupation of Hiroshima and Nagasaki.

The article does not distinguish between the motivation for directing a question to the NRC and the manner in which the NRC addresses the questions it accepts. Federal agencies bring questions to the NRC for a variety of reasons, sometimes because they need an authoritative answer to a scientific question that lies at the heart of a social or political issue. It is the NRC's function to find in the question those elements where scientific analysis and judgment of the facts by a balanced committee will assist the agency and the nation in determining their policies. Contrary to the implication in the article that the NRC mixed politics with science, the authoring committee in its report dealt only with the scientific issues.

The comments by Seymour Jablon and Brian MacMahon should be understood in the context of the distinction I have drawn above.

A second important point is that the study was *not* intended to be primarily an epidemiologic investigation. Its purpose, to paraphrase the contractual charge, was to examine allegations that there is an unusually high incidence of multiple myeloma among veterans of the Hiroshima and Nagasaki occupation forces and to present what is currently known about the incidence rate of multiple myeloma in similar populations. That is exactly what the NRC committee did, and its report does not portray the study as a full-scale epidemiologic effort. When one considers the charge to the committee, the methods used to identify and evaluate all possible claimants were appropriate. Except for the official from the National Association of Atomic Veterans, the persons quoted in Smith's article appear to agree with our committee's basic findings, and we believe that the NRC has successfully answered the scientific question that it was asked to address. Unfortunately, the social and political concerns survive.

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Control of Ph.D. Programs

Dennis Doverspike (Letters, 5 Aug., p. 506) comments on my remarks (Letters, 24 June, p. 1336) and raises to the most general level the issues posed by the differences between Steven W. Mosher and the Stanford anthropology department (News and Comment, 13 May, p. 692). I would like to clarify a few issues raised in my *Society* article (July/August 1983, pp. 4–15), from whence my letter derived, and to place in sharper relief the issue of who should control Ph.D. programs.

First, no one is seriously asserting "that only scholarship and not behavior should determine who is awarded a Ph.D." Rather, the notion of misbehavior is so broad that for it to be employed as grounds for dismissal requires the public documentation of such presumed personal forms of misconduct. To do otherwise is to return graduate studies to a darker age of not so long ago, when behavior was linked to conformity and even to denigration rituals as "proof" of scholarly worth.

Second, professional responsibility is precisely what caused candidate Mosher such grave anguish. Responsibility to women who were victimized by unwanted eighth- and ninth-month abortions and responsibility to parents faced with official criticisms of those who had more than one or two children and were encouraged to engage in unofficially sanctioned acts of infanticide. It is precisely this ambiguity in the notion of professional responsibility—to anthropology as a science of discovery or to a discipline with a narrow code of conduct—that led

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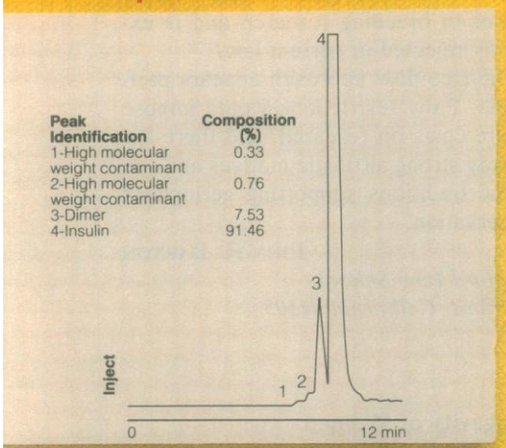

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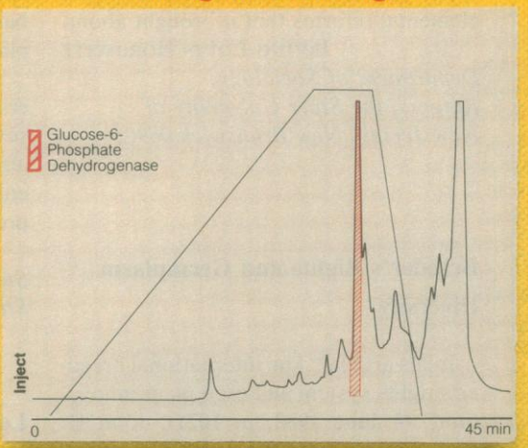

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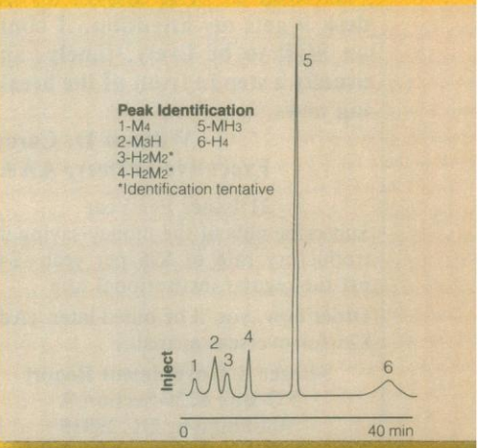

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the issues in the Mosher case to become entwined with the concerns expressed by Doverspike.

Third, not only a faculty has the "right to decide who may study." The history of 20th-century academic life is partially at least a history of the democratization of that life. And democracy means precisely the adjustment and adjudication of the control of graduate programming and decision-making. This implies a role for the administration, the media, the courts, and, yes indeed, the relevant graduate students. Surely we have not come so far in the counterrevolution against the counterculture as to deny the elemental reforms that it brought about.

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Breeder's Rights and Germplasm Diversity

In his article "The international breeder's rights system and crop plant innovation" (4 June 1982, p. 1071), John H. Barton states that "there should be international legal requirements prohibiting any restrictions on the export of germplasm (but allowing import quarantines), encouraging or requiring the collection of native materials as part of the process of spreading new varieties, and laying down much stronger requirements for placing material into collections as part of any patent process." This is desirable, he argues, for purposes of the maintenance of germplasm diversity. However, one adverse effect of such a requirement would be to deny the country the possibility of benefiting commercially from the presence of unique genetic resources within its jurisdictional boundaries. In such a case the country has little incentive, other than altruism, to incur costs to protect unique species. Thus commercial pressures for deforestation and other activities that may result in the destruction of species are not balanced by incentives for protection resulting from the possibility of capturable commercial gains that might accrue to the country at some future date. Perhaps allowing for patents and exclusive rights to unique species is one way to effectively promote their preservation.

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Sedjo is quite correct that breeder's rights laws and flow restrictions on international germplasm could create a material incentive for nations to preserve unique species. Under current or likely laws, however, this effect is only indirect. Existing breeder's rights laws exclude natural species; the sensed need for novelty along with practical legal requirements for uniformity and identifiability make it hard to extend the law to cover such species. Conceivably the holder of a protected variety might be required to pay royalties to donors of major genetic sources of that variety; such an approach would create a major barrier to breeding research and is explicitly rejected in current law.

The free flow approach appears preferable. Flow restrictions would impose severe costs on research, and there are already strong altruistic motives and scientific traditions supporting germplasm preservation.

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Longevity of Women

Apropos Constance Holden's article "Can smoking explain ultimate gender gap?" (News and Comment, 9 Sept., p. 1034), it is easy to believe that traditionally heavier smoking by men contributes greatly to the relatively greater longevity of women. But heavier drinking, too, must be considered.

Next in line of culprits, I propose the after-dinner nap. Think of the good cooks now enjoying sprightly later years who traditionally jumped up from the dinner table and spent up to 2 hours scooting around the kitchen cleaning up while hubby hoisted himself to his feet, lumbered into the living room, sank into his easy chair with his newspaper, or stretched out on the couch for an hour of shut-eye. No wonder Kannel finds that "women maintain the advantage" as regards cardiovascular mortality.

This, too, shall change! As women's smoking and drinking habits come to more closely resemble those of males, even the good cook will be piling the dishes hastily into the automatic dishwasher and dashing into the living room to join hubby in front of the television set, while the cholesterol accumulates equally in the sedentary vessels.

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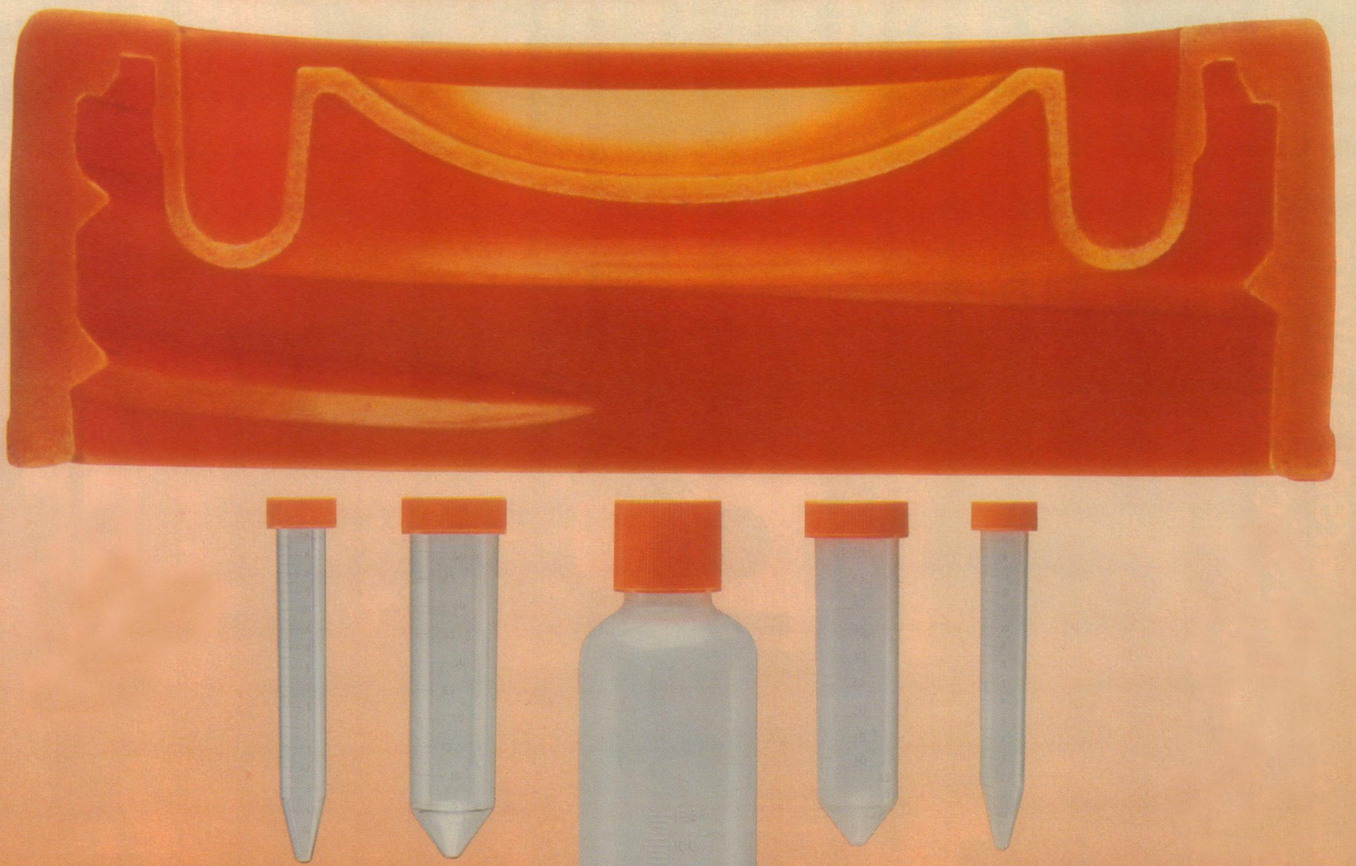

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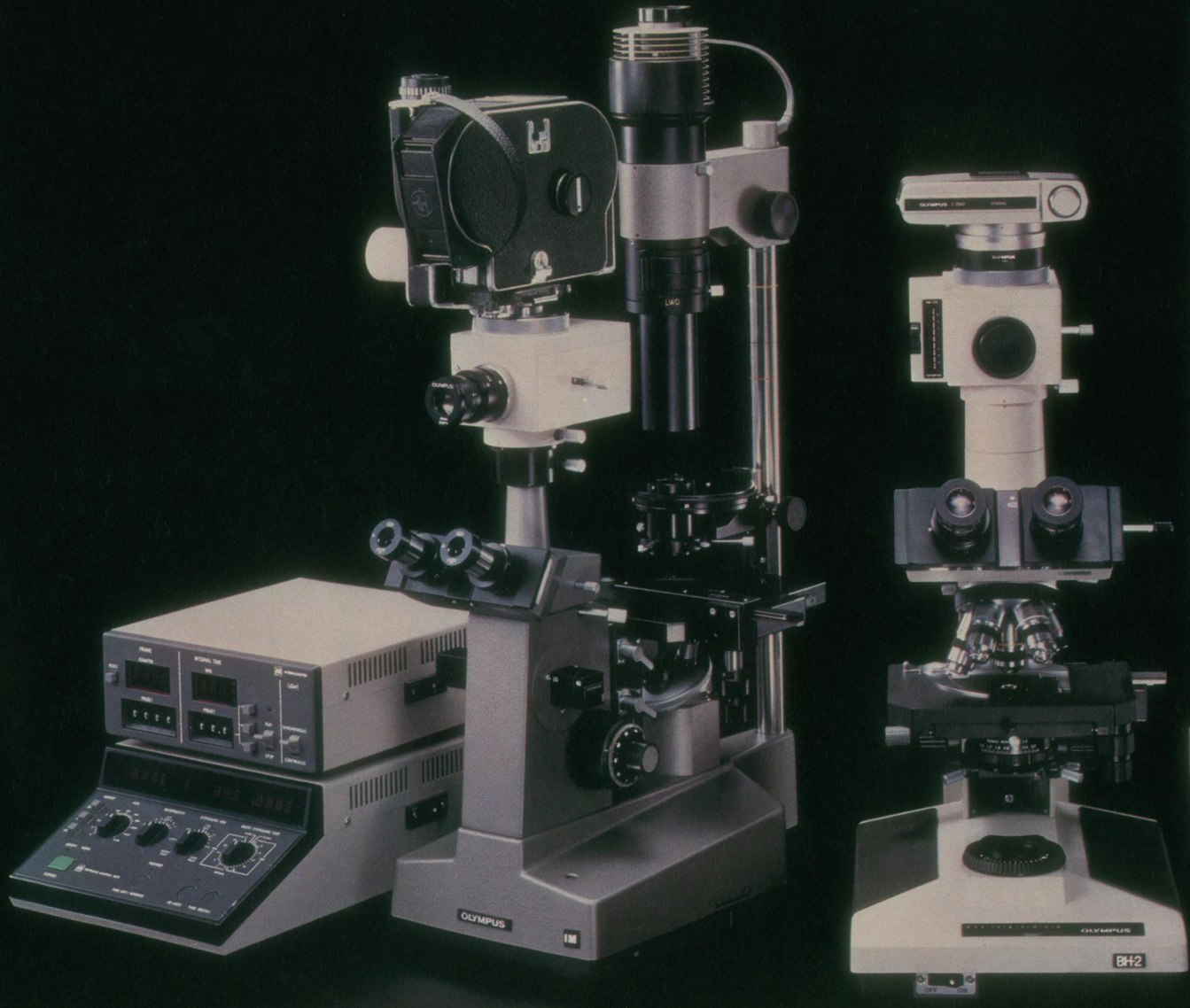

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14 October 1983, Volume 222, Number 4620 **SCIENCE**

Workers at Risk

The presence of hazardous chemicals in the workplace is having profound human costs in terms of anxiety as well as illness. Workers' concerns are likely to affect collective bargaining, compensation claims, and the morale of the work force. Yet risk analyses seldom address the perspectives of those exposed to hazards. The following observations are based on interviews with workers in various occupations who are routinely exposed to hazardous chemicals in their jobs.*

The diversity in perceptions was striking. Some workers saw risks as dangers, others as part of the job; some were resigned to hazardous conditions, others sought change. Their social relationships, attitudes about work, choices, and, above all, the extent of their control over working conditions shaped perceptions and guided responses to risk.

Many workers conveyed a sense of isolation, fostered by persistent anxiety, reluctance to talk about health, and fear that complaining could jeopardize their jobs. The use of protective equipment compounded the feeling of isolation; respirators insulated people from communication as well as hazards. Safety policies that required rotation of workers in hazardous jobs broke up work groups, inhibiting discussion of common problems. Those who had little interaction with co-workers believed their problems were unique. Embarrassment about problems such as sterility, cancer, and nervous disorders made workers reluctant to talk. Those who felt isolated dismissed problems as personal, denying the possibility of risk.

Perceptions of risk also reflected attitudes about work. Those who enjoyed their work and valued its results tended to minimize the significance of risks. The contrast between professional and production workers was sharp. Workers talked about risks in the context of job alternatives. Those with family obligations were unwilling to speak out about conditions in the workplace. Afraid that they would be labeled troublemakers, they lapsed into an attitude of resigned compliance. However, they felt they were forced to choose their job over their health. Workers who actively tried to change working conditions were those with fewer economic constraints, greater opportunities, or a union that provided protection.

The workers we interviewed were preoccupied with questions of control. They expressed a sense of powerlessness in the face of uncertainties about exposure and long-term effects on health. Contributing to their sense of impotence was the technical complexity of information about risk and their inability to use what information they received. Concern about control also reflected their lack of confidence in management efforts to minimize hazards. Many factory workers believed that production and profits were given priority over protection of health; many laboratory technicians felt that research was given priority over people. Workers complained that managers poorly understood the realities on the shop floor, yet discounted the validity of direct experience. Those in a position to exercise judgment about their working conditions worried less about risk.

What does this imply for recent policy proposals? Supplementary wages have been proposed for those who must take risks. Some workers we interviewed were willing to accept hazard pay, but suggested that this would only add to the burden of choosing between work and health. Science panels have been proposed to evaluate risk. While scientific assessment could enhance regulatory decisions, our interviews suggested that workers are not likely to accept risks solely on the basis of expert risk-benefit calculations. They believe that risks cannot be objectively measured and balanced, that personal dangers must be avoided at any cost. They want a greater voice in decisions that may affect their health. Like others concerned about the impact of technology, they seek to participate in the politics of technical decisions.—DOROTHY NELKIN, *Professor, Cornell University, and Visiting Scholar, Russell Sage Foundation, 112 East 64 Street, New York 10021*

*D. Nelkin and M. Brown, *Workers at Risk* (Univ. of Chicago Press, Chicago, in press).

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