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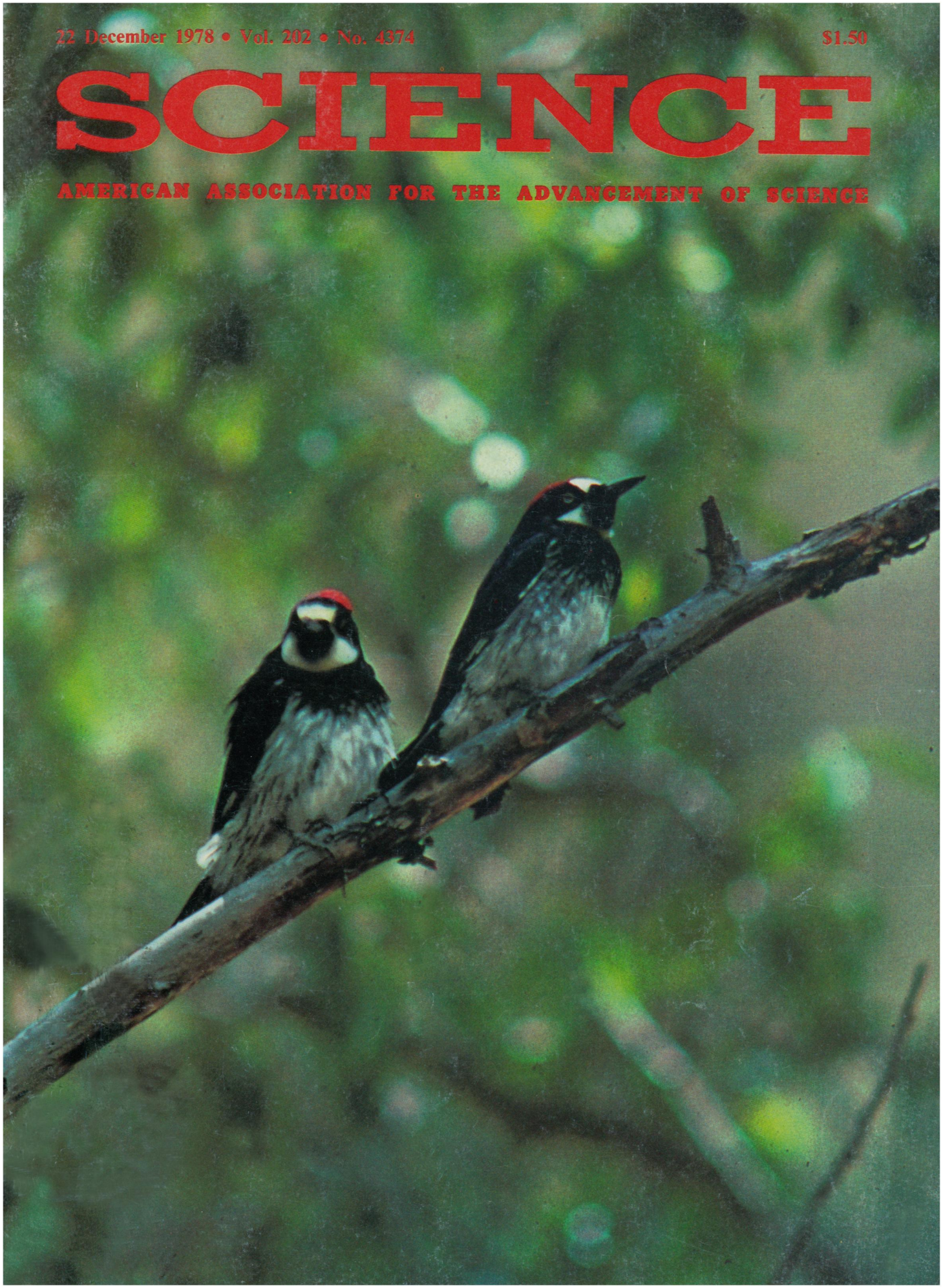

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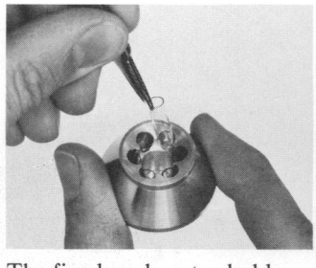

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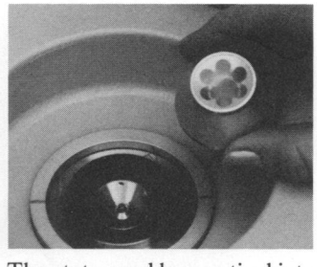

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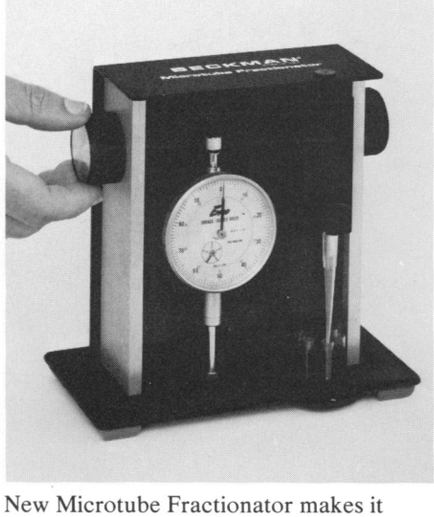

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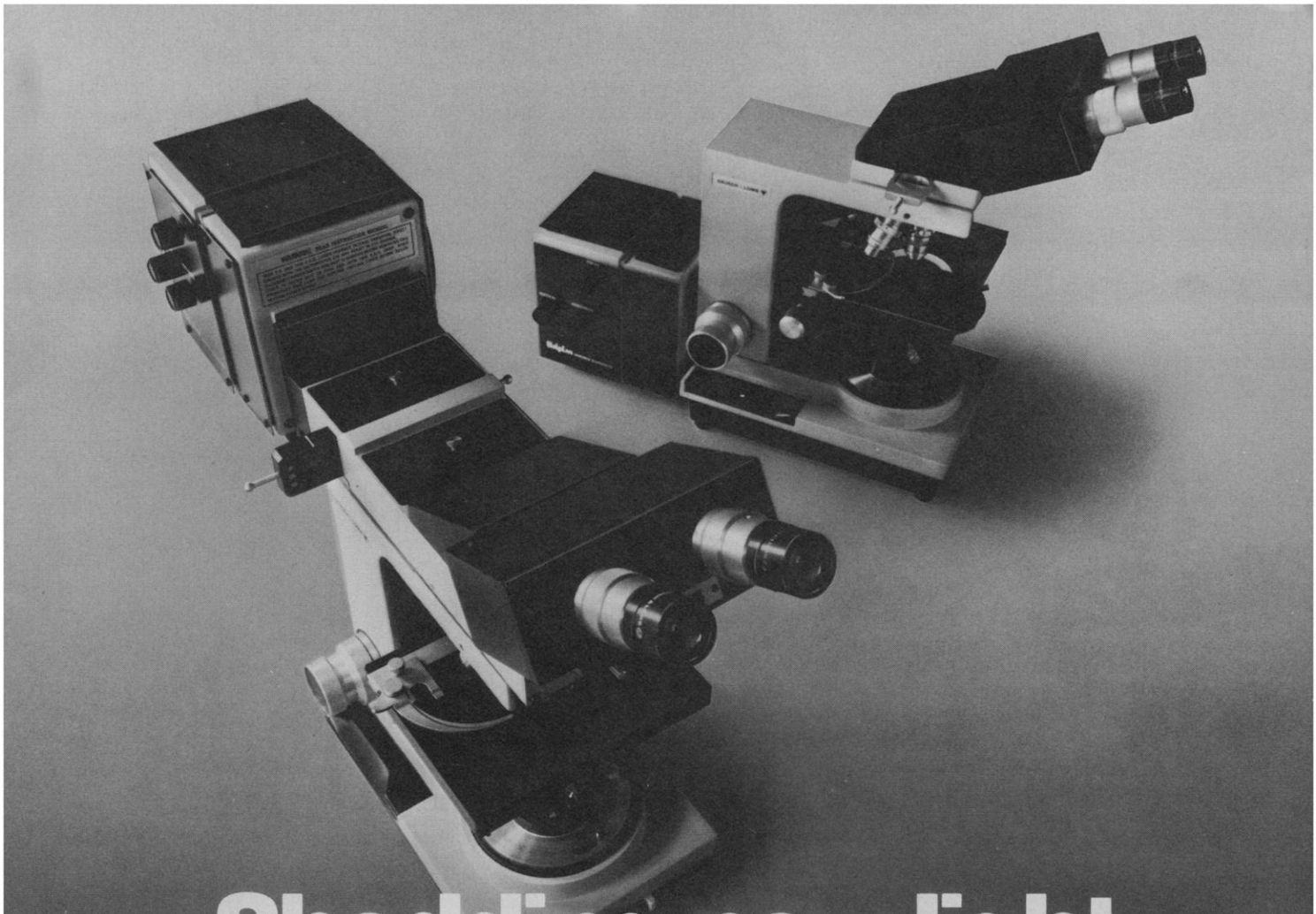

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COVER

Female (left) and male acorn woodpeckers. This species usually lives in resident, communally breeding social groups. In an area of southeastern Arizona, most of the woodpeckers migrate during the winter and breed in temporary pairs, indicating that this species can be highly plastic in its behavior. See page 1298. [Harlo Hadow, Coe College, Cedar Rapids, Iowa]

LETTERS

Occupational Health Research: Weill Replies

Sheldon W. Samuels' letter (17 Nov., p. 694) in response to Nicholas Wade's article of 8 September (News and Comment, p. 892) would not necessitate a reply were it not for the several departures from fact that should be brought to the attention of *Science* readers. Contrary to Samuels' characterization, I found Wade's article to be an accurate and lucid account of the circumstances underlying the regrettable "NCI [National Cancer Institute] disinvitation."

Samuels asserts that after the NCI action I remained on the planning committee for the conference on lung cancer surveillance. In fact, I never served on this planning committee, either before or after the disinvitation; never received a program of this meeting; and was never asked to attend in any capacity. Furthermore, the "regret" over this incident expressed by NCI officials to others was never communicated directly to me.

Second, in spite of Samuels' disclaimer, he appears to remain confused or uninformed about my role in the Occupational Safety and Health Administration (OSHA) cotton dust standard public hearings. A simple check with the Department of Labor (may I suggest Grover Wrenn, director of Health Standards for OSHA) would have revealed that I was specifically asked to give testimony and consultative assistance on (i) our study of cotton textile workers, (ii) our study of workers engaged in cottonseed oil processing, and (iii) standards and minimum criteria for the performance of lung function testing in workers. Samuels is in error in claiming that I was asked only to provide testimony on the last item, and he continues to ignore the fact that, in my testimony, I supported the OSHA standard for cotton dust exposures in textile mills (contrary to the position of the textile industry); this can be easily checked in the public record.

In addition to misstatements, Samuels' letter contains quotes out of context. In this case the quotes are two portions of my statement submitted to OSHA on the proposed asbestos standard of 1975. When the entire statement is read, these quotes are seen to be parts of a discussion of the two major alternative approaches to setting exposure standards based on dose-response data. Is a discussion of the options available within a strategy of disease prevention unacceptable to Samuels? There is increasing concern that the benefits which can be obtained by the setting of occupational health standards be supportable by scientific evidence (of the type which has led to recognition of carcinogens in the workplace). It is clear that, with limited expenditures possible for the reduction of environmental contaminants in the workplace, maximizing health benefits for workers requires that these dollars be spent where they will have demonstrable beneficial results. There is no paucity of responsible scientific and public thought in support of this thesis.

I must confess that I lose the substance of Samuels' discussion when he talks of our "experiments" on working populations. If he means by this the use of standard x-ray techniques and pulmonary function testing methods in the study of workers exposed to hazardous inhalants in many industrial settings throughout the country for the purpose of generating data on dose-response relationships, then we plead guilty! The application of epidemiologic methods for studying exposed populations in order to detect the determinants and distribution of disease forms the basis of effective occupational health standards. Samuels should be reminded that *we* do not *expose* the workers to the particular inhalant being investigated but rather direct our efforts toward the early detection of an adverse respiratory health response. If health surveillance by means of radiography and pulmonary function testing constitutes unacceptable "human experiments," then I suggest we have no common ground for dialogue. It should be pointed out that these methods are mandated by OSHA for workers exposed to some respiratory hazards (for example, asbestos). Does Samuels condemn this practice also?

More than three-quarters of the research support of our unit comes from federal funds (the National Institutes of Health and the National Institute for Occupational Safety and Health). The National Heart, Lung, and Blood Institute and the Quebec Asbestos Mining Association (QAMA) did not *match* funds; they independently have funded various phases of our research on asbestos health effects. Samuels implies that support for specific studies from industry, such as the American Textile Manufacturers Institute for the textile study and QAMA, which in part has supported our asbestos work, in some way produces a fatal blow to our objectivity and credibility. I vigorously reject this concept and firmly contend that the segment of our society which is in the best position to modify an injurious occupational envi-

ronment is management, which has not only the *right* but the *responsibility* to seek and support competent investigation and consultation from the academic community. Indeed, they should be considered remiss (frequently the case in past years) if they do not make it possible to generate objective, unbiased data from studies of the occupational environment and health status of their workers. The polarization engendered by the thoughts expressed by Samuels will serve to accomplish exactly the opposite. Credibility of scientific data and conclusions resulting from industry-supported research or, for that matter, research supported by any other segment of society should be judged by the integrity of the investigators and by peer review of the study design, collection of data, analyses, and interpretation of results. We have always welcomed such critical review on the merits of our research, for which personal attacks based on "perceptions" are no substitute.

Although Samuels makes sweeping comments concerning how my colleagues and I are perceived by various public interest groups, the eloquent statement by J. M. Calhoon (Letters, 17 Nov., p. 694) of the Marine Engineers' Beneficial Association which follows Samuels' letter makes it clear that Samuels does not even speak for the labor movement. I suggest that he redirect his energies toward substantive activities which will promote the health of the American worker and abandon his philosophy that all scientists working in this area must be identified with one camp or the other. I also suggest that he visit our unit at Tulane in order that he might better know his "adversary"; I predict that if he came with an open mind, he would be convinced of the dedication and credibility of the scientists working in our multidisciplinary group. In any case, we will not be intimidated or discouraged from continuing our investigations, which we hope will, as before, be with the full cooperation and support of workers, management, and government research and regulatory agencies.

HANS WEILL

Tulane Medical Center,
New Orleans, Louisiana 70112

Lovins' Data Source

Amory B. Lovins (Letters, 22 Sept., p. 1077) gives the impression that the "Bechtel data base" is the foundation for his arithmetic on capital costs of conventional energy technologies and, implicitly, his conclusions on the comparative economic advantages of soft energy paths. The purpose of this letter is to record my reservations about the methods employed by Lovins in his use of this data base.

Lovins refers to the Bechtel data base in 6 of the 19 citations in his letter and, in a previous reference (*1*), cited it as "probably the most detailed, authoritative, and up-to-date [data] available." Statements such as "In fact, they are Bechtel's data . . ." (referring to his assumptions on the capital costs of nuclear-electric systems) and "derived from the Bechtel data base" (regarding his assumptions for cost of 1980's U.S. frontier oil and gas) are used to authenticate his numerical analysis.

In all these citations, Lovins is referring to the data base of the Bechtel Energy Supply Planning Model (ESPM), which was developed initially by Bechtel under contract to the National Science Foundation (*2*). As codeveloper of the model and principal investigator responsible for its development since April 1975, I am familiar with the characteristics of the model and data base and with Lovins' use of the data base in his stream-of-numbers logic. In my judgment his data and conclusions bear little relation to the ESPM data base with which he purports to have started.

My principal criticisms, which I will elaborate in more detail below, are four:

- *Use*. He stretches the use of the data base for purposes which go beyond its design objectives.
- *Consistency*. He makes selective use of the data base and thereby loses consistency across technology comparisons.
- *Extrapolation*. He adds various factors not part of the data base to the basic model data.
- *Currency*. He continues to base his calculations on information published in 1975 despite the availability of extensive literature documenting updates of the ESPM since that time.

1) *Use*. The ESPM was designed to provide a tool for calculating the magnitude and timing of resources (capital, labor, materials, equipment, land, and water) required to implement alternative energy development programs. This required the development of resource data on the requirements of individual energy supply and transportation facilities. We recognized that this facility resource data base might, in addition to serving the specific needs of the ESPM for which it was designed, be useful as a starting point for other research efforts.

However, as stated in the model's

original documentation (*2*), we also recognized that the data base was inappropriate for some uses:

> If the primary purpose of a model is to optimize the technology to be used in a future energy system, data are required that state explicitly technology-cost relationships. This information, while extremely significant by itself, tends to be of secondary significance in relation to the purpose of the present study, since the emphasis is placed on obtaining information on the resources required, not on the technology best suited.

Selection of best-suited technologies is precisely the type of analysis Lovins is engaged in, but it is *not* the type of analysis for which the ESPM and its data base were developed.

2) *Consistency*. The primary strength of the ESPM and data base derives from its internal consistency, that is, the resource requirements data have been developed under common ground rules by a single organization. However, Lovins did not take full advantage of this consistency because he did not use the ESPM's cost data for solar space heating and cooling technologies (2 of the model's 98 technologies), which he considered "unreasonably high" (*3*). As these were only decentralized technologies in the data base, his integrated analysis relies on the ESPM only for centralized energy technology data, and he pulls together data from a myriad of sources (with accounting conventions different from those of the ESPM) for the various soft energy technologies he considers.

3) *Extrapolation*. The intent of the ESPM analysis is to calculate direct resource requirements in specific categories which are felt to be potential constraints to energy development, rather than to calculate total dollar costs of alternative energy futures. Our documentation (*4*) states that, "Reported dollar costs do not cover the total dollar costs of energy programs. . . . In particular, the excluded cost factors represent a major fraction of total energy costs." Lovins apparently recognized this fact and proceeded to add to the basic model data various factors that were not part of the data base. However, his resulting data are often dominated more by the factoring assumptions used than the original ESPM data base.

Lovins' treatment of capital costs for coal-electric and nuclear-electric systems in (*1*) clearly illustrates this fact. Lovins comes up with capital costs of $2476 per kilowatt delivered for coal-electric systems and $3179 to $5000 per kilowatt delivered for nuclear-electric systems. [Here he says in (*1*) that "the estimate of over $3000/Kw seems unrealistically low" and "A realistic calculation would yield a nuclear capital cost nearer . . . $5000/Kw delivered."] Contrast these figures with the original ESPM data of approximately $1100/Kw installed capacity for both coal and nuclear systems (in 1974 dollars, including transmission and distribution). The figures cited by Lovins appear to be strongly influenced by arbitrary assumptions on his part having nothing to do with the Bechtel work, and such statements as, "In fact, they are Bechtel's data" are thus extremely misleading.

4) *Currency*. While I appreciate Lovins' comment in (*1*) that "Dr. Gallagher has generously shared the results of Bechtel's update of the model's data base," the fact is that no significant update had yet occurred at the time of that writing. However, since that time 13 reports have been published which describe updates, refinements, and uses of the model and data base. None of these updates appear to have been incorporated by Lovins in his arithmetic. At least three of the published reports (*5–7*) are directly relevant to Lovins' use of the ESPM data in that they describe significant modifications of the basic data originally developed in 1974–1975. As an example, in (*6*) detailed engineering statistics are developed on the escalation in costs for 98 types of energy facilities from 1974 to 1977. Even a cursory glance at the current ESPM data base as described in these reports indicates the need for substantial reworking of Lovins' estimates.

Lovins has stated in (*1*) that "a serious question can be raised whether economic calculations are particularly relevant today." Without debating that issue, I would argue that if economic calculations are to be a component in the analysis of centralized versus decentralized energy strategies, the calculations must be based on consistent and current information.

J. MICHAEL GALLAGHER
Research and Engineering, Bechtel National Inc., Post Office Box 3965, San Francisco, California 94119

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Chemical Industry and Regulation

Philip H. Abelson's editorial, "Regulation of the chemical industry" (3 Nov., p. 473) describes problems for both the chemical industry and individual chemists and chemical engineers working in the industry. In view of the enormous quantities of chemicals manufactured and their wide distribution, these practitioners should be commended for their efficient use and disposal of chemicals to date. The uncertainty about toxic effects from chemicals in our environment, including the manufacturing facilities, suggests that the industry and chemists should not be subject to undue regulation at this time but instead should be given recognition for their effective track record. Regulation of the industry and the licensing or registration of chemists, as proposed in some states, does not appear justified except in cases where chemical use is directly related to the health of the public.

The American Institute of Chemists, through its new National Certification in Chemistry and Chemical Engineering, endorses the concept of encouraging voluntary self-development through continuing education and professional activities to offset technical obsolescence and to recognize those who make the extra effort to demonstrate their responsibilities within the profession. We believe this to be the preferred course for the profession to take in seeking solutions to the problems identified in Abelson's editorial. It may be true as pointed out in the editorial that there will be thousands of toxicologists obtaining their livelihood from the study of the toxicity of the products of the chemical laboratory. We would rather hope that this proliferation, which will be an added burden to the industry and to the consuming public who will eventually have to pay the costs, can be offset by evenhanded government policies established with the cooperation of the chemical societies. The president of the American Chemical Society, Anna Harrison, has worked extensively to fill this need and has done so with considerable success. Chemists, chemical engineers, and the chemical industry have shown responsibility for their activities in the past. I believe we can expect them to do so in the future.

S. DAVID BAILEY
National Certification Commission in Chemistry and Chemical Engineering, American Institute of Chemists, 7315 Wisconsin Avenue, NW, Washington, D.C. 20014

I am prompted to respond to Abelson's editorial wherein he relates the woes of the chemical industry that result from federal regulations. I take issue with the generally negative tone of the editorial and suggest that if Abelson wished to get a true feeling for the impact of federal regulation on chemical companies and the impact of chemical companies on the public, he made a basic error in visiting "six of the major industrial laboratories in the United States." To gain greater insight into the magnitude of public danger and the nearly insurmountable obstacles to efficient regulation, he should have visited some of the literally thousands of small and medium-sized chemical manufacturers which are virtually unregulated.

There seems to be a general image fostered by the industry that chemists are overworked, dedicated scientists hampered in their lifesaving research by overzealous, uninformed federal inspectors wielding unreasonable demands for quality and safety. As a manufacturer falling under the scrutiny of three federal agencies, I have yet to encounter a regulation that was not based on a sound knowledge of the field, or that imposed unreasonable demands on the manufacturer. Almost without exception, the "unreasonable" demands are those that prevent the companies from taking dangerous shortcuts.

Chemical manufacturers are profitable corporations, and they will resist any regulation that might diminish profit. If safety and accountability become the watchwords of the industry, it will only be after years of federal and public insistence have molded corporate policy and structure to ensure that safety and accountability are cost-effective.

DONALD J. HOUSE
Immuno Assay Corporation, 25050 Ford Road Dearborn Heights, Michigan 48127

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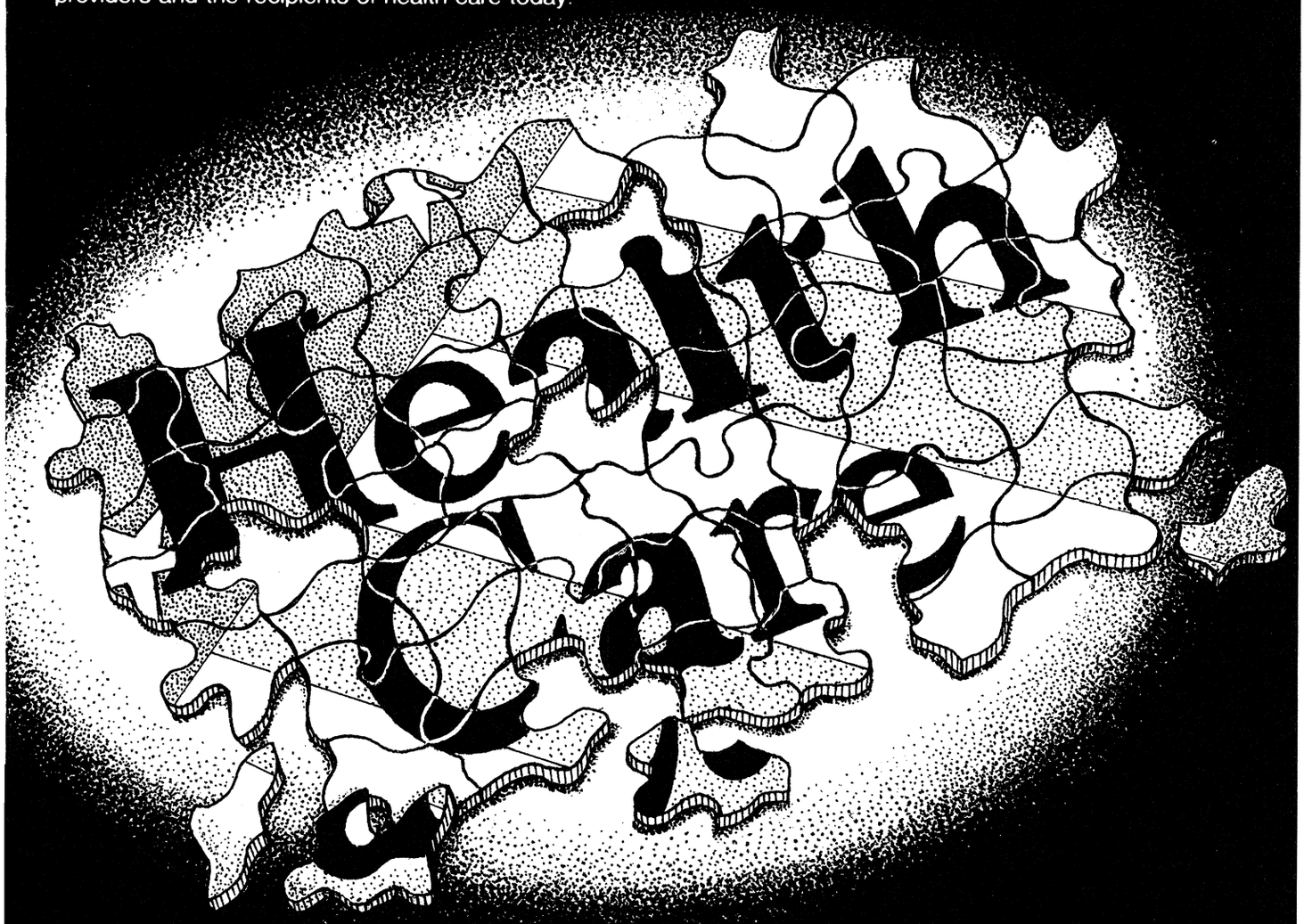

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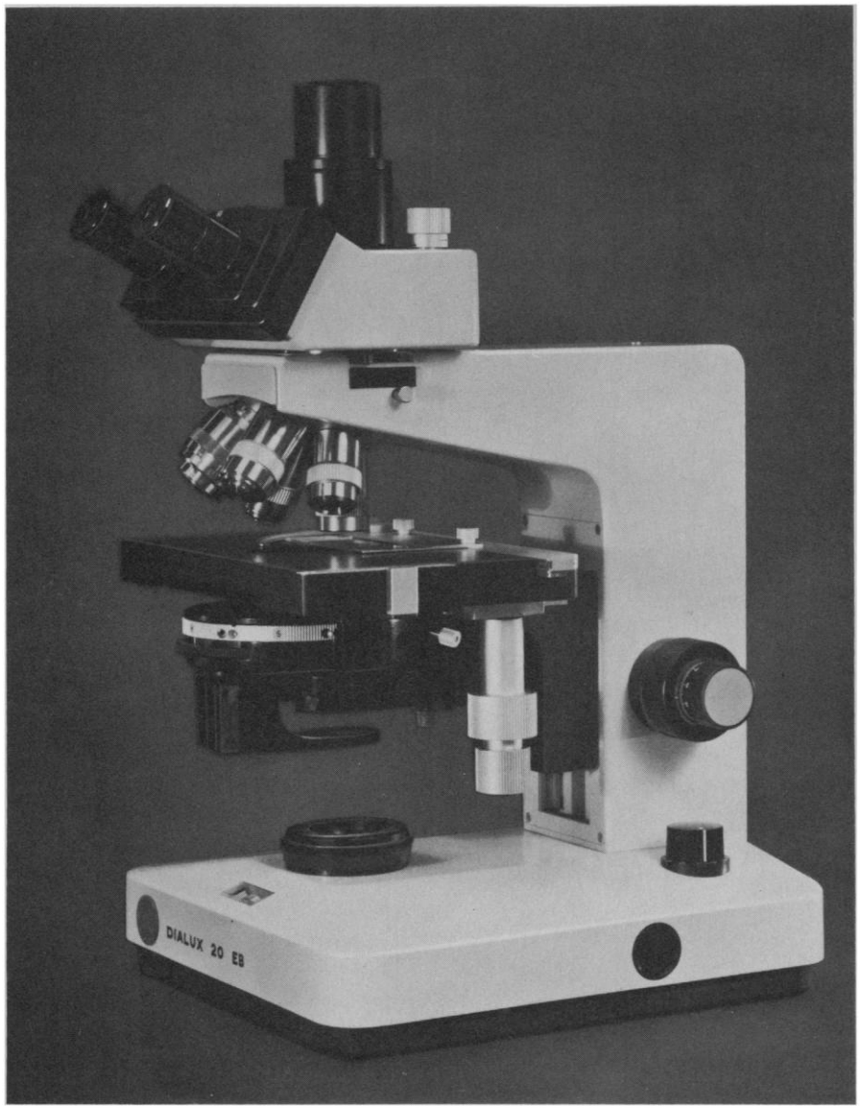

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Information Transfer: The Biomedical Model

"Knowledge is of two kinds. We know a subject ourselves, or we know where we can find information upon it." In 1775, when Samuel Johnson wrote these words, there existed men of science who could claim to possess knowledge in their disciplines that was both broad and deep. Today, even with the fractionation of science, it would be a presumptuous scientist who claimed to know everything in his specialty. What is true for scientists is true also for librarians; to paraphrase the great doctor: scientific literature is of two kinds—we have the information ourselves, or we know where to borrow it.

The problem of information transfer in contemporary science is exacerbated by two trends. First is the geometric increase in published knowledge in all branches of science and technology. Second, as inexorable as the first but more pernicious, is the rapidly rising cost of books and journals. Many libraries are not able to keep pace. The net result of both trends has been a decrease in the proportion of the total scientific record held by each library.

Steps to cope with this dilemma are already being taken by libraries and information centers. The solution is a long-term one and has two elements: improving our ability to search the aggregate record of what has been published and *identify* pertinent materials, and improving our ability to then *retrieve* the books and journal articles themselves.

For the first of these two elements the key is computerized on-line bibliographic retrieval. In the area of biomedicine it has been demonstrated that a large and growing body of literature, both periodical and monographic, can be indexed, entered into a central computer, and searched economically in real time from computer terminals in more than 900 institutions across the United States. The experience gained from operating this system—called MEDLINE—should have wide application in other scientific fields.

Today's health professional has an array of bibliographic data bases available for instantaneous searching over the MEDLINE network: journal articles, monographs, audiovisual materials, toxicology and environmental health data, chemical information, health planning and management literature, cancer research information, and so forth. The number of references and abstracts in these combined data bases is now approaching 4 million. More than 1 million on-line searches are being performed each year.

The second element—retrieving the actual book or article—depends not so much on computers (although they have their place) as on sharing resources. As the increasing volume of scientific literature strains the capacity of shelves and budgets, science librarians rely more and more on cooperation to provide for the needs of their users. In many instances local and regional consortia have been formed. Union catalogs and lists of periodicals allow an information center of modest size to provide access to what would be an extensive collection if it were housed in one institution.

In this area also, the health sciences have assumed a leading role. A network of biomedical libraries, ranging from local community hospitals to 11 regional medical libraries to the National Library of Medicine, ensures that a document, no matter where it is located, is available to any other member of the network. Within the network, more consortia of health science libraries are being encouraged. Members of consortia not only share their books and journals but arrange cooperative on-line search services and training—activities that individually they are too small to engage in but that collectively they find feasible.

The information services pioneered by biomedical libraries may provide a useful pattern for improving communication in other fields of science. There are problems yet to be overcome, but experience indicates that the basic approach of computerized bibliographic access with concomitant library resource sharing is sound.—MARTIN M. CUMMINGS, *National Library of Medicine, National Institutes of Health, Bethesda, Maryland 20014*

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