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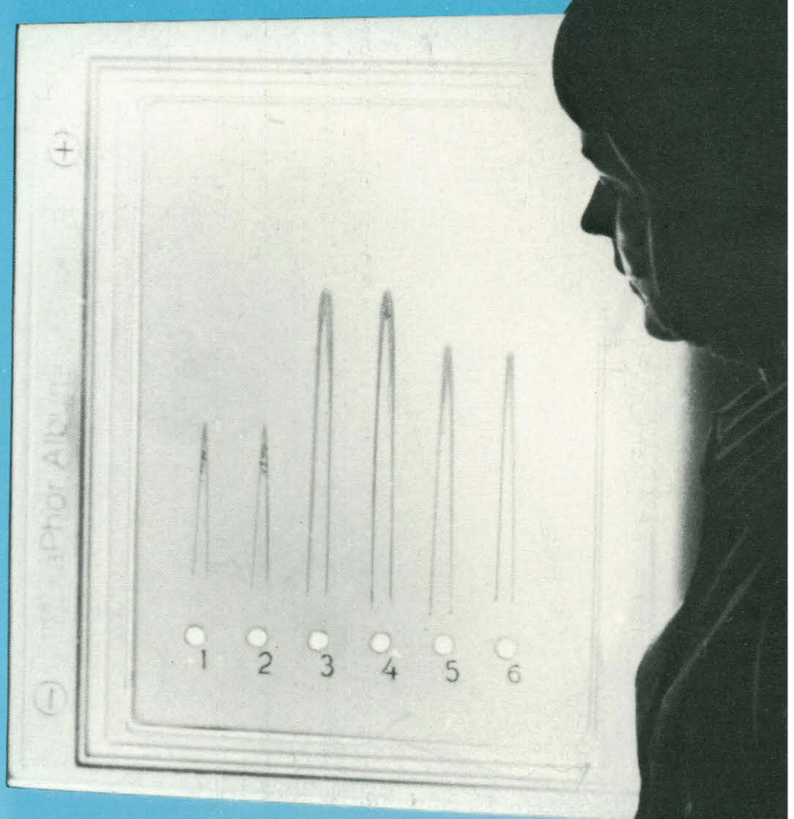

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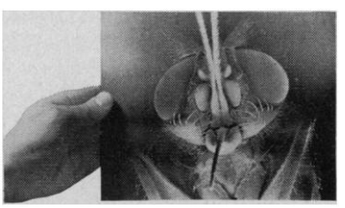

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COVER

Twin monolithic quartzite statues of the Egyptian pharaoh Amenhotep III (18th dynasty, circa 1400 B.C.) are identified in classical times with the Greek hero Memnon. Research in 1971 and 1972, supported by the National Geographic Society, showed that each statue weighs 720 metric tons and stands 14.3 meters above its pedestal block. The material was secured at Gebel el Ahmar, 420 miles downstream on the Nile. See page 1219. [Fred Stross, University of California, Berkeley]

LETTERS

Deep-Sea Drilling

Deborah Shapley's report "Law of the sea meeting: A wet blanket for ocean research" (News and Comment, 14 Sept., p. 1024) was most interesting but told only part of the story.

The spokesmen for the American scientific community have weakened their prospects for a sensible regime for ocean research by their insistence on unlimited freedom. There is nothing sacrosanct about a research vessel. It is just as capable of polluting the seas or creating a hazard to navigation as any ordinary vessel. Moreover, the *Glomar Challenger* has demonstrated a capability to reenter the drill hole in the ocean floor with fresh drill bits that would enable it to strike oil or gas in deep waters. At the same time it has no capability to prevent blowouts comparable to that required of commercial operators. This means that a research vessel could possibly cause a blowout of such disastrous proportions as to pale the Santa Barbara blowout by comparison. Yet, the spokesmen for the American scientific community have persisted in their quest for total freedom from coastal nation control over scientific research beyond a narrow territorial sea.

The U.S. government, in its initial position in the United Nations Seabed Committee, went along with this view and worded its proposed draft of an international seabed treaty of 3 August 1970 accordingly. One need only visualize the reaction of the good citizens of Santa Barbara to word from Washington that neither the federal government nor their own local or state government had any power whatever to prevent a foreign counterpart of the *Glomar Challenger* from proceeding with a deep-sea drilling experiment in the Santa Barbara Channel just beyond the 12-mile limit to appreciate that this position had to be modified—as the U.S. delegation has now done—in the light of our own national interest and that of other coastal nations.

The 1958 Geneva Convention on the Continental Shelf, in a clause incorporated into article 5 of the treaty, reportedly with the active support of the United States, prescribes that

> 8. The consent of the coastal State shall be obtained in respect of any research concerning the continental shelf and undertaken there. Nevertheless, the coastal State shall not normally withhold its consent if the request is submitted by a qualified institution with a view to purely scientific research into the physical or biological characteristics of the continental shelf, subject to the proviso that the coastal State shall have the right, if it so desires, to participate or to be represented in the research, and that in any event the results shall be published.

The scientific community has, at times, admittedly had serious difficulties obtaining the consent required by this clause or the comparable consent required by nonadherents to the Geneva convention under customary international law. These difficulties provide a solid basis for seeking meaningful modification of the quoted language. They do not, however, provide a basis for its complete elimination.

The American Bar Association (ABA) has taken a constructive approach to this important problem in the resolution on the natural resources of the sea adopted at its last annual meeting on 6 August 1973. In the portion of this resolution dealing with scientific research, the ABA

> (12) SUPPORTS the general principle of freedom of scientific research, but recognizes the right of coastal States, within internationally agreed guidelines designed to provide the maximum practicable application of this principle, to impose reasonable restrictions on activities on their continental margins which will entail threats to their national security or hazards to the environment, as by drilling into the seabed.

The end result of the international negotiations now under way is much more likely to be palatable to the American scientific community if its spokesmen will support this sensible approach to the problem and work with the U.S. delegation for its effective implementation.

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Nitrites in Foods

A. E. Wasserman and I. A. Wolff, who discuss the use of nitrate in their reply to P. H. Schuck and H. Wellford (Letters, 29 June, p. 1322), do not deal adequately with the question of the use of nitrite in cured meat and fish products, which Schuck and Wellford suggest is an unnecessary hazard to health. Wasserman and Wolff also ignore the problem of formation of carcinogenic nitrosamines in vivo (*1*), which Schuck and Wellford address in their letter. It is this amply

demonstrated possibility which poses the greatest hazard to the public, and is the major reason for the proposal to eliminate nitrite from our food whenever possible.

A limit of 200 parts per million (ppm) of *residual* nitrite in food (meat) set in 1926 is arbitrary and has no scientific basis. The preservative effect depends on the amount of nitrite added to the food before processing (a minimum, so it is said, of 150 ppm). After processing, the residual nitrite can be, and often is, as little as 10 ppm in, for example, ham or canned luncheon meat. It is this residual nitrite which takes part in nitrosamine formation in vivo, and it would seem that an upper limit of 200 ppm is far higher than indicated by good manufacturing practice, and high enough to be a threat to health.

Wasserman and Wolff state that there is a long history of usage of nitrate (and, by implication, of nitrite) with apparent safety. This is an unwarranted conclusion, since cancer is a widespread and common affliction, the cause of which is unknown. Evidence is accumulating about the formation of nitrosamines from nitrite and secondary or tertiary amines (agrichemical residues, drugs, and so forth), both in food and in vivo, which suggests that nitrosamines formed in this way are a cause of cancer (*2*), perhaps the major one. If, as Wasserman and Wolff suggest, the avoidance of botulinus poisoning takes precedence over the possible carcinogenic hazard from nitrosamine formation, consistency would demand that food manufacturers add nitrite to all products in which a botulism hazard exists. One can assume that this is their recommendation for vichyssoise soup and processed mushrooms, large batches of which have been recalled in the past year or so because of the finding of *Clostridium botulinum* contamination in some samples. The feeding study of Van Logten *et al.* (*3*), cited by Wasserman and Wolff, is irrelevant, as the usually accepted practice in toxicology was not followed, namely the administration of greatly exaggerated doses (often 100 times or more the human exposure) to compensate for the small number of animals (180) in the experiment. The results of this 2-year feeding test (in which *nitrite* was added to the meat), cannot possibly be extrapolated to the experience of millions of humans who might consume proportionate doses of nitrite for 50 years or more. Moreover, this experiment did not test the possibility of

formation of nitrosamines in the stomach.

Wasserman and Wolff state that there is no correlation of the amounts of nitrosamines that might be ingested under normal conditions with development of harmful effects, in man or animals. I draw their attention to the philosophy behind the Delaney Amendment—any amount of a known carcinogen is a hazard. To talk of the *potential* hazard of nitrosamines found in food is obfuscation, since these are carcinogens several orders of magnitude more potent than aminotriazole or cyclamates, which have been banned by the government. The benefit of the doubt should be given to the public rather than to the food processors. Surely it is time that nitrites (and nitrates) were removed from the GRAS (generally regarded as safe) list, as were cyclamates, until such time as they are proved safe for human consumption beyond a reasonable doubt.

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Journal Evaluation

Garfield, in his empirical study "Citation analysis as a tool in journal evaluation" (3 Nov. 1972, p. 471) compares *Solid State Physics, Immunology, Journal of Experimental Analysis of Behavior, Chemical Review*, and other journals with small intersections of common interest. This seems strange, but acceptable, as long as the data are used carefully. However, because I wondered why there were no general geology journals listed in the 152 most frequently cited journals ranked by impact factor (figure 8, p. 477), I talked to a science and technology librarian and was struck with the potential for misuse of this article. Garfield's study can, and probably will, be misused by library administrators who want to cut back on periodicals. After all, Garfield does say (p. 474), "It is apparent, even from the makeup of this partial listing, that a good multi-

disciplinary journal collection need contain no more than a few hundred titles. That is not to say that larger collections cannot be justified, but it does say something *indisputable*, in terms of cost and benefit, about how large a journal collection need be (or how small it can be) if it is to provide effective coverage of the literature most used by research scientists" (italics added). Also (p. 477), "Another application, which harried librarians may welcome, is the correlation of data on citation frequency and impact with subscription costs. Such a correlation can provide a solid basis for cost-benefit analysis in the management of subscription budgets." One might add that it can also provide a tenuous basis, since "harried" librarians, by definition, would be unlikely to analyze the analysis.

Some years ago (*1*) I expressed doubts about the effectiveness of citation analysis as a tool for evaluating individual authors, and I have some of these same doubts about Garfield's use of citation analysis as an evaluative tool for journals. The publication of papers that are original, creative, and cite few other papers would be inhibited. Editorial preference would be given to papers with prolific citations of the publishing journal and of journals with which they have friendly affiliations. In some cases, referees would require that references to their own papers be included before a paper is accepted for publication. These kinds of pressures would result in the inclusion in papers of many unjustified citations and would lead to distorted evaluations.

It seems, also, that answers to many of the "Unanswered questions" in the closing section of Garfield's article could have been hypothesized, and the study could have been based upon an experimental design to determine the usefulness of the hypotheses. Instead Garfield uses a purely empirical approach, mixing apples, oranges, and ad hoc speculations.

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The analysis of journal citations described by Garfield provides food for thought, since many scientists have a largely unproductive reading list. The reason for this broad scanning of the literature is not only the diverse nature of the interests of many active workers, but also that many are working in fields that overlap disciplines. The journals listed by Garfield as most frequently cited are major field journals devoted to broad coverage of highly populated disciplines.

It is significant that no veterinary, animal science, or pathology journals are listed. These fields are well covered by a relatively large number of journals, considering the number of people actively engaged in full-time research. They are also fields in which a worker must draw from many disciplines to provide a basis for his work. One worker may publish regularly in basic journals and in those devoted to his discipline. These factors place the journals serving the low-population, interdisciplinary fields at a marked disadvantage in any "numbers game" evaluation.

I would be loathe to see any policy decisions made from such a preliminary, all-encompassing study, particularly by libraries assessing their journal collections. The library is a store of information to be used, not to be kept or maintained for itself. It is a service to the community that supports it. In this regard a journal has served its purpose if it is *consulted* once. How many consultations make a citation? Too many libraries have lost the service concept without their being given a "numbers game" basis for their policies.

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Several earth scientists have asked why geology journals were not represented on the list of 152 high impact journals. The list originally submitted contained 565 titles. The *American Journal of Science* ranked 182nd by impact; the *Journal of Geology* ranked 240th.

Information on the most frequently cited 1000 journals has now been published (*1*) in the *ISI Journal Citation Reports* (*JCR*), issued by the Institute for Scientific Information. The *Journal of Petrology* ranked 50th. However, this should not obscure the fact that most geology journals do not have a broad impact. The most important general "geology" journals include *Science* and *Nature*. They are cited in geology journals more often

than most specialized geology journals. It is interesting that Janke does not mention the *Journal of Geophysical Research*, which ranked 60th; *Geochimica et Cosmochimica Acta* (104th); and the *Journal of Sedimentary Petrology* (146th). Similar comments could be made with respect to other fields, such as veterinary science and pathology, which are more dependent on basic biomedical and multidisciplinary journals than on those in their own narrow specialty.

Certainly *JCR* and other citation data *can* be misused. But so can other techniques. Objective sociometric data or statistics can be manipulated for political or other objectives. Should we abandon the census for this reason?

A number of "harried" librarians and departmental chairmen are using *JCR* data to weed out rarely used journals but also to add journals previously excluded. Most scientists are aware that society is demanding more efficiency in the operation of research establishments, but Janke and Bide do not discuss cost-effectiveness for libraries with finite budgets. Bide would justify the purchase of expensive journals even if they are used only once. Weinstock (*2*) demolished an earlier assertion that a library can aspire to order anything and everything. The ultimate extension of such an assertion is that each specialized library should become a Library of Congress.

Janke refers to his previously expressed doubts about citation analysis but doesn't cite any evidence to support his earlier contentions. Can he provide any examples of "papers, however bad, being heavily cited, while others remain uncited because they are too far ahead of their time" (*3*)?

The assumption that original and creative papers cite few other papers is completely contrary to fact. In the 10 years that the *Science Citation Index* has been published, there has not been any perceptible change in the rate of citation by or to the average paper.

The factual answers to many of the speculative questions I posed in my article are available in my weekly articles in *Current Contents* (*4*). I gladly confess that I am an empiricist.

Countless examples could be cited to prove that the impact numbers game can work to the advantage of small journals that would otherwise be neglected in favor of larger or more familiar and prestigious journals. Furthermore, before the existence of *JCR*, would it have been obvious that the virology journals had become basic to plant pathology collections, or that the *Journal of Experimental Medicine* covers mainly immunology, or that the *Journal of Petrology*, although 636th in citations, ranked 50th by impact? (The last observation is based on data compiled after my article was published.)

Bide and Janke seem to give my colleagues in the library and information sciences little credit for their ability to analyze data.

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Mesoamerican Calendar

My attention has been called to an error in my report (7 Sept., p. 939) on the Mesoamerican calendar. Charles H. Smiley of Brown University has kindly pointed out that it is only the Goodman-Martínez-Thompson correlation whose zero starting point corresponds to the zenithal sun position I have postulated as the origin of the Mesoamerican calendar, and not the Spinden correlation, which is some 52 days out of phase with it. Thus, inadvertently, my findings have provided further reason to accept the validity of the former system and to finally reject the latter. This conclusion is also warranted by radiocarbon dating done at the University of Pennsylvania and called to my attention by Henry N. Michael of Temple University.

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SCIENCE

Regulation of Human Experimentation

The scientific community should view with caution the current drive to set up government regulation of the use of human subjects in research (*Science*, 19 October, p. 265). Too often, in too many countries, authorities, in the name of some worthy cause or another, have imposed restrictions on the freedom of inquiry.

Serious abuses of free inquiry have undeniably occurred. As one example, researchers studying syphilis among poor blacks in Tuskegee, Alabama, allowed the disease to run its course so they could complete their investigation of its long-term effects. Such abuses involve only a tiny minority of investigators. Nevertheless, the transgressors are researchers, and a lay person can hardly distinguish between them and the overwhelming majority of ethical scientists.

The abuses have not gone unnoted. Hearings, held first by Senator Walter Mondale, and more recently by Senator Edward Kennedy, have focused the attention of Congress, the Department of Health, Education, and Welfare, the media, and the public on these problems. The World Health Organization and HEW have formulated a variety of regulations, including creation of ethical review boards that could withhold prior approval of research involving human experimentation and could sanction violators. A tough regulatory bill drafted by Kennedy's staff and approved by the Senate, but not yet by the House, hangs over us (*Science*, 19 October, p. 265).

The scientific community should not delay setting up its own ethical standards and regulatory mechanisms for dealing with possible abuse of human subjects. The reasons are compelling. First, subjects do need protection, and if scientists can agree together to provide it, it can be done in ways that will not unduly bureaucratize or hobble science. Second, government regulations are aimed chiefly at "federally funded programs"; persons serving as subjects in other research—especially that funded by drug manufacturers—need protection at least as badly. Third, concern with the humanitarian aspect of scientific work should not have to be imposed on researchers. Researchers should express their commitment to solving this problem by voluntarily providing effective mechanisms for dealing with it.

The first rung of such a voluntary review ladder should be local human-subject review committees composed of scientists; persons from other academic disciplines, such as humanities, law, theology; and some representatives of the subjects themselves. The next rung should be constituted of regional appeal boards. The highest should be a nationwide board, with the same composition as the local ones but involving persons of national stature, to evolve review standards and clarify generic questions.

A project passed upon would be issued a certificate of approval. One would expect that the various government agencies, as well as foundations, would be quick to agree not to support unapproved studies. Prisons, schools, mental hospitals, and other institutions that have captive or underage populations would not allow unapproved researchers access to their populations. Authorities of such institutions would thus back up standards formulated by the scientific community, rather than set standards themselves. The few investigators who would continue to conduct unapproved research would soon find themselves cut off from the scientific community and from sources of reputation and legitimation and their work branded as unethical. If the scientific community does not act, government regulations will and should follow.—AMITAI ETZIONI, *Professor of Sociology, Columbia University*, and *Director, Center for Policy Research, Inc., 475 Riverside Drive, New York 10027*

For additional material, see B. Barber, J. Lally, J. L. Makarushka, D. Sullivan, *Research on Human Subjects* (Russell Sage Foundation, New York, 1973); J. Katz, Ed., *Experimentation with Human Beings* (Russell Sage Foundation, New York, 1972); A. Etzioni, *Genetic Fix* (Macmillan, New York, 1973).

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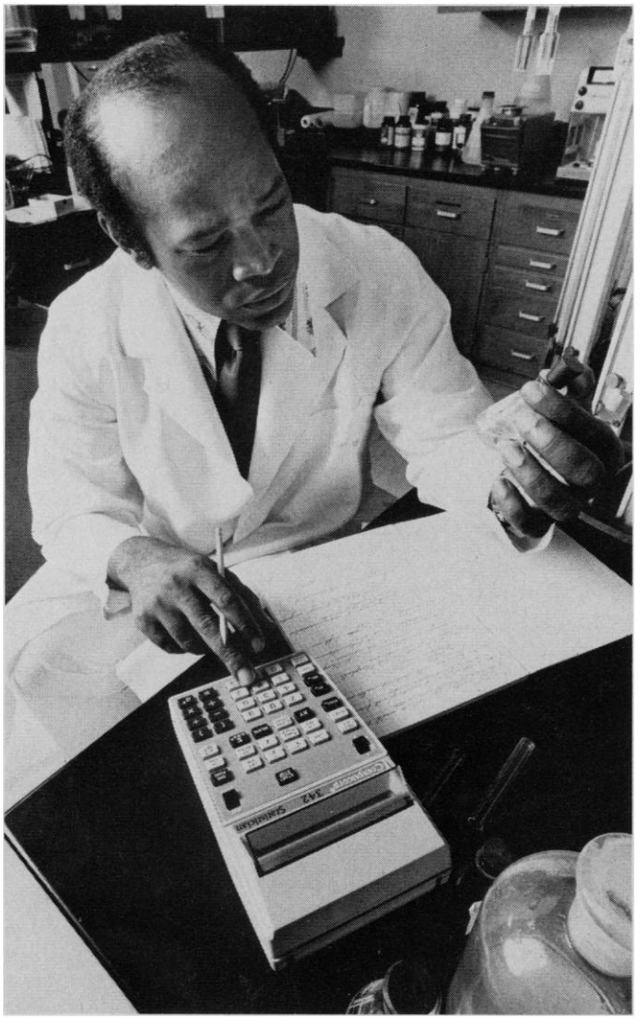

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