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AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

Proceedings Issue

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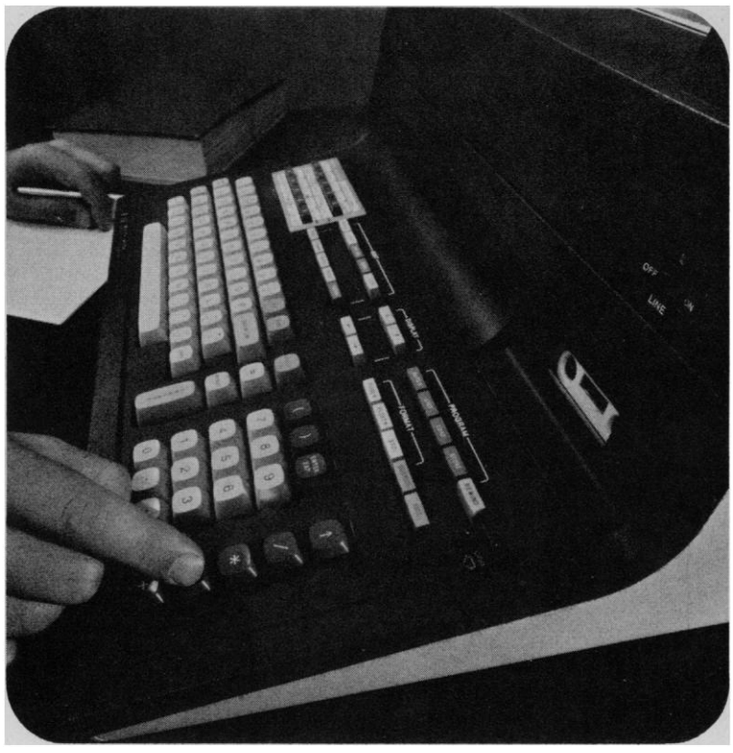

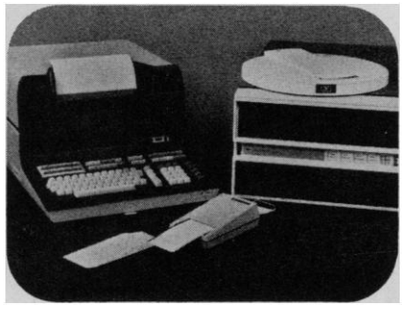

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AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

The American Association for the Advancement of Science was founded in 1848 and incorporated in 1874. Its objects are to further the work of scientists, to facilitate cooperation among them, to improve the effectiveness of science in the promotion of human welfare, and to increase public understanding and appreciation of the importance and promise of the methods of science in human progress.

COVER

Low frontal region and high zygomatic arches of the opossum (*Didelphis marsupialis*) produce wide visual-field overlap without any construction of the interorbital region. See page 436. [W. L. Hylander, Department of Anatomy, Duke University, Durham, North Carolina]

A Scientific Idealist

Edward Uhler Condon, lover of peace, science, and people, nemesis of UFO's, stuffed shirts, the House Un-American Activities Committee, and bureaucracy, died on 26 March 1974.

He was fond of pointing out that the sign at the city limits of Alamogordo, New Mexico, proclaiming it "Home of the Atomic Bomb," should have read, "Home of the Atomic Bomb and of Edward U. Condon." The Trinity explosion made Alamogordo a monument to man's fear of his own intelligence—ironically at the birthplace of a man completely devoted to peace, individualism, faith in humanity, rationalism, and freedom of thought. Thus passes one of the few remaining scientific idealists, who persisted in rejecting cynicism in the face of personal experiences of abuse from those who feared truth or controversy.

He will be remembered for his extraordinary contributions to physics, particularly in the early application of quantum theory to the physics of atoms, molecules, and the solid state. He was honored in his time as president of the AAAS and of the American Physical Society. He directed research at the National Bureau of Standards, Westinghouse Electric Corporation, and Corning Glass Works. He lent his name to countless good causes. And he was misunderstood. To set the record straight, let the following be remembered.

Equally at home in industry, government, and university, Condon even undertook a campaign to improve the military service academies. He was, above all, a rational humanist who did not make the sharp distinctions others draw between basic science and industrial science, who did not believe you could measure a man's morals by asking where he worked. He made moral judgments, to be sure, and many felt the ire of his tongue or pen. But he was never underhanded or devious.

UFO buffs accused him of prejudice, of conducting his investigation with a closed mind. But Condon confided to his friends that the attraction of the project lay in the possibility—however small—that a real discovery might be there for him to make. He believed that those too easily convinced might be blinded to the shreds of real evidence about new phenomena. The mystics who challenge science do not understand

that skepticism is not intolerance—it is the discipline of an open mind.

Those who first accused him of potential disloyalty went to prison. Condon was exonerated of "security risk" charges on every appeal save one, when a political decision overturned a recommendation favorable to him. During his tenure as a Fellow of the Joint Institute for Laboratory Astrophysics, he received again a Secret clearance which permitted him to advise a government he honored as the servant, not master, of the people. He served his country loyally and well with his mind, his leadership, and his criticism, and the record proves it.

LEWIS M. BRANSCOMB
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Schizophrenia Exchange Program

The report by Deborah Shapley (News and Comment, 8 Mar., p. 932) on the U.S.–U.S.S.R. schizophrenia research exchange program raises some serious questions about the attitudes of the Americans involved.

The American participants are concerned about the alleged misuse of psychiatry in the U.S.S.R., and especially about the possible misdiagnosis of political dissidents. Szasz (*1*) has noted that psychiatric justice is not altogether just in the United States and that it is often used to deny citizens their constitutional right to trial. We might also recall that, in October 1964, the magazine *Fact* published a poll of 12,356 psychiatrists registered with the American Medical Association about the fitness of Senator Goldwater to run for the presidency and 2417 responded; 1189 thought that Senator Goldwater was not psychologically fit to run, 657 felt that he was fit, and 571 decided that they did not have enough information to judge (*2*). Neither those who judged Goldwater fit nor those who judged him unfit had enough information on which to base a judgment. Thus, the concerns of the Americans participating in the exchange program hardly justify ending the exchange any more than the alleged misuse of psychiatry in the United States should justify the Russians' pulling out of the program.

The differences between the criteria for diagnosing schizophrenia in the U.S.S.R. and those used in the United

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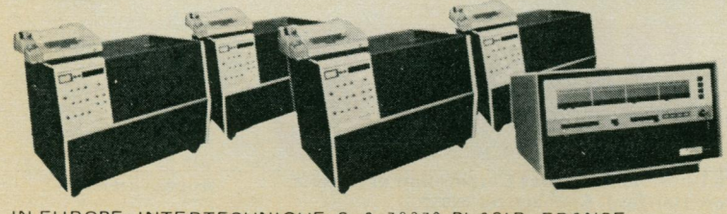

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States are reported to be causing the American participants some concern. However, differences between U.S. and British diagnostic criteria have been thought to be of sufficient interest to merit a cross-cultural study (*3*).

The American participants fear that certain Russian dissidents have been misdiagnosed. It is interesting that the American psychiatrists have not had an opportunity to interview and examine these dissidents. They seem, therefore, as ready to diagnose Russian citizens without data as psychiatrists were to diagnose Senator Goldwater in 1964 without data. Has psychiatry advanced so little in 10 years?

A final reason the American participants give for possible withdrawal is that they think they may learn little from the exchange. This raises the question of why the exchange was initiated. Its purpose was surely more political than educational. I doubt that its aim was to provide immediate data to American psychiatrists on fast-breaking research developments in schizophrenia research in the U.S.S.R.

Luckily, European psychologists such as Lynn (*4*) continue to review Russian research on schizophrenia, so the results of that research are being disseminated to interested Americans.

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Equal Opportunity in Biomedical Research

The major reason given by the Nixon Administration for abolishing NIH (National Institutes of Health) predoctoral fellowships and training grants was that we already have sufficient numbers of trained investigators. This is certainly not true with respect to trained biomedical researchers from minority groups such as blacks, Mexican-Americans, and American Indians. The present policies of the NIH will actually result in a reduction in the numbers of such individuals because access to training for careers in research will primarily be limited to individuals with affluent par-

ents who can afford the expense of college and graduate school. The percentage of minority applicants to graduate schools whose parents can afford to finance graduate education is far less than that of white applicants. While there are some loan programs, they are particularly unattractive to members of minority groups from impoverished backgrounds. The more attractive financial prospects of a career in medicine mean that if the same debts must be incurred in training for a career as a physician or as a biomedical researcher, the former will more often be the preferred choice.

Currently the major support for access to research careers by minority students in graduate school is being provided by private foundations. We find it hard to understand why the Department of Health, Education, and Welfare is doing so little to support graduate training of minority applicants when everyone agrees that there is a shortage of such individuals.

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Labor-Intensive Production

In his editorial "Corporations and the less developed countries" (30 Nov. 1973, p. 873), Philip H. Abelson mentions an IBM typewriter plant in Bogotá, Columbia, that relies on labor-intensive production techniques as an example of increased corporate responsiveness to the desires of the host countries. I suggest that an additional, more telling, incentive is at play—corporate self-interest.

This conclusion comes from my observation of a similar project under development in Bombay, India—an industrial estate near the international airport that will deal exclusively with the assembly of electronic equipment. Sponsored by the semigovernmental Trade Development Authority, the estate will house predominantly non-Indian concerns. It is to be a free trade zone, devoted exclusively to export production. Components will be flown in and assembled at the estate, and the finished product will be flown out again. The production process is highly labor-intensive. The value added in India will amount to more than 50 percent of the product's final cost. The project will provide employment for some 45,000 people.

In this case, labor-intensive techniques are being developed, not because of any benevolent feelings on the part of employers, but because labor-intensive techniques are profitable. Because the wage rates in India are low, it is more profitable to have the electronic equipment assembled by hand in India than to use a capital-intensive (or labor-intensive) technology in a developed country. The companies get a cheaper product; India gets the employment.

Such mutual benefit is likely to provide a more reliable and significant binding cement between the multinational corporations and the less developed countries than is corporate benevolence.

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Virus Research

The provocative report "Microbiology: Hazardous profession faces new uncertainties" by Nicholas Wade (News and Comment, 9 Nov. 1973, p. 566) raises important questions, all of which deserve consideration and discussion. In general, a distinction should be made between the primary hazards to which the scientist is exposed and the potential of secondary hazards to the public at large. Most discussions of the latter tend to ignore the biologic constraints by which infectious disease patterns are stabilized by a kind of "environmental homeostasis." Sulkin and Pike's extensive reviews of laboratory-acquired infection (*1*) fail to document the secondary spread of agents initially alien to man (for example, louping ill and Newcastle disease viruses) or intrinsically pathogenic for man, but acquired by an unnatural route in the absence of the natural vector (for example, Venezuelan equine encephalitis virus).

Wade attributes to those in "virologic circles" a concern that "the ability to genetically manipulate flu viruses could lead to a new combination that might escape from the laboratory, by infecting an employee, say, and spread to the population at large." He then quotes Wallace Rowe of the National Institute of Allergy and Infectious Diseases as saying, "This could recreate the conditions for an influenza pandemic like that of 1918." Rowe voices

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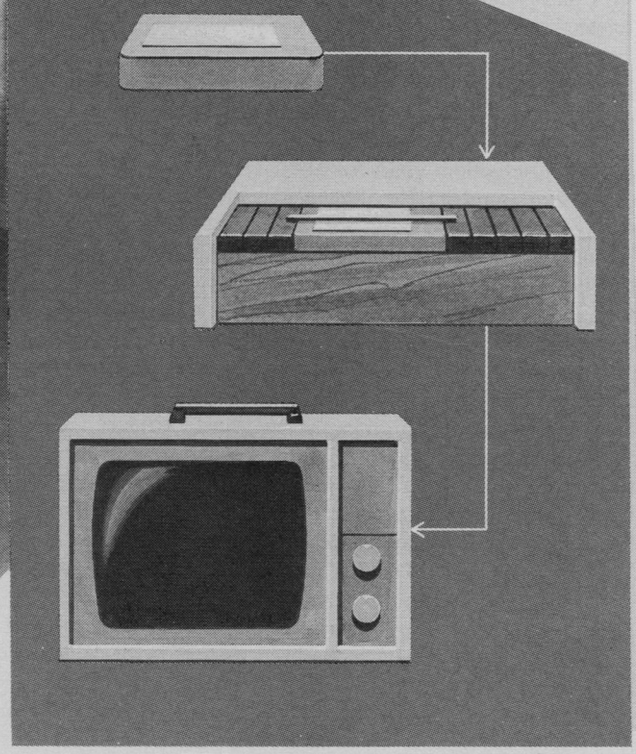

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a concern that I have also heard expressed by others. While there is a growing acceptance of my suggestion (*2*) of the possible role of genetic recombination of influenza viruses in nature in the genesis of pandemic influenza, re-creation of "the conditions for an influenza pandemic like that of 1918," would require not only a new virus but a world war and the subtraction of a half century of progress in the health sciences, including the elimination of most antibacterial drugs in present use. (Most deaths in 1918 were the result of secondary bacterial pneumonia.) What about the possibility of a pandemic, not like that of 1918, but like those of 1957 or 1968? How can possibility be refuted in biologic science? Of course it is possible that hybrid viruses of laboratory origin may "escape" despite past evidence to the contrary. However, do we then cease our "genetic manipulations" of the viruses at a time when we are just beginning to appreciate their implications? Is it generally appreciated that contemporary influenza vaccines are made from recombinant or hybrid viruses deliberately designed in the laboratory for optimal production characteristics (a prime requirement for vaccines which have to be redesigned every few years to match mutations of the virus in nature)? More important, the segregation of influenza virus genes by genetic recombination (reassortment) has, in recent years, accelerated our understanding of viral replication, the biologic function of the viral proteins, and the nature of antigenic variation. Is the abandonment of this method of genetic analysis being implicitly proposed?

The question is not "Could it happen?" but rather "Is it likely to happen?" If it does, does the risk exceed the real and present danger that is constantly posed by mutation or recombination in nature of wild type viruses?

A pandemic of influenza apparently requires: (i) the appearance of an influenza A virus, the hemagglutinin antigen of which differs markedly from that of the preceding virus; (ii) a population that has no antibody to the new virus but high levels to the old one; and (iii) probably as a consequence of (ii), the disappearance of the old virus to provide an ecologic niche for the transmission and circulation of the new strain.

How is an antigenically novel virus produced? Certainly not by genetic recombination, which adds nothing new, but rather reassorts the old (that is, the antigens of the input parental viruses).

Certain of these "old" antigens now present in animal influenza viruses have not yet seen the human host; their recombination with human viruses could create hybrids endowed with genes necessary for their replication in man. Clearly, such viruses, one of which I am guilty of "creating" (*2*), should not be considered as candidates for live virus vaccines in man. It is not likely that they will "escape from the laboratory, by infecting an employee." Laboratory-acquired infection by an influenza virus is a rarity even with wild type strains recently isolated from man. Almost all strains of influenza virus, upon their isolation in alien laboratory hosts (principally the chick embryo). lose their virulence for man. Indeed, a problem with experimental live-virus vaccines is the maintenance of sufficient human virulence to allow infection to occur. Furthermore, secondary spread from vaccinated subjects seldom has been observed. To add to this, the indolent progress of a wild type virus in the early stages of a pandemic has been frequently observed and suggests the need for a concatenation of factors, including optimal population density, environment, and season for a successful pathogen to emerge. Finally, because influenza virus virulence is clearly polygenic, the crossing of domesticated viruses will usually lead to the production of progeny of intermediate virulence (that is, less virulent than the most virulent parent) as "virulence genes" are redistributed. I submit that all present viral vaccines have been obtained by "genetic manipulations"—largely empirical.

It is time to capitalize on the legacy of modern molecular biology in the deliberate design and choice of the viruses with which we shall live and which shall defend us. Certainly, as his power in the laboratory increases, the biological scientist must couple his enthusiasm with sobriety and caution. His colleagues and critics owe him similar restraint in our present climate of research pragmatism.

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International Science Education

The people of the United States have long wanted to be helpful in assisting the developing countries. Most of the aid programs have emphasized financial support or its equivalent and have been administered by federal agencies. This type of approach has had somewhat disappointing results and enthusiasm for it has been declining. The academic community has contributed in a variety of ways, including bilateral arrangements between specific pairs of universities.

But there remains a considerable latent potential that has not been tapped. Many individual scientists at universities want very much to be helpful in other ways but do not perceive a suitable mechanism. Recently the Commission on Science Education and the Study Group on International Science of AAAS have suggested consideration of another possible approach that emphasizes intellectual rather than financial resources. The proposal is based on an analysis of the reentry problems faced by foreign students after they return to their homes in a developing country. One of their difficulties is that their education usually does not equip them for teaching science, which is the one thing that almost all of them will be asked to do when they return. This, however, has often not been clearly spelled out for them. As a consequence, they devote most of their time to study and research leading to the acquisition of an advanced degree. It has not been impressed upon them that their contribution to the economic and social development of their country may also depend quite strongly upon the impact that they may have as teachers. Their graduate studies have usually not helped them become better teachers.

A good student in physics, for example, may have been part of an eager team of researchers working in an exciting area in the vanguard of his field. He probably had access to modern library facilities and expensive and sophisticated research equipment. But when he returns to his country he finds that money, time, and facilities for research are not available. He is asked to teach an undergraduate course in physics for which he is unprepared. He becomes disillusioned.

The following is an example of the kind of assistance that might be provided to the student. A team of concerned AAAS scientists in his university could help him by communicating with the educational authorities in his home country and could help him try to determine what the nature of his teaching duties will be on returning home. They could guide him to obtain appropriate assistance in the form of teaching experience and exposure to modern approaches, methods, and materials for science teaching, possibly including the use of instructional technology suitable for his country. They could also give him some pieces of research apparatus to take home to enable him to continue some aspect of the research in which he had been involved here.

The main responsibility, of course, still rests with the authorities back home who should employ him in a position that takes advantage of both his educational and research experience. If correspondence with the scientists and educational authorities of his country could benefit from collaboration with the AAAS team on his American campus, it might receive greater attention than if the student attempted to do it alone.

We would welcome suggestions on the relative priority that might be given to this type of project. An enthusiastic response* could lead to implementation of such a program. We would also welcome suggestions of other possible approaches for involving the membership of AAAS in cooperating with developing countries.† In the coming years such help will be increasingly needed and the matter is worthy of our best efforts.—ALBERT V. BAEZ, *Chairman, AAAS Commission on Science Education.*

* Letters should be addressed to Dr. Arthur Livermore, Deputy Director, Science Education Office, AAAS, 1776 Massachusetts Avenue, NW, Washington, D.C. 20036. † Suggestions should be sent to Dr. Irene Tinker, Director, Office of International Science, AAAS, 1776 Massachusetts Avenue, NW, Washington, D.C. 20036.

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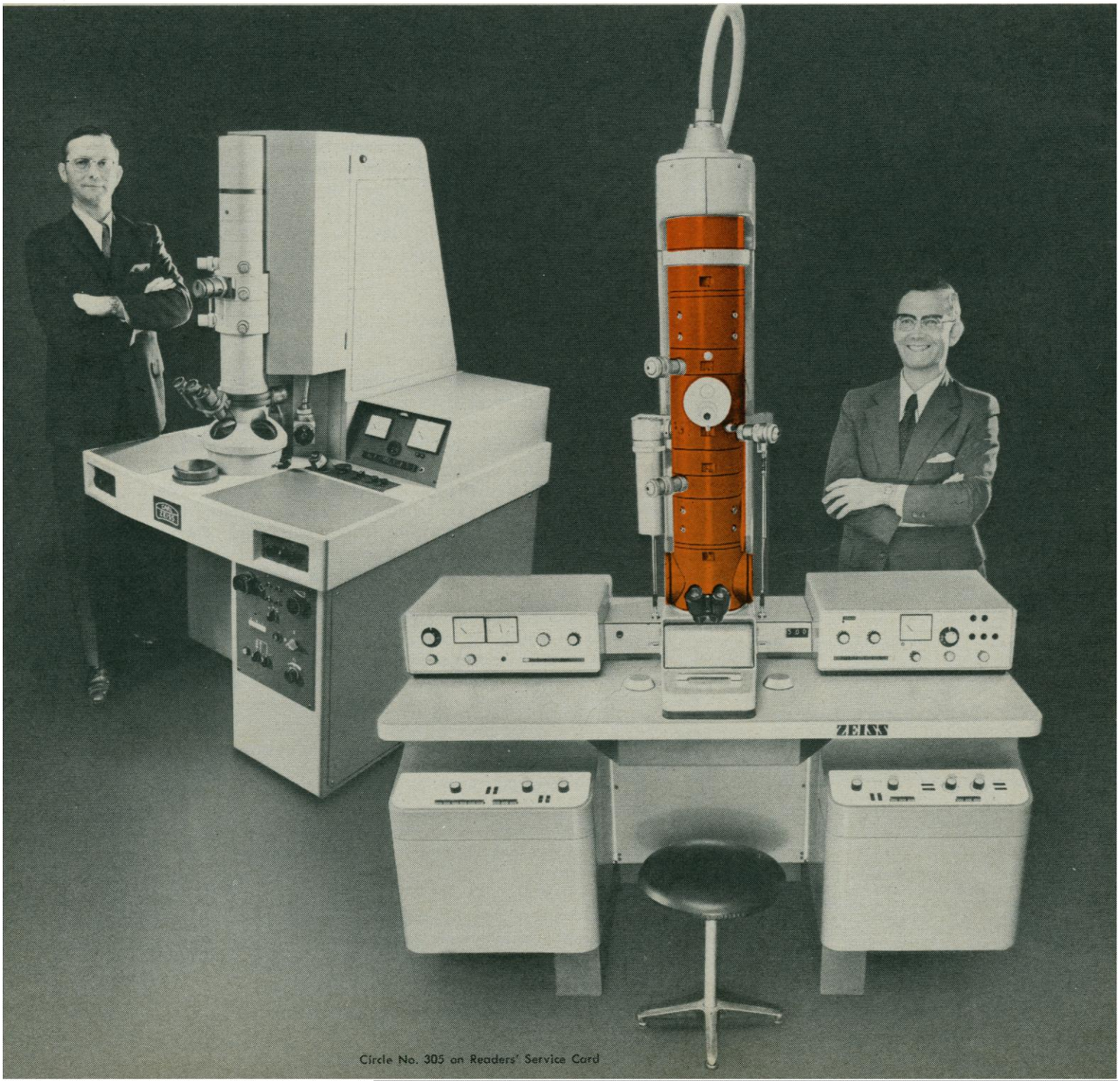

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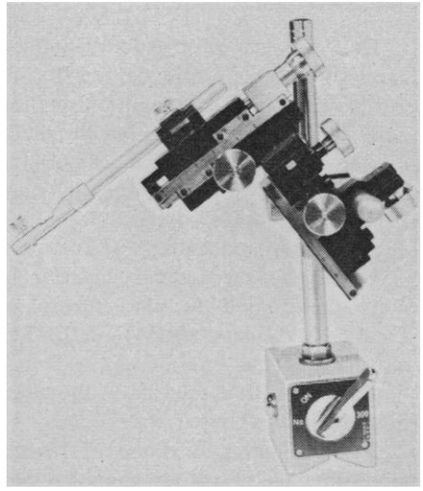

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Specification Selector for Wire Cloth is a 20-page guide to choice of materials for filtration and other laboratory uses. Michigan Dynamics Division, Ambac Industries Incorporated. Circle No. 347 on Readers' Service Card.

Effective Use of the Laminar Flow Biological Safety Cabinet is a 20-page pamphlet that treats construction, placement, advantages and disadvantages and maintenance of these devices. It is part of a training package that includes slides and a cassette recording also. U.S. Department of Health, Education, and Welfare, Public Health Serivce, National Institutes of Health, National Cancer Institute, Office of Biohazard and Environmental Control. Circle No. 348 on Readers' Service Card.

Optimum Parameters for Spectrophotometry discusses ultraviolet and visible spectrophotometry in terms of procedures, design, and topics related to accuracy and precision of photometric measurements. Varian Associates, Instrument Division. Circle No. 349 on Readers' Service Card.

Newly offered instrumentation, apparatus, and laboratory materials of interest to researchers in all disciplines in academic, industrial, and government organizations are featured in this space. Emphasis is given to purpose, chief characteristics, and availability of products and materials. Endorsement by *Science* or AAAS is not implied. Additional information may be obtained from the manufacturers or suppliers named by circling the appropriate number on the Readers' Service Card (see pages 410A and 506C) and placing it in the mailbox. Postage is free.—RICHARD G. SOMMER

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(Continued from page 458)

Drainage Basin Form and Process. A Geomorphological Approach. K. J. Gregory and D. E. Walling. Halsted (Wiley), New York, 1974. x, 456 pp., illus. $18.95.

The Earth Environment. John J. Fagan. Prentice-Hall, Englewood Cliffs, N.J., 1974. xii, 244 pp., illus. Paper, $4.95.

Electrodermal Activity in Psychological Research. William F. Prokasy and David C. Raskin, Eds. Academic Press, New York, 1973. xii, 502 pp., illus. $29.

Electromagnetism. F. N. H. Robinson. Clarendon (Oxford University Press), New York, 1973. x, 110 pp., illus. Paper, $4.95. Oxford Physics Series.

Electronic and Structural Properties of Amorphous Semiconductors. Proceedings of a seminar, Aberdeen, Scotland, July 1972. P. G. Le Comber and J. Mort, Eds. Academic Press, New York, 1973. xvi, 630 pp., illus. $35.

Environment and Birth Defects. James G. Wilson. Academic Press, New York, 1973. xiv, 306 pp., illus. $19. Environmental Sciences Series.

A Guide to Natural Cosmetics. Connie Krochmal. Quadrangle, New York, 1973. x, 228 pp., illus. $8.95.

A Guide to the Medicinal Plants of the United States. Arnold and Connie Krochmal. Quadrangle, New York, 1973. viii, 260 pp., illus. $9.95.

Human Behavior in the Social Environment. A Social Systems Approach. Ralph E. Anderson and Irl E. Carter. Aldine, Chicago, 1974. viii, 184 pp., illus. $8.50.

Immunology. The Many-Edged Sword. Harold M. Schmeck, Jr., Braziller, New York, 1974. 144 pp. Cloth, $6.95; paper, $2.95.

Implications of Continental Drift to the Earth Sciences. Vol. 2. Proceedings of a symposium, Newcastle upon Tyne, England, April 1972. D. H. Tarling and S. K. Runcorn, Eds. Academic Press, New York, 1973. xvi, pp. 625–1184, illus. $36.50.

Initial Reports of the Deep Sea Drilling Project. Joint Oceanographic Institutions for Deep Earth Sampling (JOIDES). Vol. 15, Dec. 1970–Feb. 1971. Prepared for the National Science Foundation by the Scripps Institution of Oceanography, La Jolla, Calif., 1973. (available from the Superintendent of Documents, Washington, D.C.). xxii, 1138 pp., illus. + charts. $17.65.

Integrated Theory of Finite Element Methods. John Robinson. Wiley-Interscience, New York, 1974. xxii, 428 pp., illus. $29.50.

Length of Stay in PAS Hospital, United States, 1972. Commission on Professional and Hospital Activities, Ann Arbor, Mich., 1973. lxii, 190 pp., illus. Paper, $8.50.

Length of Stay in PAS Hospitals, United States, Regional, 1972. Commission on Professional and Hospital Activities, Ann Arbor, Mich., 1973. lxxxviii, 662 pp., illus. Paper, $11.

Life-Span Developmental Psychology. Personality and Socialization. Proceedings of a conference, Morgantown, W.Va., 1972. Paul B. Baltes and K. Warner Schaie, Eds. Academic Press, New York, 1973. xvi, 452 pp., illus. $14.50.