The Role of Basic Research in Universities

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At a time when government and society are reassessing national priorities, and support for basic research is uncertain, a clear definition of the role of basic research in universities is of vital concern. It is therefore appropriate that we examine university goals and the contribution of basic research toward attaining those goals. Arguments are put forth below to the effect that education is the only proper endeavor for a university, and the prime role for research in a university is as the principal tool for graduate education. It is the education of men and women, who through research learn to think creatively and imaginatively, that justifies a significant university involvement in basic research.

Basic research has often been defended on the ground that it leads to or supports practical developments that benefit society. This case has been stated strongly and clearly by *DuBridge* [1967] but the findings and implications of Project Hindsight [*Sherwin and Isenson*, 1967] appear to many to be at least as persuasive. The feeling that research expenditures should be justified in terms of identifiable benefits to society is well put by Congressman Craig Hosmer [*Hosmer*, 1968] who states: 'The science community should take greater pains to make clear that its efforts contribute directly and indirectly to progress benefiting every man, woman, and child in the country. The public will not buy science for science's sake—so sell it to them for their own sake. Public interest is in the human sciences, man as a living being and man in his environment. That is where the money will be. Therefore, adjust research priorities to the public's priorities to the extent possible. The public does not ask for a money-back guarantee if an idea fails, but it wants reasonable assurance of some visible benefits if it succeeds.'

It is not hard to provide such assurances for applied or 'relevant' research. However, providing reasonable assurance of some visible benefits to society from most basic research projects is difficult at best. How do we relate basic research to the problems that pervade today's public interest? What visible benefits can most basic research projects provide toward the solution of problems such as air and water pollution, poverty, integration, garbage disposal, the growing urban crime rate, the war in Vietnam, etc.? When one realizes that the expenditure for basic research in a typical university with a strong science-engineering graduate program is of the order of ten million dollars per year, it becomes clear that there is a lot of justifying to be done. As anyone who has tried can testify, the link between a specific basic-research project and a projected practical application is tortuous and often unconvincing. It is true that some undirected basic research does occasionally pay off in a practical way. However, most of the research conducted on a university campus does not.

The obvious truth is that people in a university normally undertake a specific basic research project because it interests them, not because it may prove to be of practical value. It is then difficult to provide, after the fact, a link with matters of practical interest.

We should question whether it is sound policy to continue justifying expenditures for basic research in terms of direct practical benefits. For example, *Hoyle* [1968] asks 'whether justifying ourselves with gadgets is really the way we should look on our relation with society. I think the policy is unsatisfactory because it is basically dishonest; we are not what we pretend to be; we are not in business as widget manufacturers.'

If not with gadgets, with what can the academic community best defend and justify the expenditure of public funds for basic research? The answer, I believe, is to look at research, both basic and applied, as means through which we achieve some valued goal. That is, even basic research should be considered as being applicable to a specific purpose. In a university, this purpose is the support of the educational objectives of the institution. With applied (or relevant) research, the practical benefit is usually easily identified. However, the assumption that the results of basic research, which have often turned out to be of practical value in the past, will continue to be so in the indefinite future is no longer widely accepted. Furthermore, this assumption may no longer be valid. The nature of both contemporary society and of science and technology have changed markedly.

Basic and Applied Research

Basic research began to affect technology significantly in the period beginning around 1900 with the introduction of the results from basic chemistry research. Electromagnetism, solid-state physics, and nuclear physics followed with dramatic impacts that have been documented repeatedly and convincingly. Thus, starting around 1900, we see that basic research did indeed produce a rapid series of benefits for society. It is fair to state that nearly all of the technological achievements of our society rest solidly on a foundation of basic research. However, applied research has evolved in a sophisticated way since the end of World War II and has taken the lead in providing material benefits to society. By applied research, I mean only that the research is directed toward some practical objective, even though that objective may not be clearly seen or immediately accessible.

An example of applied (or directed) research is the development of the transistor at the Bell Telephone Laboratories. In this case, the research management committed funds and manpower to the study of semiconductors with the thought that a better understanding of semiconductors would lead to new or improved devices for communications. The research was directed in the sense that the program had as its ultimate objective the development or improvement of communication devices. The techniques and the physical laws used in this research program were the same as those that would be used in a similar program of basic research that had no practical objectives in mind.

The difference between basic and applied research then seems largely to be whether a practical or a purely intellectual result is the conscious goal. Discovery of new laws of nature, which ultimately benefit mankind, is more likely to arise from basic than from applied research. However, fundamental discoveries that change basic physical laws are so rare that these events are hardly useful as either a distinction between basic and applied research or as a justification for support of basic research. Indeed, according to *Wheeler* [1968], 'Not since the quantum idea flowered into wave mechanics in 1925 has there been a change in fundamental principle. . . . Regularities, yes; beautiful symmetries, yes; but new laws, no. Not for [44] years!' He continues, 'No one in chemistry or biology feels himself cheated because the relevant physical laws are already known. There is challenge enough, and to spare, in unraveling fresh regularities and in finding new ways to put together old building blocks. So too in physics. And with each passing decade we understand the principles better because we have applied them to more issues. We believe in them all the more firmly because they have never let us down . . . not one single effect has been discovered which has led to a new law of physics, and not one single finding has ever been obtained which is generally recognized to be incompatible with existing law.' This is not to say that no new law will ever be discovered again; it is just that the interval between discoveries is long. The foundation of knowledge gained from both basic and applied research is necessary in order that fundamental discoveries can be made. Meanwhile, nearly all research is performed with heavy reliance on the laws of nature as we presently know them. Inconsistencies between the research results and these laws are, almost without exception, ascribed to error in the research. The difference between 'applied research' in industry and 'basic research' in universities is principally defined by the goals of the research, rather than by techniques or methods.

Urban Problems

Support for basic research has been weakened by the apparently sudden public realization that scientific and technological progress has not been all for the good. The problems of air and water pollution, the population explosion, the invasion of privacy, and the difficulties in urban transportation are examples of social ills that can be attributed to undesirable side-effects of both basic and applied research.

Because the role of basic research in universities is not well defined and because the difference between basic and applied research is often slight, a new pressure on universities is developing. That pressure is for the universities to drop or reduce basic research and to engage in research relevant to the social problems of our times: pollution, civic disorder, poverty, transportation, integration, etc. It is very unlikely that universities can be organized to work effectively with city, state, and federal governments for the solution of these operational problems. Universities are traditionally slow to react in an organizational sense to change, and, if universities were somehow restructured to handle such operational problems, their creative educational function would be seriously damaged. In order for universities to become directly and meaningfully involved in urban problems, they would undoubtedly have to organize interdisciplinary or interdepartmental research programs that would have to be directed to achieve the desired objectives on a set schedule and within a framework dictated by the operational requirements of government. There would be little room for basic, undirected research. If we go a step farther and ask what organizational and management structure would have to be placed on a university if it were to accept line-responsibility for operational problems, we can see that the least we should expect is the destruction of academic freedom as we now know it. Note that I am not arguing against university research, either basic or applied, that is relevant to social needs. The point I wish to make is that active participation of universities in the operational problems of government would be harmful to the educational function of the universities.

A close student-faculty relationship is essential to superior education. A professor distracted by extra-university matters can not contribute effectively to the demanding task of education. Already there may be cases of so great an involvement of university faculty in governmental and industrial problems that the educational environment on campus has suffered. If we consider the distractions, operational priorities, and necessary management activity that would be required to meet the operational needs of an interdisciplinary program of urban research, we can see that conditions on campus could become so extreme that students would be regarded as a hindrance rather than as a primary responsibility. This would be a tragedy, for while there are many varied organizations ready and able to work on the problems of society, there exists no organization other than the university to fill its educational needs. (The research activities themselves are not a distraction—research is the primary tool for graduate education, and in addition, research provides a form of self-education for the individual faculty member and a more stimulating environment for the student.)

Research and Graduate Education

If we accept the thesis that the university today is not organizationally equipped to become involved in operational problems and that it would be harmful if it were to become so, we should then ask, 'What is the function of the university in modern society?' The answer is, I believe, an obvious one: the proper function of the university is education. This answer need not be qualified or modified by the inclusion of other functions such as 'community service' or 'acquisition of new knowledge.' Education is, after all, a vital community service and acquisition of new knowledge is a necessary by-product of graduate education. Although it may not be possible to restrict university activities exclusively to education, those activities not supporting educational objectives should be kept to a minimum.

Even though universities may not contribute directly toward solutions to the several urban problems that presently trouble our society, they do indirectly, through their primary product—the educated citizen—contribute a great deal. *Solutions to the pressing problems of today (and tomorrow) will be provided by creative, innovative, and educated individuals.* While the university will not be the sole source of such individuals, it will certainly be the prime source. Thus, it is the output of the university, and not the university itself, that should be looked to for the solution to operational problems.

As the problems of society that grow from science and technology become more complex, the level of creativity and education required to deal successfully with these problems increases. If the level of creativity required is high, an undergraduate education will, in general, not be enough. A graduate education involving research is one obvious way to provide the additional education that is necessary.

It is useful to state here the difference between a graduate and an undergraduate education, and the relationship of research to graduate education. Following *Booker* [1963], we define the ideal undergraduate education as one in which the student learns how to understand and apply what is already known. The ideal education for the Ph.D., on the other hand, is one in which the student learns how to solve problems for which there are no known solutions. (It must be acknowledged that these ideals are not always met.)

The value to our society of educated citizens, who in Booker's words, 'have reasonable confidence in [their] ability to face what is novel and to continue doing so throughout life,' is obvious. The Ph.D. recipient has the flexibility and mental attitude necessary to recognize, attack, and solve problems that are unlike any ever solved before. Since many of the problems of society have their base in science and technology, we should look to the graduate programs in engineering and science for the trained manpower needed to solve these problems of our time.

A recent study by the *National Science Board* [1969] shows that the size of the nation's graduate education program is smaller than optimum. Their report states quite firmly that 'it is not possible to produce too many highly educated people in the United States as long as appropriate educational standards are not sacrificed.' They further show that for the next few years, it is necessary that graduate education grow proportionally faster than undergraduate education in order to supply the nation's needs.

If we adopt the proposition that education is the only proper business for a university, the role of basic research in universities would be defined by its educational function, and an appropriate funding level would be established. The minimum level of research funding at a university would be set by the size of the graduate education program. In order that the programs be intellectually stimulating and of high quality, the research on which these programs are based should demonstrate such qualities. Research should continue to be judged on national standards by peer groups so that research excellence (and therefore educational excellence) is not sacrificed in order to turn out large numbers of Ph.D.'s at low cost. Thus the character and scope of university research need not change; it only need be recognized that the primary function of basic research in a university is the support of graduate education. Except for special cases, research programs that cannot attract graduate students should either be dropped, supported at a minimum level, or conducted in governmental or industrial laboratories. The precise fields of research need not be defined; it is necessary only that the research be effective in graduate education.

This last point bears on the problem of the relevance of modern education. The problem is an old one. An education that is relevant today may well be old-fashioned tomorrow. It is not possible to foretell the future so accurately that an educational program can be created that will cover the lifetime needs of a given individual. It is far more practical to have a broad educational program that enhances an individual's creative potential. For example, the discipline of a Ph.D. program in high-energy nuclear physics is quite satisfactory in this regard. While the training is not necessarily relevant to problems the Ph.D. recipient will tackle later in life, he has learned from his thesis work how to enter a field about which he knows little and, through diligence and organized effort, make a significant and original contribution. Having done it once, the Ph.D. recipient should feel that he can do it again in a different field. The best research is that which is effective in attracting and intellectually challenging the best minds of the nation. Their natural ability and their training, plus the realities of the market place, will take care of the problem of relevance.

Summary

It is difficult to see how the problems of society and government can be solved by direct university involvement. Rather, the universities should maintain, in a narrow sense, the concept that their only proper business is education. Community service and acquisition of knowledge are valued by-products of this primary mission. Graduate education can be used to provide society with a large number of people who are trained to think creatively, who can solve problems whose solutions cannot be looked up in a book. Universities are the only institutions that can provide this national resource on the scale required. The essential point is that the most creative talent available to the nation will flow from university research programs. The level of federal funding required to establish and maintain research programs to be used for graduate education should be tied closely to the number of able students wishing to obtain advanced degrees. Those research programs of high quality that are successful in graduate education should be the ones supported as the *minimum* program of basic research in a university.

Acknowledgment

I wish to thank D. D. Clayton, W. A. Fowler, R. C. Haymes, F. S. Johnson, R. F. Stebbings, and G. K. Walters for their helpful comments on earlier drafts of this paper.

REFERENCES

Booker, Henry G., University education and applied science, *Science, 141,* 486-576, 1963.

BuBridge, Lee A., University basic research, *Science, 157,* 648-650, 1967.

Hosmer, Craig. What ever happened to federal funds, *Phys. Today, 21,* 23-27, 1968.

Hoyle, Fred, Science, society, action, reaction, *Phys. Today, 21,* 148-149, 1968.

National Science Board, *Toward a Public Policy for Graduate Education in the Sciences, NSB-1;* and *Graduate Education: Parameters for Public Policy, NSB-2* (available from U. S. Government Printing Office, Washington, D. C. 20402, 1969).

Sherwin, C. W., and R. S. Isenson, Project hindsight, *Science, 156,* 1571-1577, 1967. (In this report, it was concluded from studies of case histories of the development of weapon systems that basic research contributed little to these development programs, but that directed research was extremely beneficial.)

Wheeler, John Archibald, Maria Sklodowska Curie: Copernicus of the world of the small, *Science, 160,* 1197-1200, 1968.

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International Geophysics

GLOBAL MONITORING PROGRAMS IN PERSPECTIVE

Ten years ago we completed the International Geophysical Year. It was a magnificent effort of worldwide geophysical observations, marked by the emergence of the first satellites. The IGY was also a landmark in organization. Under the leadership of Sydney Chapman, Lloyd Berkner, Joseph Kaplan and Marcel Nicolet, the IGY became a model in international cooperation for other programs that followed.

Now we are at the threshold of a pioneer effort to observe the atmosphere on a worldwide scale. The Global Atmospheric Research Program (GARP) is, of course, related to and the logical forerunner of a global observation system for a World Weather Watch, and will answer: 1) whether a World Weather Watch can be justified on the basis of long-term predictions; and 2) how to design the global observation system for a WWW in the most economical and effective manner. But even if there were to be no World Weather Watch, GARP by itself should give us a better understanding of the general circulation of the earth's atmosphere. In this context, understanding means being able to simulate the atmosphere on a computer; GARP will give us the data to determine whether this computer simulation is in fact a valid one, and enable us to improve numerical models for large-scale dynamics of the atmosphere.

Presented before the Symposium on Meteorological Observations and Instrumentation sponsored by the American Meteorological Society and the American Institute of Aeronautics and Astronautics, Washington, D. C., February 11, 1969.

Four Global Observing Programs

But GARP is not an isolated global observation program. There are three others that I would like to describe briefly, discuss their relationship to GARP, and discuss how they might interact and help each other.

The International Hydrological Decade. Its primary purposes are: (*a*) to make simultaneous observations throughout the entire world in order to obtain the information needed to understand the global hydrologic cycle, (*b*) to strengthen the scientific base for water use, management, and conservation, (*c*) to stimulate education and training in hydrology, and (*d*) to improve the ability of participating countries to cope with water problems.

It is important to account for all of the water of the earth, in the atmosphere, on the surface, and in the ground, in its gaseous and liquid form as well as in its solid form as snow and ice. This involves, of course, not only the current distribution of water but also a knowledge of exchange rates between different forms and different locations, so that the water distribution can be reasonably predicted

Forum

Revolution in Water Renovation

Within the past year or so, physical-chemical techniques of wastewater treatment have become sufficiently economical to augment conventional biological methods, and may even replace them if research and development lower cost further. These developments were brought about because of the need for more complete removal of organic wastes from municipal sewage, because of the need to remove inorganic nutrients that are causing eutrophication of lakes, and because of the need in many water-short places to produce drinking water at a reasonable cost. These new techniques may revolutionize concepts of water management and could affect capital investments involving tens of billions of dollars.

We have come a long way from the outdated and unacceptable concept that 'the solution to pollution is dilution,' when pollution problems were solved simply by carrying the wastes away with plenty of water, and natural processes could absorb the pollution input. Even so, we have not moved far, most waste treatment processes now being used were in development over the last fifty years. Biological or 'secondary' treatment basically accelerates the natural processes through which bacteria oxidize organic material, converting it into CO_2 and its inorganic components. Removing most of the oxygen-demanding material (BOD) before discharging the effluent keeps the dissolved oxygen in streams from being seriously depleted. This in turn assures that the stream will not become anaerobic, that fish will not die, and that the recreational and aesthetic value of the stream will be maintained. But with increasing population concentrations, even an 80% to 90% BOD removal may not be good enough. And, of course, biological processes do not effectively remove inorganic nutrients, which lead to the growth of noxious algae and weeds, especially in lakes and estuaries.

Advanced waste treatment, sometimes referred to as 'tertiary' or 'physical-chemical' has suddenly moved from the research laboratory into the pilot plant and then into full-scale plants both in the U.S. and abroad. A 7.5 million-gallon-per-day plant at Lake Tahoe removes most phosphorus, nitrogen, and 99% of the BOD. Complete renovation has been achieved on a pilot plant scale at the Blue Plains Plant in Washington, D. C., and further work holds out the prospect of swimming in the now-polluted Potomac River. Since March 1969, Windhoek, the capital of South West Africa, has obtained drinking water directly from reclaimed wastewater in a process which still incorporates biological steps. However, pilot plants operated by the Interior Department and in South Africa have been producing highest-quality water directly from sewage which has not undergone any biological treatment. The results have been spectacular: Removal of essentially all phosphorus and nitrogen, removal of bacteria, and removal or deactivation of viruses. The process steps consist of chemical clarification with a metallic hydroxide coagulant, preferably hydrated lime which is later recalcined. The pH increases to 11.5 or higher. Essentially all phosphorus precipitates, along with most organics, particulate impurities, and bacteria. The nitrogen, now in the highly-reduced ammonia form, is 'stripped' by blowing air through the water. This is followed by stabilization, by sand filters, chlorination, activated carbon filter, and by ozone treatment if complete organics removal should be required. One of the remarkable features is the removal of all nitrogen; in the absence of the aerobic biological step, no nitrates are formed.

Modern process engineering methods are reducing the cost of full physical-chemical wastewater treatment to levels which, in some areas, would be competitive with the cost of conventional waste treatment plus the cost of drinking water supply. In turn, this opens new vistas for water management, especially for urban water systems. The bottleneck may lie in the transmittal of these new results to state and municipal administrators, to consulting engineers and plant designers, and to public health authorities.

Hydrologists, and geophysicists generally, have an important role to play, not only as scientists but as active communicators.

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Professional Communication

Michael Church's contribution to 'Forum' in the October issue of EOS is another in a long series of sincere requests on the part of scholars for some courage in the professional and learned societies to break the bonds that are binding us in our information system.

I would like to see the AGU break these bonds soon by doing precisely what Michael Church suggests: insist that each author submitting a paper for publication submits an abstract of the contents and conclusions that conforms to a fairly rigid specification to be laid down by the editor. These abstracts would then be published in a reunited *JGR* along with one or two articles or papers selected because of their merit that would appear in full length. I differ from Michael Church in my suggestion that the abstracts be printed in a reunited *JGR* because I think EOS has a special role to play, which it is doing very well, and should not be expected to participate in this particular venture.

With regard to the supply of reprints of the complete paper for those interested, this I believe is well within economic limits now, particularly if one bases the production of these short runs on the advanced developments of several firms. A member could be entitled to ask for a given number of complete texts in any one membership year without payment, and in excess of that there could be a nominal charge of one dollar regardless of the length of the articles so that the accounting would be kept simple. The only financial judgement then would be to estimate the number of free copies that could be permitted within the present fee structure.

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Research and the University

In the September 1969 issue of EOS, A.J. Dessler argued that the federal government should fund any research program to be used for graduate education depending on the number of able gradu-

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Sept. 22–Oct. 1 Symposium on the Development and Utilization of Geothermal Resources, United Nations, Pisa, Italy. *Contact:* Geoffrey R. Robson, Technical Secretary, United Nations Geothermal Symposium, United Nations, New York, N. Y. 10017.

Nov. 8–12 Fortieth Annual International Meeting of the Society of Exploration Geophysicists, New Orleans, La. *Contact:* SEG, P.O. Box 3098, Tulsa, Okla. 74101.

Dec. 1–8 **International Symposium on the Results of Research on Representative and Experimental Basins,** IASH, Unesco, and the Royal Society of New Zealand, Victoria Univ. of Welllington, New Zealand. Complete papers are due by Mar. 31, 1970. *U.S. Contact:* R. F. Hadley, U.S. Geological Survey, Water Resources Division, Federal Center, Denver, Colo. 80225. (see Nov. 1969 NEWS section)

Dec. 6–11 Second International Air Pollution Control Conference of the International Union of Air Pollution Prevention Associations, Wash., D. C. *Contact:* Arthur C. Stern, Dept. of Environmental Sciences and Engineering, School of Public Health, Univ. of North Carolina, P.O. Box 630, Chapel Hill, N. C. 27514.

Dec. 7–10 **National Fall Meeting of the American Geophysical Union,** Jack Tar Hotel, San Francisco, Calif. *Contact:* AGU, 2100 Pennsylvania Ave., N. W., Wash., D. C. 20037.

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Apr. 12–16 **Fifty-Second Annual Meeting of the American Geophysical Union,** Sheraton Park Hotel, Wash., D. C. *Contact:* AGU, 2100 Pennsylvania Ave., N.W., Wash., D.C. 20037.

Aug. 2–14 **Fifteenth General Assembly of the IUGG,** Moscow, USSR. (see Feb. INTERNATIONAL GEOPHYSICS)

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Structural features of mass aggregations of jellyfish, E. A. Zelickman, 8 pp.

The horizontal distribution of phytoplankton in the Gulf of Mexico, V. V. Zernova, 12 pp.

Use of shipborne radar for the determination of wave parameters, V. V. Dremlyng, 4 pp.

The modeling of sea waves by digital computer, K. Ya. Shvetsov and A. N. Shorin, 8 pp.

Instrument for recording aerodynamic pressure, A. P. Kerstner, 5 pp.

Experience in statistical processing of bottom topography data on the second cruise of the research vessel *Akademik Kurchatov,* G. V. Agapova and V. F. Kanayev, 6 pp.

Radiocarbon determintation of zooplankton production, E. A. Shushkina and Yu. I. Sorokin, 8 pp.

Plenary meeting of the 'Atlantic Ocean and Baltic Sea' basin division, 2 pp.

A catamaran for research in Chesapeake Bay, 4 pp.

A record descent (News from abroad), 1pp.

The changed name and extended scope of the Liverpool Tidal Institute, 1 pp.

On the 70th birthday of academician Ye. M. Kreps, 2 pp.

On the 70th birthday of R. Ya. Knaps, 2pp.

REVIEW

The vertical distribution of oceanic zooplankton, M. Ye. Vinogradov, 2 pp.

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ate students wishing to enter this field. This is a highly unbalanced position; it is untenable for two main reasons:

1) Many prospective graduate students are undecided as to their exact orientation. If, for instance, they are physicists, some may turn to oceanography, interplanetary physics, or solid-state geophysics not because of an inward motivation, but because of external circumstances. Thus, even if Dessler's funding criterion were valid, its application would be somewhat restricted. One can plausibly argue that there is no simpler way to orient these students than by a funding geared to the national priorities. Incidentally, Dessler's funding criterion has not been used much recently; the N.S.F., for example, allocates determined amounts of research funds and a determined number of fellowships to different fields of science.

2) In many fields, research programs have become very expensive. The government is under no evident obligation to spend funds for more graduate programs in these fields, even if they are all excellent. (How many 80 inch telescopes and linear accelerators should we build?)

I would further question Dessler's simple position that 'the proper function of the university is education' and that 'The precise fields of research need not be defined; it is necessary only that the research be effective in graduate education.' This position refuses to take cognizance of the problem of establishing priorities among different fields competing for limited funds, e.g., should one establish a department of music or one of oceanography? Historically this position has not been adopted very often either; as G.J. McLindon has noted in the *L.S.U. Alumni News* (Vol. 45, 1969), 'There are some who still feel that universities should concentrate on pure education, this being something removed from the day-to-day concerns of society. In point of fact it has seldom been this way — degrees in science, engineering and business testify to this.'

Perhaps the most puzzling point of Dessler's paper is his thesis that a university should not become 'directly and meaningfully involved in urban problems,' because this would 'leave little room for basic research' and would eventually 'destroy academic freedom.' I suggest that the evidence at hand contradicts him: for instance, the problems of agriculture and of a rural society were directly and meaningfully tackled by the land-grant colleges, which did not suffer either consequence. If such was successfully done for rural problems, it appears scientifically unsound to predict that it cannot also be successfully done for urban problems. The many urban affairs programs now being started in universities will, in a very few years, yield experimental evidence on this point.

Despite some disclaimer, it seems that Dessler would, in fact, like to isolate the universities from the national needs. This is no more the answer than is the simplistic demand for immediate and total relevancy. How to balance these competing claims is, from the individual's point of view, a personal problem; from the society's point of view it is a political problem. As suggested by P.H. Abelson (Science, Vol. 165, 1969), scientists should help solve it by greater involvement in the community as well as by bringing politicians to the campus.

I would finally like to mention a point that Dessler does not touch, but which is of immediate importance, namely the source of the research support. I suggest that the government is far from monolithic. Consequently anyone who accepts a grant from a government agency is *by his deeds* supporting that agency. It is, for instance, inconsistent to work outside of the university to reduce the U. S. military expenditures and at the same time to accept research funds from the Department of Defense. Students are quick to note these inconsistencies; they often do not judge them kindly.

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Author's Reply

There is indeed a disagreement between Prof. DeBremaecker and myself with regard to our views of the proper role of the university in our society. While it should be clear from my paper that I am opposed to neither applied research nor land-grant colleges, it should be equally clear that I am concerned that the high degree of organization that would be required in order to become directly involved in operational solutions to urban problems could interfere with both the basic research programs and the educational functions of the university. Urban problems are much more complex than agricultural problems. It does not follow that success in dealing with the technical problems of agriculture implies success in dealing with the combined social-economic-technical nature of urban problems.

DeBremaecker makes the point that the allocation of research funds should not be completely decided by the recipients. Although I did not discuss this particular topic in my paper, I agree with him that the matter is complex. My point was that graduate education provides a basis for determining over-all financial requirements for a minimum level of financial support for basic research.

DeBremaecker evidently believes that universities have a direct role to play in the determination of government policies and actions. Perhaps the main source of his difficulty with my paper arises from this activist philosophy that causes him to read into my paper ideas that it does not explicitly contain. It is true that I believe education is the only proper endeavor for a university. I do not believe that the university should be used as a privileged sanctuary from which attacks are launched against government policies. These ideas are, to some degree, reflected in my paper, and they are apparently the ones with which DeBremaecker basically disagrees.

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Editing JGR—Space Physics

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Two years have passed since my responsibilities for editing the space physics section of the *Journal of Geophysical Research* were handed on to an able successor. During this period, my feeling that the membership of the American Geophysical Union wishes to know more regarding the editing of their journals has been reinforced. The various journals of the American Geophysical Union, along with the meetings it sponsors, provide the chief justification for the AGU's existence as a scientific organization. Because its journals are central to many aspects of the scientific endeavors of the AGU and its individual members, interest in the functions and responsibilities of the editors is wide and natural. It was therefore surprising to me to find that these functions and responsibilities are, to a large extent, poorly understood, perhaps, only for the simple reason that editors write little about the details of their editorial philosophy and their job.

The principal purpose of this article is to acquaint those who might be interested in journal matters with the views and the method of operation of at least one editor. I also offer this article with something of a spirit of evangelism. I would hope that other editors might write something of their views and experiences. Finally, I would hope that by dispelling some of the mystery the editorship might become a sought-after position, rather than one that tends to be shunned. The job of editor is interesting and rewarding, and it is neither unduly difficult nor time consuming.

The editorship is in many important aspects regarded differently by each individual editor. His views of the function of an editor and the role his journal serves within the scientific community influence the journal's development and its part in reporting and stimulating the progress of science. An editor gives a journal its style.

A striking example of an editor's influence on the character of an AGU journal and the society it serves is the capture of space physics for JGR and the AGU by Phillip Abelson. He assumed the duties of editor (with James A. Peoples) at the end of 1958. This was the exciting period when the first satellites were being launched and NASA was being formed. It was not at all clear at the outset which scientific organization would be the principal home for the space physics community or which journal would publish its research. Abelson aggressively recruited space physics papers for JGR. When he spotted such a paper in some other journal such as *Nature*, *Physical Review Letters*, or *Astrophysical Journal*, he would telephone the author and ask that they send their next publication to him. He promised, and he delivered, rapid publication for these papers. (In 1959 space physics papers in JGR were usually published within three months from the date of receipt.) Attention was called to these papers by the simple device of listing them first in the Table of Contents. At that time JGR was not split into sections, and all fields of research were published together. Thus, space physics got star billing. This preferred treatment and attendant rapid growth were, incidently, a cause of concern for scientists in some other disciplines, such as the hydrologists who established a separate journal, *Water Resources Research*, for their work.

By the beginning of 1960, because of Abelson's aggressive and perceptive actions, JGR was solidly identified with space physics. AGU took advantage of this fact and, under the leadership of Homer Newell and Robert Jastrow, formed a Section of Planetary Sciences to attract papers for the AGU meetings. (This section was later split into two sections: Planetology and Solar-Planetary Relationships.)

Methods of Operation

For purposes of discussion, it is convenient to identify three principal methods of handling submitted papers:

Method 1. The editor turns over each incoming paper to an Associate

Editor who is knowledgeable in the general field covered by the paper. The Associate Editor selects appropriate expert referees. He then makes a decision regarding publication on the basis of his and the referees' opinions regarding the paper. This decision is sent to the editor in the form of a recommendation that can then be passed on to the author. In this system, a heavy burden falls on the Associate Editors. These Associate Editors (sometimes referred to as 'super-referees') are usually reimbursed actual postage and telephone expenses, and are provided with a supply of JGR stationery.

Method 2. The editor himself contacts experts to serve as referees for a given paper. He acts almost solely on their advice. His decisions are the distillation of the recommendations of his referees. The editor tends to keep his personal opinions in the background while he seeks a consensus among the referees.

Method 3. The editor sets standards of acceptability for publication and attempts to judge each paper within those standards. He calls on reviewers as necessary to act as expert consultants to assist him in making a judgment. The editor assumes responsibility for the report sent to the author. Thus, he is not necessarily constrained by the opinions and the judgments of the reviewers.

There are strong and weak points related to each of the above methods of operation. An editor's choice depends in large measure on what he feels his relationship to the journal ought to be. I do not believe an editor would slavishly stick to one method; his selection often depends on circumstances and on the content of the paper being considered. I preferred the third method of operation. However, for more than half the papers submitted, I used the second because it did not require the high degree of expert knowledge that the third did, and also method two was less work for me. I never used the first method. The following discussion is therefore limited to my experiences with only these last two methods of operation.

A Journal's Philosophical Basis

There exists a wide range of views regarding the purpose of the scientific journal. Some argue that journals serve only an archival function and that the important research communications take place through informal channels such as at meetings or through preprints. Others argue that journals should play the role of science newspapers and strive to print everyone's research papers with a one-day lead time and on a daily publication schedule. Still others propose that the journal in its present form is finished; computer distribution of selected reprints, for example, is heralded as the new way.

The thinking of most editors follows more traditional lines. Yet, even within the tepid statement that the purpose of the scientific journal is the dissemination of the results of scientific research, different outlooks are possible. For example, should the journal publish only what is somehow carefully determined to be 'correct'? Or should it be, at the other extreme, a wide-open forum, publishing everything submitted, and let the reader beware?

A complete discussion of the possible alternatives that might provide the philosophical basis for an editor's method of operation is impractical. I propose to state here only my thoughts and conclusions on operational matters with the understanding that these ideas are certainly neither unique nor the ultimate answer to editorial philosophy.

Before taking on the responsibilities of the editorship, I visited several editors whose editorial style I admired greatly. The one who most influenced my thinking was S. Chandrasekhar, editor for over 20 years of the outstanding *Astrophysical Journal*. Much of the following demonstrates his influence.

An editor should be conscious of three separate elements of the scientific community he must deal with: the readers, the authors and the referees. The needs and desires of these three elements are frequently conflicting.

The readers of a journal must be regarded with sympathy. A journal is presumably printed to be read. Yet, some papers are written in such an opaque style that it is impossible to understand their point within the time most readers are willing to spend on them. The present volume of scientific literature makes it impractical for a reader to spend much of his time on any one paper. The reader is in trouble as soon as he tears the wrapper off his copy of the journal. He simply cannot read and understand more than a fraction of what is printed.

The authors, on the other hand, do have a right to publish their work. Their reputations as scientists, and hence their careers, are strongly affected both by their ability to publish and by the quality of their published work. Therefore, I feel it is important to somehow maintain the journal's standards without harassing the authors.

The referees should ideally be motivated by a desire to serve their scientific community. They receive little for their efforts aside from an opportunity to read a paper in their field a month or two before they would normally receive preprints. They may also feel an idealistic pleasure in having assisted an author to improve his paper or in having helped to prevent publication in the journal of a paper that would have perhaps embarrassed the author and led the reader to doubt the quality of the other papers published by JGR-Space Physics.

Journal Standards

The journal standards should be slightly higher than the prevailing scientific standards of the community that it serves. In this way the journal can apply continuous pressure toward improving the quality of the reporting of research results.

It is important that the standards be set neither too high nor too low. If the standards for publication were set too high many authors would become discouraged and turn away from JGR. The journal would quickly become cliquish, catering to a restricted segment of the scientific community who, forming a closed loop in which they act alternatively as author or referee, would effectively exclude papers they felt were 'incorrect.' Such a development would be bad even though high standards of sorts might be seemingly achieved. For one thing, the vigor provided by controversy and by new ideas from outside the clique would be largely missing. New ideas are rarely received with enthusiasm. The history of science is replete with stories of the virtual suppression of ideas that swam against the prevailing tide of contemporary scientific opinion.

Some might argue that such injustices and impediments to the rational search for truth could never arise today. Forget it. Unless the editor resolves to keep the journal an open forum, it will tend toward publication of ideas that are judged by the referees to be 'safe.'

Finally, an overly cautious publication policy would cause the journal to lose direct contact with many of those in the scientific community who could benefit from a constructive relationship with a journal that sought to help them improve the presentation of their research papers. Even now, direct editorial contact of JGR members is small. Less than 15% of the AGU membership publish in any section of JGR during a given year and only about 5% of the membership publishes in JGR-Space Physics. A further trend toward elitism in publication standards would achieve little in the way of short-range benefits and would probably be harmful to the long-range best interests of AGU and its membership.

At the other extreme is a policy of publishing, in an uncritical way, virtually everything submitted. Such a policy would lead to a rapid downward spiral in journal standards and an upward surge in volume of papers published. There would be a definite danger that the better authors would prefer to publish in more discriminating journals where their work would not be as apt to be lost in a vast sea of mediocre papers. Whatever refereeing was done would be of little value; a journal's standards are, after all, established by what it actually publishes. The referees would tend to write their reports in the light of what they saw actually appearing in the journal. Some might wish to write for an unrefereed journal, but few would find much satisfaction in reading one.

I adopted a guiding principle, therefore, that publication was not to be restricted to those ideas that pleased the referees or that fell in with majority opinion. I concluded that a purposeful effort should be made, within the constraints of journal standards, to publish each paper submitted. However, high standards of good scientific methodology and clarity of writing were to be maintained. A natural consequence of such a policy was the appearance of controversy, which had to be both encouraged and controlled.

Operational Procedures

The preceding discussion has been rather general. The following is intended to illustrate how these general principles were applied in daily operation. This discussion will utilize, as a rough framework, the steps that a paper passed through from initial receipt to eventual acceptance or rejection.

The first decision regarding new submissions was the suitability of their content for the space physics section of JGR. Papers submitted on subjects such as meteoritic composition or lunar structure, although space-related, were better suited to the solid and fluid earth section of JGR. Such papers were transferred immediately with no further action on our part other than a letter to the author notifying him of the transfer. Other papers, on such subjects as instrumentation or the collection of routine data with no analysis or interpretation, were returned to the author with a letter informing him of a general policy that prohibited consideration of such papers. In such cases, I would endeavor to refer the author to a suitable journal or data repository.

Referee Selection. The most critical step in the editorial process is, perhaps, the selection of a referee (or referees). The assistance of good referees is absolutely indispensible in the maintenance of journal standards.

I often tried to use only one referee per paper. There were several reasons for doing so. There was the simple matter of the sheer number of papers to be reviewed. Approximately fifty new papers and thirty-five revised papers were received each month. If each one were sent to two different referees (so no single referee had more than one paper a month), 170 referees would be tied up each month either refereeing new papers or rereading papers that they had refereed earlier and had recommended some modification. However, in my opinion, there are less than 100 first-class referees available to review papers for JGR–Space Physics. These same scientists are usually asked to review papers for other journals and to review research proposals for governmental agencies. One should not expect them to be able to maintain a high work level on JGR matters over an extended period of time. The careful reading of a paper of average complexity and the writing of a useful report must take a total of, say, at least 3 or 4 hours of a reviewer's time. This is a significant investment of professional effort.

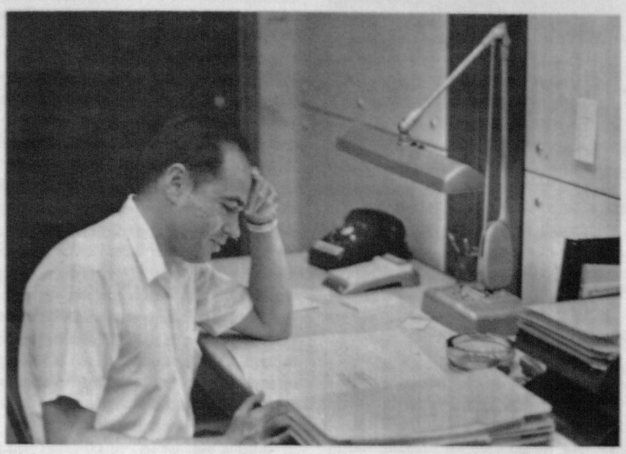
I thought I could detect a definite reluctance on the part of most referees to handle more than one paper per month. (One eminent scientist was willing to review papers only if I agreed not to send him more than one paper every six months–this arrangement worked out fine.) A referee who felt he was getting more papers than he had time for would sometimes react either by sending in his report late or by submitting a perfunctory report that showed an indifferent attitude. Or, he might just ask that he not be sent any more papers for a while. If one believes that referees should ideally be only the most knowledgeable, mature, and experienced segment of the scientific community, then one must be prepared to operate with a rather limited number. Since the advice of such referees was a precious resource, it seemed prudent to use it sparingly.

Also, if several referees are used on each paper the reviewing process is inevitably slowed. The editor is under some obligation to wait until the slowest referee responds before making a decision regarding a given paper. Additional time is usually required in the case of conflicting referees reports–one referee saying the paper is just great, the other referee saying it is the worst paper on the subject he has ever had the misfortune to read. (This actually happened to me on several occasions.) There are many ways to handle such a dispute, but all of them require additional time and effort and usually leave someone dissatisfied.

The intent then was to proceed with one referee per paper. This was practicable for only about half the papers. Sometimes, when a new referee was being used for the first time, I would pick an established referee in whom I had great confidence and use his report to 'calibrate' the efforts of the new referees. Or if a paper was on a subject I knew little about, or if the paper for some reason looked unusual, I would seek the advice of two referees, the thought being that two referees were twice as likely as one to catch some error or flaw.

Now to choose a referee. I would first read the abstract and perhaps the introduction and conclusion sections. From this I could judge whether the work was something new and exciting or of a more routine nature. I would try to determine whether or not the work was likely to be controversial. Also, I would look for such items as what appeared to be excessive length, or I would note that the name of a scientist who had contributed much to a field was missing from the reference list.

If a new phenomenon were being reported, little refereeing would be required other than the assurance that the paper was clearly written and that extravagant claims were not being made. If the paper was in a field with which I was sufficiently familiar, I might referee it myself. Alternatively I might ask someone relatively nearby to review it quickly. An example of such papers was the early work on the wake of the moon in the solar wind. These first papers were published within about 3 to 4 months from the date of initial receipt. (This is about two months faster than would occur with normal handling.)



A.J. Dessler looks over submitted manuscript.

If a paper were apt to provoke controversy, I would ask someone who might feel strongly about the paper to have a look at it. In such a case I would usually have a second, less involved referee also look at the paper. The report of the first referee would be an indication of the strength of the controversy, while the report of the second referee would be directed more toward the merits of the paper, or the lack of them. It is a good thing to have valid controversies fought out within the pages of the journal. However, they should be carefully stated, and to the point, so that protracted exchange does not clutter up the journal. The purpose of the refereeing in such cases was to assure that the arguments for one side were clearly and briefly put and that the existence of other points of view was acknowledged.

Papers submitted by distinguished senior authors called for yet another type of referee. I feel that a scientist who has been a recognized leader in a field of research over a span of years has earned some privileges. One of these privileges is the right to publish his papers without having it picked at on minor points (or even allegedly major ones). The editor must consider who is to be protected from what when a distinguished author sends a paper to JGR for publication. It is hard for me to be sympathetic with those who argue that the journal's reputation must be guarded against the possibility that a distinguished scientist has made a mistake. If he has made a mistake, it will no doubt be an interesting one. Furthermore, his reputation would suggest that he isn't very often seriously in error. Therefore, I had such papers reviewed only for clarity and perhaps checked for some obvious omission or random lapse that occasionally strikes all authors. This level of refereeing thus preserved JGR's position as a refereed journal while giving the distinguished author virtually an automatic acceptance for papers he submits.

For such papers, there was little point in asking the advice of one of JGR's senior referees. Therefore, I would either read over the paper myself or, most often, ask a graduate student to write a referee's report. The graduate student was usually flattered to be asked to serve as referee, and he would respond very quickly. Also, if the referee's report was not what I wanted, the graduate student's advice could be easily ignored, while the report of a senior referee could not.

Only one paper by a distinguished author was ever finally rejected for publication. None of the referees to whom the paper was sent would admit to understanding it. In desperation, I finally turned to the author and asked him to name several scientists who he thought could understand his paper. I chose two from his list and sent the paper to them. They were not asked whether or not the paper was correct, only whether or not they understood it. Neither one did, although one referee recommended publication anyway because he felt the work of this particular scientist would eventually be understood and found to be correct. The paper was rejected on the grounds that there was no point in taking up space in a journal for a paper that could not be understood.

To maintain a reasonably rapid publication schedule, it is necessary that the referees respond relatively quickly. Several steps were taken to avoid undue delays in the refereeing process. First, the referees selected were called by Marian Truax and asked if they would be willing to review a particular paper for JGR. About four out of five responded positively. Such a verbal agreement carried an implicit obligation of reasonable promptness. The referees were asked in covering correspondence to return the paper with their report within three weeks. Most referees complied with this request. The few who were late were called after about four weeks to find the cause of the delay. After five weeks, if no referees' reports had been received, the file on the paper was returned to my desk. Some action on my part was required at this point in order to maintain the publication speed of JGR.

The mean time from initial receipt to actual publication had been held down to about five months by Peoples and Abelson. I endeavored to match this schedule, which allowed only two months for the complete cycle of refereeing and copy editing. Therefore, after five weeks, I would usually give up on the tardy referee and send him a letter saying, in effect, 'never mind.' If the tardy referee was the only one selected, I would either review the paper myself, or I would enlist the aid of someone who would agree to review the paper immediately. If two referees had been selected and one of the referees had responded, I would usually make a decision based on the single referee's report.

Disposition of a Paper. Once the referees report was in hand, an editorial judgement was required. Usually a given paper fell easily into one of two categories: (1) a good paper that could be published either as it stood or with slight changes, and (2) an inadequate paper that would require substantial modification before it could be judged acceptable. About one paper in five did not fit either category; papers such as these absorbed more than their proportional share of editorial and refereeing effort.

A check list was sent to each referee (see box) along with the paper he was to review. The purpose of this list was to guide the referee toward consideration of the points that I regarded as primary in judging the acceptability of a paper for publication.

Marian Truax telephones a referee.

Rigid attempts to publish only what is correct may result in the publication of only what is popular. But clarity and conciseness are valuable and attainable goals. If a paper is in error, and it is short and clear, little damage is done (except perhaps to the author's reputation). It is the long, obscure papers that are to be avoided. Thus, point 1 on the check list was directed toward obtaining from the referee specific advice on parts of the paper that could well be deleted or sections that could perhaps be rewritten to be clearer and less wordy. This is not to say that length alone is necessarily a bad thing, or that shortness alone is a good thing. Some papers are justifiably long because, for example, they report several years of work in one paper rather than the customary series of papers. And as far as short papers go, Jim Peoples used to delight in pointing out that he had read papers where 'an author has gone on and on and on for a whole page.'

Another important point to consider is how the paper contributes to the work that may have been already published on the topic being treated. If the work fits in well with prevailing ideas, is it really new? For example, for the past several years an experimental paper providing more detailed observations of the Van Allen radiation belt, but yielding no new insight as to underlying mechanisms, would be unacceptable. This aspect of the referees' report is covered by point 2 of the check list.

If the work reported fits in well with related publications, then their proper acknowledgment will usually make the paper both shorter and easier to understand. On the other hand, if an author is marching to the sound of a different drummer, then his paper should be specific as to where, how, and why his work differs from the common view. If he can not do this (point 3 of the check list), there is probably some underlying fault with the paper, and it would be judged unacceptable. As an extreme example, one notes that crackpot work is virtually never securely founded on previous work.

About half of the referees' reports were modified, or even rewritten, before being transmitted to the author. The most common modification was the deletion of gratuitous remarks by the referee on the quality of the author's research. I remember one review that began, 'The only thing the author has proved in this paper is that he doesn't understand the subject.' Or, 'I am surprised that (scientist x), whom I previously held in high regard, could have written such an unforgivably bad paper.' Such mischievous or iniquitous remarks would certainly wound the feelings of most authors and even render some incapable of rational consideration of the balance of the referee's report. Also deleted were irrelevant offhand opinions such as other possible methods of attacking the problem. If an author is required to speak to all conceivable alternatives raised by the referee, the paper would grow in length but not in clarity.

If two referees' reports were at hand for a given paper, I would try to make them consistent, or perhaps I would combine them into a single report. The editor, having called on two (or more) experts for their opinion, must decide, when they differ strongly, whether to settle the matter himself, call on yet another expert referee to try to obtain a consensus, or exchange the referees' reports to see if they can come to some agreement. It would obviously do no good to bring the author into a disagreement among the referees; he would simply applaud the favorable report. I found that sending copies of conflicting referees' reports back and forth also accomplished little. The referees usually held to their initial positions. Bringing in another referee to serve as mediator was fine as far as the referees were concerned. However, two months or more might be required to reach a consensus. This could not be regarded as satisfactory form either the author's point of view or the journal's. The author should reasonably expect to receive an initial judgment regarding his paper within a month to six weeks after submission.

I believe a journal's reputation suffers if too much time is taken to make decisions regarding publication. If the time between initial receipt and ultimate publication becomes unduly long, the value of informal distributions by preprints will grow, and the journal will tend to acquire a tedious archival function rather than that of a primary means of scientific communication. Therefore, I usually chose to adjudicate conflicting referees reports myself. This course of action had the advantage of providing quick response, but it had the marked disadvantage of sometimes leaving one referee with the feeling that he was not appreciated; 'If you didn't want my opinion, why did you bother asking me?'

An interesting sort of difficulty arose about once a year because of the extensive revision of some referee's reports. Take, for example, the case where two conflicting referee's reports came in for a particular paper. One of the referees thought the paper was acceptable except for some minor points that could be easily corrected. The other referee was quite negative, expressing reservations on a fundamental point. My first reaction was that the critical referee had noticed something that the other referee had missed. Therefore, I combined the two referee's reports into a single report that listed the major objection of the critical referee and only the (minor) criticisms of the other. Thus, the combined report was very negative in tone. Unknown to me, the referee who wrote the favorable review sent a carbon copy of his report directly to the author. The author immediately recognized what had happened to the favorable review and sent a copy of the modified version directly to the referee that supported him. There was then a beneficial exchange of correspondence between me and this particular referee. He convinced me that the other referee's objections were unsound so the paper was accepted rather quickly; I convinced him that the editorial policy of modifying referees' reports usually worked well, and the policy was worth retaining. We parted friends.

Because the referees' reports were sent to the author as an editorial judgement, it was necessary that the anonymity of the referee be preserved. The authors generally felt that they were dealing with me rather than a faceless critic. I made it a point never to hide behind a referees'

CHECK LIST FOR REFEREES

of Typescripts Submitted to the

JOURNAL OF GEOPHYSICAL RESEARCH

The success of the Journal of Geophysical Research in the efficient dissemination of the results of sound scientific research depends, to a considerable extent, on the conscientious efforts of its referees. The referee plays the role of the expert consultant in matters related to maintaining reasonable standards of quality, clarity, and conciseness of presentation.

This check list is supplied to aid in the critical evaluation of papers submitted for publication. It is not necessary to answer all the questions. An adverse answer to any one of them reduces the importance of the others.

1. Is the paper well written? Are the results presented clearly and tersely? Are the assumptions, conclusions, and claims clearly and explicitly stated? Are any parts of the paper (text, tables, figures, intermediate mathematical operations) unnecessary? Does the paper contain unnecessary review material?

2. Does the paper contain enough new material to warrant publication in the Journal of Geophysical Research?

3. Does the author give proper credit to related work? Is this work placed in context with prevailing ideas or related work?

4. Is the abstract of the paper self-contained without being too long? Are the essential contents of the paper presented for maximum effectiveness in abstract journals?

If possible, have your comments typewritten on an unsigned sheet appropriate for transmittal to the author. If you have any comments you wish to communicate only to the editor, write them in a covering letter or enclose them within brackets in the body of your report. In the latter case, your report will be retyped with these items deleted.

Please return the typescript with your review within two or three weeks. If you find that you are unable to review the paper within this time period, please return it immediately so it can be sent to another referee.

report. For example, recommendations on whether or not a paper should be published were almost always deleted from the referee's report. (Note that the check list does not ask the referee for his opinion as to whether or not the paper should be published.)

The question of whether the identity of the referee should be kept from the author is an interesting one. Depending on circumstances and editorial style, a case can be made either way. (It has also been suggested that the author's name be removed from his paper before it is sent to the referee so that the anonymity is complete; supposedly this would remove any influence of status or personal relationship, either good or bad, that the author may have with the referee.) The editorial style I utilized required that the referees be anonymous. If the referee is to be regarded as an expert consultant whose advice can be accepted, modified, or discarded, according to the over-all judgement of the editor, the anonymity of the referee is essential. If, however, an editor wished to have the referees share in the process of deciding if and when a given paper was acceptable for publication, then he would probably not modify their reports, and a good case can be made for allowing the identity of the referee be known to the author. The method I used, which preserved the referees' anonymity had one significant disadvantage: the referees' sense of responsibility and status was minimized. An anonymous referee, perhaps overly burdened by other responsibilities, might be tempted to take advantage of his invisibility to do a less than craftsman-like job on his review. The referees also occasionally expressed some dissatisfaction in not knowing the outcome of their efforts on a given paper. However, the referees were, by and large, willing to perform responsibly as anonymous consultants. The attendant advantages of maintaining speed of publication were, in my opinion, compelling factors in the choice of policy regarding referees.

The most common form of criticism in a referee's report was that some mistake had been made in the paper, or that some basic fact had been overlooked. The mistake was rarely a simple algebraical or numerical one and was, therefore, debatable. If an author insisted on publication and would expose the difficulty clearly and concisely, the paper would usually be accepted for publication.

Because of the large volume of correspondence, form letters were used whenever possible to lighten the workload. There was a spectrum of 8 letters for the initial report to the author that ranged from outright acceptance to a curt rejection. Often paragraphs from different form letters were combined. Any additional or special information for the author was typed as a P.S. to the form letter.

One of my most common requests to an author, particularly during my last two years as editor, was that he reduce the length of his paper. A general request (e.g., 'Please shorten your paper by 20%.') did little good. The author would usually reply 'where?' A request to shorten a paper usually had to be accompanied by the identification of a specific part (e.g., 'Please delete the review material contained on pages 3–9.) An editorial on brevity (*J. Geophys. Res.*, *73*, 4133, 1968) was gratifyingly effective. I felt that the quality of the copy submitted to the journal improved steadily because of the pressures exerted by such efforts.

An author could respond in one of four ways to a critical review. (1) He could object strenuously to the referees' report. (One author defended his work by writing, 'The stupidity of the referee is only exceeded by that of the editor.') (2) He could argue that the referee had misunderstood his paper, put a clarifying explanation in his letter to me, and return the paper essentially unchanged. (3) He could modify the paper in an effort to answer the objections of the referee. (4) Finally, he could withdraw the paper from further consideration if either he agreed with the criticisms of the referee or if he felt the task of rewriting the paper to satisfy all the objections that had been raised was too arduous. In this latter case, the paper would usually appear later in some other journal, sometimes, surprisingly, with the modifications that had been requested.

If the author reacted as (1) above, I would try to pick some sentence, or even a phrase, from his letter that was not contentious, and reply only to that. The goal was to engage the author in a calm dialog. Once this was accomplished, progress could be made toward discussion of possible modification and further review of the paper. A reaction as (2) above was answered with the observation that the referee was well above the mean level of expertise of the JGR readership on the subject of the paper. If the referee missed the point, other readers would likely make the same mistake. The content of the author's letter of clarification should, therefore, be integrated into his paper. This suggestion was usually well received by the author and the subsequent modification generally resulted in immediate acceptance of the paper for publication. Reaction (3) will be discussed below; there was almost never a response to (4).

It is interesting that, although an effort was made to publish everything submitted, a full one-third of the papers submitted never appeared in the space physics section of JGR. Only 20% of the papers were ultimately rejected as being sub-standard. This final action was usually taken after at least two rounds of refereeing. The remaining 10% to 15% of the papers that never appeared were either transferred to another section of JGR or submitted to some other journal that was more appropriate to its content, or, more often, the paper was withdrawn voluntarily by the author after he recognized, on the weight of the referee's report, that it contained significant faults that should be removed before the paper was resubmitted for publication.

Approximately one-quarter of the papers submitted were accepted immediately on the basis of a favorable referee's report. The rest were sent back to the author with requests for varying degrees of modification; a few were rejected at this point. Those papers were generally returned to the author with the referee's report within about a month of the author's submission to the journal. The authors usually responded to the referee's report rather quickly, returning the paper with some modification or reply within two or three weeks. When the paper was returned, I endeavored to judge whether or not the author had substantially made the various changes that had been requested, or, if the objections were more serious, if he had altered the paper adequately to answer the difficulties that had been raised in the referee's report.

It was important that I be able to judge a fair number of these cases myself in order that the mean time of five months from initial receipt to ultimate publication be retained. Remember that three full months are required for the mechanical processes of typesetting, galley proofs, page proofs, and printing and mailing. If one month is required for the receipt of the initial referee's report and transmission to the author, and another three weeks are required for the author to respond, a second round with the referee would not be possible without extending the publication time to more than six months. In about two-thirds of the cases, I felt I was able to judge well enough whether or not the author had satisfied the points raised by the referees. If I felt that the author had met the referee more than half way, the paper was accepted immediately. If not, the paper was returned to the author for more work. The remaining papers (about 22%) were either returned to the original referee or were sent to a new referee for an independent appraisal.

The marginal cases, or those papers that would ultimately be rejected, were the most time-consuming of editorial judgement as well as involving the greatest amount of refereeing effort. The good papers breezed through with a minimum of editorial and refereeing manpower. A statistical description of the fate of papers submitted to JGR—Space Physics is given in Table 1.

Controversy. In general, with marginal papers, I would decide in favor of the author after I was convinced that the paper was reasonably short and the controversial parts were explicit and clear. It is an unfortunate fact that the most interesting science and the most heated controversies usually occur together in those fields of research where little definitive knowledge is yet available. I encouraged the development of debate on such papers by nurturing the Discussion and Reply formate. During the last two years of my editorship, hardly an issue of JGR-Space Physics would appear that would not have a critical discussion of some paper and usually an author's reply.

In order that running controversies not be carried out in the pages of JGR, the following procedure was established. The author of the paper that was criticized was given an opportunity to examine the criticisms and reply to them as he deemed appropriate. The Reply was then transmitted to the author of critical communication; on the strength of this Reply he could modify or amend his criticisms. The amended version was then transmitted to the original author for further reviewing. On the convergence of this process, a Discussion and a Reply were published as successive communication. The controversy was regarded as closed at that point as far as JGR was concerned.

The process of convergence somtimes took months as Discussion and Reply were transmitted between author and critic. The reader was eventually presented with a rather terse distillation of what sometimes amounted to many pages of private correspondence. It was rare that the Discussion and Reply led to any immediate resolution of the controversy. But controversy is healthy in an evolving field of research since it focuses attention on areas that require more work. The controversy would often be settled later by a third party who was inspired by the Discussion and Reply.

The Discussions were treated as an especially open forum. I can recall only two that were rejected. Many were withdrawn after the discussor realized that his criticism was faulty or invalid.

Copy Editing. The copy editing for JGR—Space Physics was done in my office by Dianne Drda. The work of the copy editor is demanding and exacting. Mrs. Drda read every work and looked at every punctuation mark of every paper that appeared in JGR—Space Physics twice. (Note: not once, but twice. How many issues of JGR have you read cover to cover, twice?) In the process of preparing papers for the printer, Mrs. Drda would alert me to faults that both the referee and I had overlooked. (For example, a conclusion section that was virtually word for

TABLE 1. Disposition of Papers Submitted to the Space Physics Section of JGR in 1968 in Per Cent (approximate) of Total Submitted

Submitted (about 50 per month)	100%
Not Appropriate—returned to author or transferred to other section of JGR	4%
To referee for first evaluation	96%
Accepted immediately on basis of first report	24%
Returned to author	72%
To referee for second evaluation	22%
Accepted on basis of revision or following second cycle of review and revision	43%
Rejected	20%
Withdrawn by author	9%

word the same as the abstract; a figure that either wasted space or had no scientific content.) Often, equations were rewritten to cut printing costs. In such cases, it was useful to have these equations checked over by a scientist to make sure that their meaning had not been changed. Similarly, when Mrs. Drda would rewrite sentences that were grammatically ambiguous, they could be checked immediately to make sure they still conveyed the author's intended meaning. Nearly all of the routine checking of such material was done by graduate students: first David Cummings and later Arthur Few. They performed this duty with an admirably conscientious and professional style.

Table of Contents. When all the galley proofs gor a given issure had been received from the printer, it was necessary to make up the Table of Contents for that issue. There are several editorial styles for arranging the papers in a Table of Contents. In some journals the papers are placed in chronological order. Jim Peoples used a system, relatively popular in geophysically oriented journals, listing first those papers that dealt with matters farthest from the earth; thus, the last papers listed dealt with the earth's interior. The obvious advantage of such mechanical systems is that they avoid any invidious comparisons of whose paper is listed nearer the top.

I chose to retain Abelson's policy of using the prerogative of the editor in making up a Table of Contents to highlight certain papers or certain subjects by placing the titles of these papers near the top where they are seen first by journal readers. The papers selected to be listed first were those that I thought were either most newsworthy or those that I thought would somehow benefit the journal. Newsworthy papers were those reporting unexpected experimental results or markedly new thèoretical ideas. The beneficial papers were usually those that reported on work that was most often published in some competitive journal. For example, JGR has always been weak on papers on the plasms physics of space and on cosmic-ray physics. The *Astrophysical Journal,* the *Physics of Fluids,* and the *Physical Review* usually publish the bulk of the most scholarly work on these subjects. I felt that it might be helpful to the status of both JGR and AGU for plasma physics and cosmic-ray physics papers to be listed near the top in the Table of Contents. Thus, it was not necessarily the papers that might be judged excellent that were listed first; it was the papers that served a public relations purpose of either (1) giving the journal an image of excitement by highlighting the really new work or (2) trying to attract or hold certain areas of research where JGR was weak by calling attention to those papers that we did publish in these selected areas.

JGR copy editor Diane Drda

Associate Editors

The principal role of the Associate Editors was to help formulate journal policy. Some Associate Editors did, of course, also serve as referees. However, the responsibilities of Associate Editor and referee were distinct and separate. I corresponded with the Associate Editors when ever I needed counsel on some contemplated policy change.

To keep the Associate Editors for space physics a reasonably cohesive group, it was necessary that their number be limited. Therefore, I asked that only four Associate Editors be appointed for JGR–Space Physics for each three-year term, so there would be a total of twelve for space physics. (Associate Editors are appointed by the President of AGU on the recommendation of each editor. There is no formal limit to their number.) Thus, limiting the number of Associate Editors enabled me to communicate with them more readily and, perhaps, raised the status of the appointment. Because the number was kept small, I usually would not recommend reappointment of an Associate Editor after his term had expired.

The Associate Editors made many valuable suggestions and contributed directly to the evolution of the journal. To list a few examples, the subtitle *Space Physics* for JGR arose from a plea by Don Williams (1967–1969) for more discipline identification in the title of the journal than was carried by the word 'geophysical.' John Simpson (1968–1970) pointed out the need for Letters to the Editor with abstracts. The Brief Report format was adopted to meet this need. Billy McCormac (1969–1971) was the most persuasive of the Associate Editors regarding the need for brevity in journal articles and in the opportunities for achieving it through suitable editorial policy. G.K. Walters (1967–1969) presented a compelling case for dropping the inclusive page numbers in the references as had been required by the journal style manual. (Only the beginning page number for each reference is now required.) Each such input from the Associate Editors contributed to the steady development of the excellence of the journal.

Some Observations and Concluding Remarks

Although the preceding discussion has outlined in some detail my views and method of operation of the editorship of the Space Physics section of JGR, it leaves many things unsaid. However, I am satisfied that the significant aspects of editorial philosophy and its implementation have been adequately set forth. While there are a dew (perhaps) hilarious stories of the editorship to be told, and some (no doubt) interesting case histories to be presented, these would add little to the purpose of this article. But there is one subjective topic that should be mentioned, that is, what are the rewards and the disadvantages that accompany the editorship. Any such evaluation is bound to be affected by the individual style, the goals, and even the personality of each editor. The reader should judge for himself the relative importantce of these various factors.

Disadvantages. The disadvantages of the editorship are straightforward, and they rather thrust themselves on you; one does not have to seek them out. First there is the matter of the time required to adequately meet the responsibilities and achieve the goals of the editorship. A useful rule of thumb is that the editor should be prepared to invest an average of about one-half to one hour for each paper submitted. Thus, for the fifty papers per month we received, and for the editorial style I employed, I put in between thirty and forty hours per month.

This is not an undue investment of time. However, the demand for this time is relentless. If I did not put in an hour every day, the work would begin to pile up on my desk: a never-ending flow of new papers with referees to be chosen, papers with referees' reports to be acted on, and authors' resubmissions to be judged. If I were out of my office for a few days, a seemingly enormous stack of file folders was on my desk waiting for me. (The stack grew, by actual measure, at the rate of two feet per week.) My absence from the office and the attendant accumulation of work on my desk was bad for three reasons: (1) In an effort to get caught up, I would sometimes perform my tasks with less than adequate attention to detail. Quality would sometimes be sacrificed. (2) Papers sitting on my desk were obviously being delayed, and the goal of prompt publication was being defeated. (3) The work load for my office staff would vary unacceptably. When I was out of the office they would soon run out of work. When I returned, they would be swamped. This is not the road to a smooth and effective operation. I tried having one of my colleagues on the Rice Space Science Department faculty fill in for me if I were going to be out of the office for more than a few days, but I was never completely satisfied with this sort of arrangement, and I eventually abandoned it. I felt that I could never stay away from the JGR office for more than a week or ten days; absences of even a few days were uncomfortable.

Another disadvantage that accompanies the editorship is the hostility that the editor must occasionally face. This was especially so for the style I employed of attempting to use the referees as consultants and taking responsibility for the referees' reports. There is, however, little doubt in my mind that this is a universal and unavoidable appendage of the editorship, no matter what the method of operation. While the occurrence of harshly critical outbursts were actually relatively rare, their very unpleasantness seemed to magnify them. Laudatory, supportive, or just encouraging letters are even rarer that the abusive letters, so an editor should not expect to be cheered onward in his work. (Chandrasekhar told me that he received a complementary letter once, and he was considering framing it.)

Rewards. Some of the advantages or rewards of the editorship are readily apparent. For example, as an editor of JGR I received an honorarium of $5000 per year. Once every three months my day would be brightened by a crisp check for $1250. Another reward is the prestige and peer recognition that accompanies the position of editor. For obvious reasons, neither of these points are talked about much by editors. (In fact, it is with some reluctance that I write them here.) However, these are factors that should be explicitly recognized.

For me, the greatest reward was the opportunity to accept the responsibility for a major journal and to see concrete results from my attempts to influence its stature in the world of science. I thought this could best be done by publishing the best quality scientific papers obtainable and publishing them on a time scale shorter than any competing journal could match. Although this was an ideal to be approached, never fully and consistently realized, it was a source of great and lasting satisfaction to organize and to influence the efforts of those associated with the journal to obtain some recognizable results.

I hope that most readers will agree that the rewards and staisfactions of the editorship by far outbalance the disadvantages. A term as editor is an interesting, satisfying, and rewarding experience. As some of our present editorships are vacated (virtually all AGU editors now have definite term appointments), prospective candidates should not be put off by tales of horror of the editor's life. I found that, as in any job, the editorship had good days and bad days, but most of them were good.

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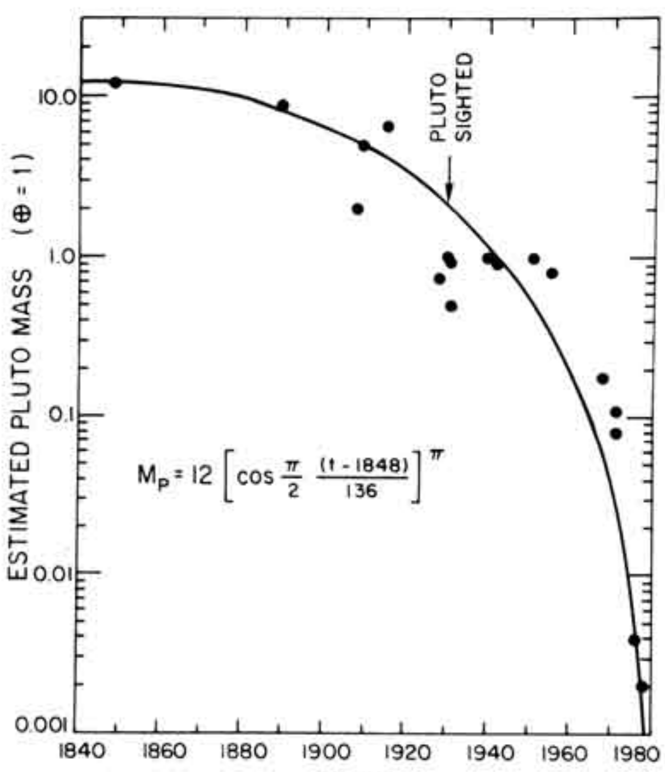
Forum

From the Ridiculous to the Sublime: The Pending Disappearance of Pluto

Pluto is so distant that it is difficult to learn much about it from direct observation. For example, starting more than 100 years ago, astronomers first postulated its existence and began estimating its mass by assuming it was responsible for observed perturbations of the orbits of Neptune and Uranus. Succeeding estimates of mass were made by the most eminent astronomers of the time; for example, estimates were made by astronomers such as Pickering, Lowell, Nicholson, Mayall, Eckert, Brouwer, and Clemence, with the latest estimate being made in 1978 by Christy and Harrington. At the recent meeting of the 50th Anniversary of the Discovery of Pluto, R. L. Duncombe and P. K. Seidelman assembled these earlier estimates of the mass of Pluto. We have plotted these (see figure), starting with the estimate by J. Babinet in 1848 which gave Pluto a mass 12 times that of Earth. The graph clearly illustrates that while Pluto was sighted in 1930, it was slighted in the 1970's.

Let us argue that these mass estimates should be taken seriously; it is difficult to ignore these many careful analyses made by so many eminent astronomers. We argue that they are not wildly in error; notice that the points are not scattered but follow a definite systematic trend. We are only prudent when we conclude that these earlier mass estimates are largely correct; we treat these data with the respect that the effort that went into obtaining them warrants.



Estimated mass of Pluto as a function of time. The dots are the experimental data; the equation is plotted as the solid line, which is the best-fit curve on which the theory is developed.

The consequence of following this chain of logic is to reach a most spectacular conclusion. The plot of mass versus time clearly indicates the impending disappearance of Pluto! The mass of Pluto as a function of time is fit by a cosine function raised to the pi power. It shows that Pluto's mass was first estimated when it was near its heaviest, and its mass has been dropping alarmingly during the past few years. As one might have guessed, it is scheduled to disappear in 1984, a year in which other ominous things are supposed to take place. This event may be welcomed by those of us who have been yearning for the 'good old (pre-Pluto) days' when planetary orbits were more circular; we will no longer have to tolerate Pluto's eccentricities. On the other hand, those of you interested in observing Pluto should hurry.

If we use our equation to extrapolate forward past 1984, we see that more interesting things are in store. After 1984, the cosine function is negative, and we all know that a negative number raised to an irrational power is Complex! That is, Pluto reappears, but with a complex mass. The real part of this complex number is negative. While this idea may seem repellant to some, Pluto will be repellant to everything at this point. The mass also has an imaginary part, but we can't imagine what effect this might have. Pluto will reappear as a real planet in 2256; this is a fortuitous time, for by then, the space shuttle will have become operational, and we will have the opportunity to institute a new planetary observation program by launching the Space Telescope. Pluto's mass will then be increasing rapidly until it once again reaches 12 Earth masses in the year 2392.

One can push mathematical extrapolations too far. Perhaps Pluto will not go negative; perhaps there is a physical explanation for this disappearing act. Velikovsky postulated that Venus was once a comet. (Despite claims to the contrary by reputable scientists, we can't prove Velikovsky wrong. While spacecraft have visited Venus, they have never visited a comet.) Pluto may be a comet also—a fresh one, since it was sighted for the first time only in 1930. We know fresh comets ablate as they approach the sun, for that is how cometary tails are born. Pluto has also been approaching the sun. It is now inside the orbit of Neptune, merrily evaporating away.

The National Aeronautics and Space Administration (NASA) is presently contemplating (which is a lot cheaper than planning, which in turn is much cheaper than building) a Halley Intercept Mission (Him). The rationale for going to Halley now despite the backlog of missions developing in the pipeline because of NASA's inability to get new missions approved (BMDPBNIGNMA) is that Halley will not return for 76 years. However, Pluto may never return! Even if we believe our conservative mathematical estimate (which conserves Pluto), Pluto will not become real again for 272 years, and who knows where it will reappear after being repellant for so long. NASA should redirect its priorities immediately and develop mission Ploto (Positively Last Opportunity to Observe) to Pluto. (We note that this name gets us off the hook if someone discovers a way to negatively observe Pluto. In the present environment, keeping off the hook has a certain intrinsic appeal to mission planners.)

In closing, we should emphasize that a few years ago astronomers would have said these early mass estimates were ridiculous. However, the present evidence suggests that Pluto is simply evaporating with time. Clearly, theories about Pluto have gone from the ridiculous to the sublime.

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EOS, vol. 61, page 690

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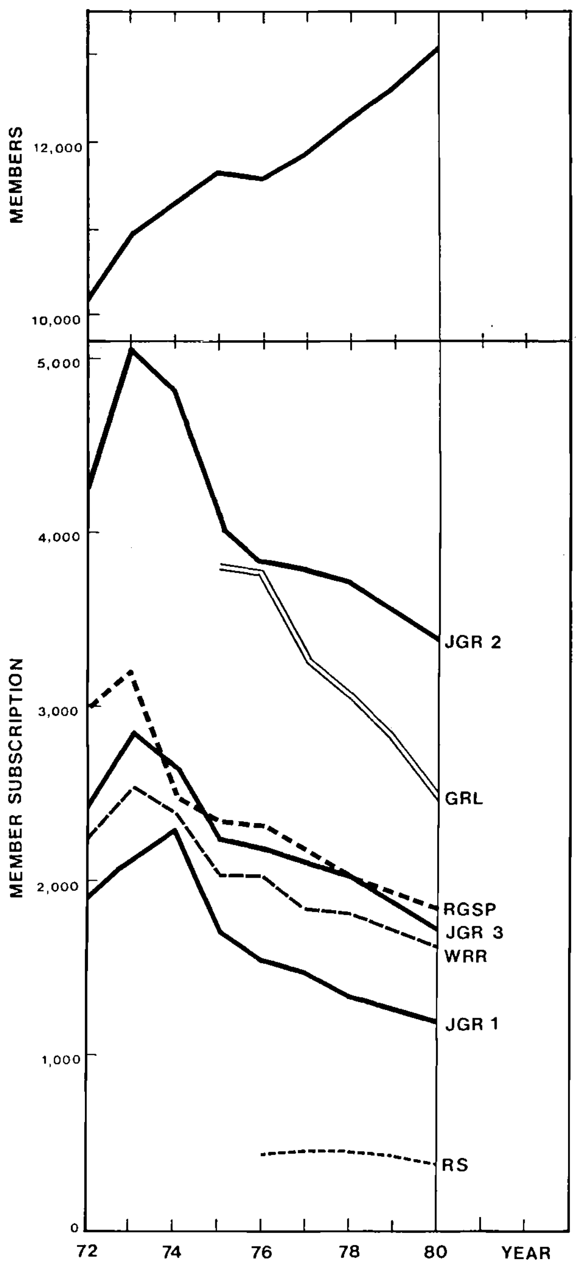
EOS, vol. 62, no. 19, May 12, 1981, pages 489, 490

Editorial

Member Subscriptions

The Publications Committee solicits comments and advice from the membership about the decline in member subscriptions to AGU journals. The phenomenon is illustrated below. During the period of this decline AGU membership has increased by several thousand, and there have also been marked increases in participation in annual meetings and in the numbers of papers submitted for publication. We therefore conclude that declining circulation is not due to a declining population of geophysicists or to decreasing research activity. What are the causes, and how can the trend be reversed?



An obvious hypothesis is that the decline results from increasing subscription rates. If this is true, what is the appropriate response? Prices to members reflect the costs of fulfilling member subscriptions and depend on the sizes of the journals. Lower prices can be charged for smaller journals. Should JGR be further subdivided? Should AGU establish new journals, more narrowly focused, and therefore potentially smaller, than those we already have? If so, to what extent should the subject matter of new journals be restricted to avoid competition with existing AGU journals?

Please let us hear from you on this or any other matter concerning AGU journals and books.

Publications Committee
James C. G. Walker, Chairman
Thomas E. Graedel
Jurate M. Landwehr
Peter H. Molnar
Bruce A. Taft
Donald L. Turcotte
Martin Walt

Forum

Pluto revisited

A. J. Dessler and C. T. Russell (*Eos*, Forum, October 28, 1980) are behind the times. Pluto already disappeared into Never-Neverland and has returned again! Dessler and Russell committed several blunders in their analysis that were further obfuscated by their failure to adhere to such fundamental AGU standards as showing error bars and publishing references.

Nevertheless, I have unearthed an old, dusty issue of *Science*, wherein one finds that *Ash et al.* [1971] report a value for Pluto's mass that probably accounts for the third last data point in Dessler and Russell's graph. But Ash et al's value reflects their assumption that the density is 3 gm/cm^3. They actually measured a negative mass.

You see, unlike the open-minded Dessler and Russell, Ash et al. were so biased in favor of a positive mass for Pluto that they discarded their own determination that the mass of Pluto is -0.081 (± 0.005) times the mass of Earth. Had Dessler and Russell included this definitive determination of Pluto's negative mass in their analysis (with or without error bars), they would have arrived at far different conclusions.

In particular they would have seen that Pluto's mass is actually increasing. Far from having to launch a PLOTO mission in the immediate future, we can proceed with the Halley Intercept and VOIR missions secure in the knowledge that Pluto will still be exhibiting accretionary behavior well into the next century.

References

Ash, M. E., I. I. Shapiro, and W. B. Smith. The system of planetary masses, *Science*, *174*, 551–556, 1971. (Readers should refer especially to pp. 554 and 555, as well as to footnote 37.)

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EOS, vol. 62, no. 19, May 12, 1981, pages 490, 494

I am astounded that scientists of the calibre of Dessler and Russell are able to arrive at such ludicrous interpretations of the data on the mass of Pluto as they have reported in the Forum in *Eos* on October 28, 1980. Clearly, the most consistent interpretation of the decrease by 4 orders of magnitude in the ratio of the mass of Pluto to that of the earth is that the earth is getting heavier.

This hypothesis also explains many other phenomena, such as my increasing difficulty in getting around as well as I did 20 years ago. Furthermore, NIAHOALMLTFAPTSTETOTSADP (NASA is a heck of a lot more likely to fund a program that studies the earth than one that studies a distant planet).

In closing, let me plead with you to publish this comment since my publication list this year is very thin (C. Russell, public communication, 1980).

Forrest Mozer
Professor of Physics
University of California, Berkeley

The elegant formula of the Pluto mass derived by Dessler and Russell (*Eos*, *61*(44), 690, 1980) reminds me of my conversation some years ago with my daughter, who was a physics senior at Rice. In explaining Buddhism incarnation, I introduced the imaginary time which changes the exponential function decaying with time (representing entropy or other quantity) into the circular function of time with the real and imaginary parts. I interpreted that both are existing, but only the real part is perceptible to human beings. She thought I became crazy. Well, how do you two gentlemen interpret your formula in terms of the realistic time which is complex, instead of the real time?

Takashi Ichiye
Professor, Texas A&M University

Russell freely admits to circular reasoning.—Ed.

In the light of President Reagan's attitude toward equal rights for women (not necessarily for the ERA!), perhaps NASA would fare better in its quest for comet funds if it were to accompany the proposal for the 'Halley Intercept Mission (Him)' by a Halley Exploration Report ( ).

James Hugh Nelson
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News

First Space Shuttle Payload

Preparations are being made at the Kennedy Space Center for installation of the first payload to be carried into space aboard the space shuttle *Columbia* during STS-2, its second test flight, now scheduled for this fall.

The payload is called OSTA-1 for NASA's Office of Space and Terrestrial Applications, which is providing most of the seven experiments. It is designed to demonstrate the space shuttle's capability as an operational space platform for scientific and applications research. The experiments are concerned primarily with remote sensing of land resources, atmospheric phenomena and ocean conditions.

The payload experiments include an imaging radar (Shuttle Imaging Radar, or SIR-A) to help test advanced techniques for mapping geological structures important in oil and gas exploration; a multispectral infrared radiometer (SMIRR) to measure the solar reflectance of mineral-bearing rock formations; a feature recognition system (Feature Identification and Location Experiment, or FILE) designed to discriminate between water, bare ground, vegetation, snow, or clouds, and thus control sensors to collect only wanted data; an air pollution measurement experiment (Measurement of Air Pollution from Satellites, or MAPS) designed to measure the distribution of carbon monoxide in the middle and upper troposphere (12–18-km altitude); an ocean color scanner (Ocean Color Experiment, or OCE) to map algae concentrations, which may indicate feeding areas for schools of fish or pinpoint possible pollution problems; a night and day optical survey of lightning storms (NOSL); and a biological engineering experiment (Heflex Bioengineering Test, or HBT) to determine the relationship between plant growth and moisture content in the near weightlessness of space.

An engineering model of a Spacelab pallet, a 3-m-long, U-shaped structure that mounts in the shuttle's cargo bay, will carry most of the experiments. The pallet is equipped with subsystems that provide power, command, data, and thermal interfaces for the instruments.

The imaging radar, radiometer, feature recognition, pollution measurement, and ocean scanner experiments are mounted on the pallet; the lightning and biological engineering experiments are mounted in the shuttle's crew compartment.

STS-2 will be launched from the Kennedy Space Center into a 280-km circular orbit with an inclination of 40.3°. For approximately 3.5 days (88 hours) of the 4-day mission the shuttle will be in an Earth-viewing orientation. In this attitude the shuttle payload bay faces Earth on a line perpendicular to Earth's surface. During this period, the instruments will be operated and data collected. The mission will conclude with a landing at Dryden Flight Research Center, Edwards, Calif.

The flight operations of OSTA-1 will be controlled from the Johnson Space Center. The air pollution and feature recognition experiments operate continuously for the whole mission with the imaging radar, radiometer, and ocean experiments taking data over preselected sites. The lightning experiment is a "target of opportunity" instrument. Experiment housekeeping data is available in the Payload Operation Control Center to monitor the status and health of the instruments, and the payload can be commanded from the control center or by the astronaut crew via the shuttle's general purpose computer.

Since most of the shuttle data transmission capability will be utilized with shuttle status data for the second orbital flight test mission, all the OSTA-1 scientific data will be recorded onboard on tape and film, which will be removed from the shuttle upon landing and turned over to the experimenters for immediate screening and analysis. The instruments will be removed from the *Columbia* after it is ferried to the Kennedy Space Center.

Forum

Implementing the Peer Review Process in AGU Publications

PAGE 770

Recently, *Russell and Reiff* [1984] presented a flow-diagram analysis of the AGU publication process indicating how publication delays naturally occur. Perhaps because of space limitations, their diagram did not include some important control statements. For example, according to their diagram, all manuscripts are either published or enter an endless loop. In fact, many papers end up elsewhere: As fish wrappers, in filing cabinets, or in non-AGU publications. (Accepted papers can end up in the same places, but they have the advantage of having been published in an AGU journal.) Significantly, the number of times the paper passes through the submission-refereeing loop (N_J) is not just journal dependent. N_J also depends inversely on n_D, the density of Dogma in the paper. We are concerned with the publication process also and are motivated by reports that N_J is unusually large in the case of certain distinguished colleagues, particularly when introducing new concepts or criticizing older approaches. Some suggestions are offered here to speed publication and consequently to assist in the smoother functioning of the scientific method in geophysics.

History provides numerous examples of the difficulty in publication of new ideas for example in astronomy [*Opik*, 1977], magnetic reconnection [*Dungey*, 1983], and field-aligned currents [*Dessler*, 1984]. *Oppenheimer* [1955] was well aware of such problems and reminded us of the need for moderation in his monograph *The Open Mind*:

> Science is novelty and change. When it closes, it dies. All history teaches us that these questions that we think the pressing ones will be transmuted before they are answered, that they will be replaced by others, and that the very process of discovery will shatter the concepts that we today use to describe our puzzlement.

Such an open-minded attitude seems to have been implemented in a practical way and to a surprising degree by *Dessler* [1972] in his tenure as editor of the space physics section of the *Journal of Geophysical Research* (JGR). *Dessler* [1972] felt that authors had a right to publish their work so long as it met standards of relevance, clarity, and brevity: "The authors, on the other hand, do have a right to publish their work. Their reputations as scientists, and hence their careers, are strongly affected both by their ability to publish and by the quality of their published work. Therefore, I feel it is important to somehow maintain the journal's standards without harassing the authors." Dessler warned that "Unless the editor resolves to keep the journal an open forum, it will tend toward publication of ideas that are judged by the referees to be 'safe.'"

Dessler resolved not to limit publication to ideas that "pleased the referees or that fell in with the majority opinion." Dessler frequently accepted well-written papers that infuriated some referees and welcomed the controversy and comments that naturally ensued. He often used only one referee to speed the review process. Most significantly, Dessler never asked the referee for his opinion as to whether or not the paper should be published. Dessler asked the referee four questions which I have paraphrased: Is the paper well written? Does it contain new material? Is proper credit given to related work? Is the abstract appropriate?

By contrast, a referee for JGR -A is now asked first whether a paper is fully acceptable, basically acceptable with minor revision, basically acceptable but requires important revision, may be acceptable after major revision, or is unacceptable. "Acceptability" is nowhere defined. It might be related to the Information For Reviewers, which appears on the reverse side of the Review Form. The Information for Reviewers contains guides for the completion of a written review and includes Dessler's four questions as well as seven others. The referee is asked to determine whether the research is "scientifically sound," is presented in a "responsible manner," and is told to note that the paper "need not agree. . . with your own view in order to be publishable." Assuming that the referee has read these instructions, one wonders how many referees can find "acceptable" views with which they cannot agree. Also, how often are scientific dogmas ever found unsound or irresponsible?

Under Dessler's editorship, in cases where a distinguished senior author submitted a paper it was usually reviewed by a graduate student because "the graduate student's advice could be easily ignored while the report of a senior referee could not. Only one paper by a distinguished author was ever finally rejected for publication." Thus it is clear that Dessler used the peer review procedure only as an advisory tool and not as the final determinant of what should be published. No referee can determine with certainty (on a scientific basis) what new idea will prevail in decades to come. Thus it is not in the best interest of science to give referees the power to make such a determination.

I have proposed to limit referee power by allowing an author to publish a disputed paper after he has heard the referee charges against it. At the same time, I have also proposed allowing the referee to publish his criticism of that paper. This proposal seems closely related to Dessler's procedure which led to rapid development of space physics and encouraged scientists to communicate in AGU publications.

References

Dessler, A. J., Editing JGR: Space physics, *Eos Trans. AGU*, *53*, 4, 1972.

Dessler, A. J., The evolution of arguments regarding the existence of field-aligned currents, in *Magnetospheric Currents*, *Geophys. Monogr.*, vol. 28, edited by T. Potemra, AGU, Washington, D.C., 1984.

Dungey, J. W., This week's citation classic: Interplanetary magnetic field and the auroral zones, *Current Contents*, *23*, 20, 1983.

Oppenheimer, J. R., *The Open Mind*, Simon and Schuster, N.Y., 1955.

Opik, E. J., About dogma in science, and other recollections of an astronomer, *Ann. Rev. Astron. Astrophys.*, *15*, 1, 1977.

Russell, C. T., and P. H. Reiff, Publication process, *Eos Trans. AGU*, *65*, 354, 1984.

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Referees and Controversy

P. J. Baum has broadened the discussion of the peer review process, particularly as it pertains to the space physics section of the *Journal of Geophysical Reearch* (JGR-A). The primary point raised by Baum is that the referees tend to be cautious with regard to the introduction of new ideas or ideas with which they do not agree. When asked to decide whether or not a paper should be published (rather than the decision being made alone and unambiguously by the journal Editor), referees tend to recommend against publication of papers they do not feel are both sound and safe.

This attitude on the part of the referees is, I feel, exacerbated by the present practice in JGR-A of identifying the referees at the end of the paper: "The Editor thanks referee A and referee B for their assistance in evaluating this paper." The advantage of this practice is clear: the referees are rewarded for their efforts by seeing their names in print and therefore are motivated to do a conscientious job when asked to review a paper. However, this practice has its negative aspects. By identifying the referees at the end of the paper, their status has been elevated nearly to that of a junior author or a junior editor. If a referee receives a paper whose author is marching to the sound of a different drummer, would he be willing to recommend it for publication, have his name placed at the end of the paper identifying him as a referee, and then listen to his colleagues say something like, "Why in the world did you ever let them publish that paper?" Instead, the referee of a controversial paper is most likely to recommend rejection. After several cycles of revision and rejection, the referee may finally give up and, in exasperation, ask that the Editor not reveal his name at the end of the paper. Thus, the present practice of JGR-A of identifying referees strengthens the natural inclination of referees to reject papers with which they personally disagree or that do not appear safe. I believe the practice of identifying referees should be discontinued.

I would also like to suggest a slight variant to Baum's suggestion that a controversial but clear paper be published and the referee be allowed to publish his criticism of it. Something like this was done in the late 1960's as can be seen by picking up almost any issue of JGR-A from that period. Once a paper was published, it was open to critical comment. Hardly an issue came out during my final years as Editor that did not have one or two critical comments on some earlier paper. Criticism thus went beyond publishing the criticisms of the referees. Critical comments were immediately accepted for publication and transmitted to the author of the paper being criticized to see if he wished to write a response. If the author replied, his reply was transmitted to the critic to see if he wished to revise his comments. No refereeing was involved at any stage. After a few rounds in private, a concise statement of criticism and defense was at hand, the ensuing comment and reply were published, and the matter was regarded as closed. Running controversy was not allowed. I felt that this part of the journal was one of the most entertaining, and it certainly enlivened the journal and contributed to the maintenance of high standards for publication.

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News

Passive French Drain

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Major environmental concerns of the low-level radioactive waste management operations at Oak Ridge National Laboratory (ORNL) are that the groundwater in this location is near the soil surface and that there is a possibility of water infiltrating the disposal trenches at old solid waste storage areas (SWSA's). In the current SWSA (SWSA 6), a group of trenches (49-Trench area) collect and hold water with seasonal fluctuations ranging from 1 to 2 m. This seasonal wetting of the buried waste has resulted in the movement of ^{90}Sr to a surface stream within the disposal area. To reduce infiltration and prevent waste leaching, the entire 0.44-ha 49-Trench area was sealed with a bentonite clay cover in October 1976. Subsequent monitoring indicated that the cover had not corrected the trench water problem, which suggested a faulty seal, an alternate recharge source, or both.

To improve isolation of the 49-Trench area from shallow subsurface flow originating in upgradient recharge areas, and to suppress the fluctuating groundwater, a French drain engineered barrier (see cover, this issue) was constructed in September 1983. The drain was installed in two sections having a design width, total length, and depth of 1 m, 252 m, and 9 m, respectively, and an expected water table drawdown of 2 to 3 m at the deepest point. Discharge for each section of the drain enters small ephemeral streams which drain surface water from the site. The drain was excavated, lined with filter fabric, backfilled with crushed stone, and covered with a 0.6-m layer of excavated material in 17 days at a total cost of $153,000 ($600/m of drain). Post-construction water level monitoring in wells throughout the 49-Trench area indicates that the drain has suppressed the groundwater to a level below the bottoms of the waste trenches (4.9 m) over approximately 50% of the disposal site (within a 60-m distance of the drain). In addition, five trenches have been completely dewatered and no longer become saturated during periods of heavy rainfall.

From an economic standpoint the passive French drain was judged to offer considerable cost savings over other remedial actions considered for the site (for comparison, rock-filled caissons, $682,000; slurry wall, $168,000; buried waste, $1,000,000). The drain requires no operation or maintenance costs, has achieved a maximum groundwater drawdown of 4 m in the northeast corner of the site where the two sections of the drain intersect, and shows promise as a future site stabilization technique for problem trenches in ORNL's solid waste disposal areas.

ORNL is operated by Martin Marietta Energy Systems, Inc., under contract DE-AC05-840R21400 with the U.S. Department of Energy.

This news item was contributed by E. C. Davis *and* R. G. Stansfield *of the Environmental Sciences Division of Oak Ridge National Laboratory, Oak Ridge, Tenn.*

Continental Drilling

PAGE 771

The National Science Foundation (NSF) now is considering a proposal to begin initial studies on a 10-km drill hole, deeper than any drilled in the United States to date, to be located in the southern Appalachians. Earlier this year a National Research Council (NRC) committee recommended that this area—a thin-crust overthrust region—should be a first priority if and when monies are made available for deep drilling projects.

If NSF accepts the proposal, funding, reportedly $2 million, will be made available to pinpoint the specific drill location and to develop the necessary base of regional information needed to conduct the drilling operations and scientific investigations. NSF is expected to reach a decision soon.

According to NSF, this proposal is but one of some $20 million worth of proposals submitted for deep drill projects. NSF currently has been allocated approximately $7 million in fiscal year 1985 for deep drill activities under the Continental Lithosphere program in NSF's earth sciences division. Leonard Johnson was recently appointed director of that program.

Although the concept for such a program was first developed in the early 1960's, this first deep drill project could be the beginning of what is envisioned as a long-term national program of continental research drilling to answer basic science questions. Three federal organizations—the Department of Energy (DOE), the U.S. Geological Survey (USGS), and the National Science Foundation—are participating jointly in the program, which is called the Continental Scientific Drilling Program (CSDP). The three organizations formalized their cooperation on April 2 when they signed an interagency accord (*Eos*, May 22, 1984, pg. 361). DOE has already conducted several drilling projects through its Office of Basic Energy Sciences.

Support in Washington for a national drilling program appears to be running at an all-time high. The White House Office of Science and Technology Policy (OSTP) has recently given the concept its endorsement and was instrumental in planting the seed money at NSF to begin preparatory studies. More recently, the Senate showed its support in the form of Senate Resolution 439, passed in the early morning hours of October 3 in the Senate's scramble to adjourn. In the resolution (see box), nine Republican and two Democratic senators—led by Senator Larry Pressler (R-South Dakota)—expressed their approval of a national program of scientific continental drilling. In an unusual move, George Keyworth, science advisor to President Ronald Reagan and director of OSTP, responded to the resolution with a personal statement of support. On October 10 the House of Representatives passed a similar resolution as an amendment to the Interior Department appropriations bill; this bill was signed into law by the President on October 12.

The impetus to begin deep drilling activities in the Appalachians is based largely on a report of NRC's Continental Scientific Drilling Committee (CSDC), which gave highest priority to drilling in the overthrust area of the southern Appalachians, a geologic area which extends through the Carolinas, Georgia, and Alabama. Given funding, according to CSDC, this drilling program could get underway in FY 85 with drilling operations beginning in FY 1986. A preliminary study suggested that it would take up to 3 years at a cost of $40 million for drilling alone. Scientific activities could add an additional $20 million.

According to the CSDC report "Priorities for a National Program of Continental Drill-

Upcoming Hearings in Congress

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The following hearings and markups have been tentatively scheduled for the coming weeks by the Senate and House of Representatives. Dates and times should be verified with the committee or subcommittee holding the hearing or markup; all offices on Capitol Hill may be reached by telephoning 202-224-3121. For guidelines on contacting a member of Congress, see *AGU's Guide to Legislative Information and Contacts* (*Eos*, August 28, 1984, p. 669).

April 23: Markup of legislation to reauthorize the **Clean Water Act** (S. 53, S. 652) by the Environmental Pollution Subcommittee of the Senate Environment and Public Works Committee. Room SD-406, Dirksen Building, 9:30 A.M.

April 25: Oversight hearing on **submerged lands** by the Public Lands Subcommittee of the House Interior and Insular Affairs Committee. Room to be announced, 9:45 A.M.

April 25 and 26: Hearings to consider financing associated with the Hazardous Response Trust Fund (**Superfund**) by the Senate Finance Committee. Room SD-215, Dirksen Building, 9:30 A.M.

April 30: Joint hearing on **global forecasting** by the Senate Environment and Public Works Committee and the Governmental Efficiency and District of Columbia Subcommittee of the Senate Governmental Affairs Committee. Room SD-342, Dirksen Building, 9:30 A.M.

May 1: Markup of legislation to reauthorize the **Clean Water Act** by the full Senate Environment and Public Works Committee. Room SD-406, Dirksen Building, 10 A.M.

May 2: Markup of legislation to amend the **Safe Drinking Water Act** by the Senate Environment and Public Works Committee. Room SD-406, Dirksen Building, 10 A.M.

May 3: Joint hearing on reauthorizing the **Coastal Zone Management Act** and the **National Oceanic and Atmospheric Administration ocean programs** by the National Ocean Policy Study and the Senate Commerce, Science, and Transportation Committee. Room SR-253, Russell Senate Office Building, 10 A.M.

Geophysicists

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William E. Sharp has been appointed the Program Director for Aeronomy in the National Science Foundation's Division of Atmospheric Sciences.

Two AGU members will receive the Distinguished Service Award, the highest award given by the Department of the Interior. ***Harold Masursky*** of the U.S. Geological Survey's geologic division in Flagstaff, Ariz., and ***Steven S. Oriel*** of the geologic division in Golden, Colo., are among the eight USGS employees to be presented with the award in ceremonies on April 24, 1985.

Forum

JGR Peer Review Suggestion

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I read with considerable interest the letters of P. J. Baum and A. J. Dessler (*Eos*, October 23, 1984, p. 770) concerning the peer review process in AGU publications. The first point that aroused my attention was the discussion of "distinguished" senior authors. Distinguished in this context presumably has a definition such as "a person who used a napkin, at least once, in the presence of the editor" or a similarly objective basis. Baum noted that only one paper by a distinguished author was rejected during Dessler's editorship. I assume that any editor could make the same claim by simply redefining who is, or is not, distinguished. As a matter of fact, it seems as good a definition as any to label an author as undistinguished if he has had a paper rejected by the *Journal of Geophysical Research* (JGR).

The real heart of the Baum and Dessler letters was the issue of publication of innovative but unconventional ideas. In support of this, consider the large number of innovative (not to say unwashed or harebrained) ideas presented at the annual AGU meetings (where practically anything is accepted). Then compare these with the published versions of the same material in JGR. Clearly, the referees have taken a huge toll. The published material is usually more conventional, more pedestrian, and often as not, more correct.

Let us assume for the moment, however, that there is this large body of unpublished papers out there which has been rejected by Neanderthal referees. I say let's do something about it! I suggest that all of these brilliant, creative, earthshaking papers be collected into a special JGR issue each year. The advantages of this approach are legion. Among the benefits are the following:

- Students would be able to obtain truly exciting research ideas more easily and could avoid wading through the boring, uninspired tripe that presently appears in the journals.
- The extremely busy "distinguished" scientists would not have to waste their time responding to incompetent and unimaginative referees.
- The uninspired conventional authors presently being published could more readily see the error of their ways and thus develop more innovative personas.
- The Nobel Prize selection committee would only have to read this one issue of JGR in order to identify the *really* exciting stuff going on in the geosciences.

Obviously, this suggested approach might only work for a year or two. Very quickly, everyone would be vying for publication in this special high-profile issue. Thus the "Nobel" issue of the JGR would get completely out of hand in terms of size, and some new solution would have to be found.

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Los Alamos National Laboratory,
Los Alamos, N. M.

Recent Ph.D.s

Seismology

Seismic strain rates and the state of tectonic stress in the southern California region, **Weishi Huang**, California Institute of Technology, Pasadena, Hiroo Kanamori and Leon Silver, June 1995.

Part I: Broadband modeling of aftershocks from the Joshua Tree Landers and Big Bear sequences, southern California. Part II: Characteristics of the June 28, 1992, Big Bear mainshock from TERRAscope data: Evidence for a multiple event source, California, **Laura E. Jones**, California Institute of Technology, Pasadena, Donald V. Helmberger, June 1995.

Three-dimensional seismic velocity structure of the Earth's outermost core and mantle, **Monica D. Kohler**, California Institute of Technology, Pasadena, Don L. Anderson, June 1995.

Part I: Near-source acoustic coupling between the atmosphere and the solid Earth during volcanic eruptions. Part II: Near-field normal mode amplitude anomalies of the Landers earthquake, **Shingo Watada**, California Institute of Technology, Pasadena, Hiroo Kanamori, June 1995.

Elimination of numerical dispersion in finite-difference modeling and migration by flux-corrected transport, **Tong Fei**, Colorado School of Mines, Golden, Ken Larner, May 1995.

Depth migration in transversely isotropic media with explicit operators, **Omar Uzcategui**, Colorado School of Mines, Golden, Ken Larner, May 1995.

Migration velocity analysis, **Zhenyue Liu**, Center for Wave Phenomena, Department of Geophysics, Colorado School of Mines, Golden, Norman Bleistein, May 1995.

Planetology

Three-dimensional analysis of impact processes on planets, **Toshiko Takata**, California Institute of Technology, Pasadena, Thomas J. Ahrens, June 1995.

AGU

The Future Employment of Geophysicists

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The AGU Committee on Education and Human Resources is carefully following the current discussion about the job market for new and recent Ph.D.s. The Committee is aware that many recent Ph.D.s and Ph.D. students in geophysics are concerned about their career prospects, given the current imbalance between the number of people seeking permanent positions in their field and the number of such positions available. Moreover, many Ph.D.s find themselves unable to take advantage of opportunities outside of traditional careers.

Background

With the end of the Cold War the social contract between science and society is being rewritten. These changes are both global and systemic in nature. Therefore, the response of the scientific community must be equally far-reaching, involving significant reexamination of and possible changes in cultural structures and attitudes that have become ingrained in the scientific community since the end of World War II.

The job market for Ph.D.s has radically changed in the past decade. Two-thirds of new physics Ph.D.s now take a postdoctoral position compared to less than half in 1982, and as their postdocs conclude they find even fewer permanent positions. Because of this, over the last few years a new category of itinerant scientist has emerged. These individuals move every few years from temporary position to temporary position. At present, it is impossible to determine what the ultimate fate of these scientists will be, but it appears likely that some, perhaps many, will never hold a permanent position. It is essential to emphasize that this "Ph.D. overproduction" issue is not based on anecdotal evidence or the ranting of a few young Ph.D.s who cannot get jobs; rather, significant analytical evidence exists to support these contentions of a significant imbalance between the number of new Ph.D.s being graduated and the present and likely future capacity of the research job market and of a growing number of people in tenuous job situations [e.g., *Kirby and Czujko*, 1993; *Ellis*, 1993].

The current graduate education process implicitly perpetuates this situation. Traditionally, Ph.D. students are trained through an apprenticeship to a mentor. There is a tendency for both the mentor and the apprentice to expect that upon completion of the training, the student should "look like" the mentor, that is, find employment in academic research. When such employment is not found, both the new Ph.D. and the advisor may feel that the apprenticeship was a failure.

Response to Current Employment Situation

The Committee believes that the health of our science and society is best served by having well-trained Ph.D.s in industry, government, and teaching positions at all levels, as well as in academic research positions. We think it desirable that Ph.D.s in geophysics find fulfilling careers in all of these areas.

(1) We recognize that there now exists an imbalance between the number of persons seeking research jobs and the number of available research jobs. This imbalance is related to permanent structural changes in science, and does not appear to be a short-term, temporary effect. Thus the scientific community must adapt to the changed environment so that in the long-term the health of the geophysics community is secured.

(2) A timely reaction to this situation might include one or more of:

- informing prospective graduate students about the current and projected job market;
- reducing the number of Ph.D.s graduated per year;
- encouraging and preparing students for nontraditional (that is, nonresearch and nonacademic) career paths.

The ultimate goal of these changes would be to create an equilibrium situation in which there exists a balance between Ph.D.s produced and satisfying jobs available. We are not saying that every new Ph.D. should get a research or academic job. Instead, we believe that a reasonable fraction of new Ph.D.s should have the opportunity to pursue research; the rest should have the opportunity to pursue a fulfilling nontraditional career.

(3) Any response to this situation is likely to lead to a general reexamination of the cultural structure in which Ph.D.s are educated and science is performed.

The Committee invites all AGU members to consider how the culture in which Ph.D.s are trained influences attitudes toward and prospects of finding fulfilling employment. We suggest that all involved in the Ph.D. training process, including students and prospec- tive students, faculty, and research advisors begin a dialog exploring issues raised by the following questions.

Employment. What sort of employment does one expect a new Ph.D. to find? What has actually been found by recent graduates? Is the expectation in line with reality?

Attitude. Do graduate students feel entitled to a particular kind of job? What responsibility do mentors have toward job placement for their students? Are these feelings in harmony? Are they realistic?

Culture. Does the departmental and disciplinary culture encourage students to prepare for nonacademic careers? Can students participate in internships in industry or government? May they take courses in subjects such as public policy, law, business, education, engineering, etc.?

Social impact. What are the human costs of the current job situation? If young scientists must relocate a few times before getting a permanent position, what impact does this have on science, society, and the young scientists' lives? Are students aware of this situation? Is this discouraging bright people from entering science?

What should be done. What is the best way to achieve an equilibrium between Ph.D.s produced and jobs available? Is it "academic birth control," that is, reducing the number of Ph.D.s produced? Or is it expanding the cultural definition of a "good job" to include previously nontraditional careers in government, education, business, etc.? What changes in the Ph.D. curriculum would be required to make such careers more accessible? What effect would this have on science? Should we rely solely on market forces and informed judgement by prospective graduate students to bring about these changes, or should departments take more active steps?—*Prepared for the Committee by A. E. Dessler, NASA Goddard Space Flight Center, Greenbelt, Md.; W. Smith, National Oceanographic and Atmospheric Administration, Silver Spring, Md.; and R. Lopez, Department of Astronomy, University of Maryland, College Park*

References

Kirby, K., and R. Czujko, The physics job market: Bleak for young physicists, *Phys. Today, 46*, p. 22, Dec. 1993.

Ellis, S. D., Initial employment of physics doctorate recipients: Class of 1992, *Phys. Today, 46*, p. 29, Dec. 1993.

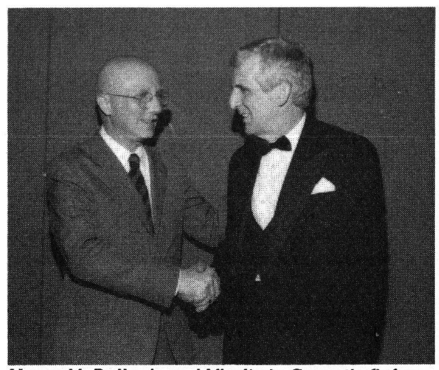
Citation for Vladimìr Cermák: 1995 AGU Flinn Award

PAGE 373

"Vladimìr Cermák, Director of the Geophysical Institute of the Czech Academy of Sciences in Prague, has for many years played a most remarkable role in bringing Earth scientists from the Eastern and Western Bloc countries together for scientific interactions. However anachronistic the concept of East and West political divisions may seem today, there are many who remember the nearly insuperable obstacles that prevented scientific exchange between those groups for decades prior to 1989. Vladimìr Cermák, through his organizing of small conferences and workshops in Czechoslovakia, accomplished the impossible. Through some extraordinarily deft diplomacy, Cermák obtained funding, secured visas, and mastered arcane currency regulations to enable small groups to meet in splendid castles and elegant country homes in rural Bohemia, facilities without urban distractions which had been placed under the custodianship of the Czech Academy of Science to serve as scientific retreats. Three meetings in the course of a decade stand out: at Liblice in 1982, and at Bechyne in 1987 and 1991, all dealing in general with heat flow and thermal aspects of lithospheric structure. These meetings were not just for prominent senior scientists, though of course many were in attendance. Of special significance were the opportunities for younger researchers to surmount the barriers that had been erected by forces well beyond the sphere of science. As one West German remarked as a graduate student in 1982, 'I remember well how impressed I was . . . to learn the details of the daily personal and scientific life of an east German colleague of my own age.' Cermák knew intuitively that the future belonged to the young, and he wanted to nurture their enthusiasm and stimulate their creativity.

"Cermák recognized that the greatest affliction of scientists in the Eastern Bloc countries was their isolation, not only from western colleagues and paradigms, but also from each other. He made great efforts to establish collaborations that would ease that isolation. From Bulgaria, Hungary, and Poland, from throughout the former Soviet Union, from Irkutsk, Novosibirsk, Archangel, and Ufa, places that are either literally or figuratively 'in Siberia,' colleagues acknowledge Vladimìr Cermák's remarkable efforts to draw them into the international scientific community. But the benefits were not unilateral; his efforts also provided Germans, French, Canadians, Japanese, Americans, and many others with an opportunity to gain insight into the eastern scientific world, and in some cases to develop collaborations.

"How did Cermák's remarkable career come to have this special dimension? Surely, a turning point came in 1968 when Soviet forces occupied Czechoslovakia, a fateful event observed by Cermák from the security of Canada, where he held a postdoctoral position at the Dominion Observatory, now the Earth Physics Branch of the Geological Survey of Canada. Many Czechs then abroad chose exile (as did many Hungarians in 1956), but Vladimìr and his wife chose to return to family, homeland, and a very uncertain future. His voluntary return persuaded authorities that he was 'reliable,' and thus he acquired a degree of freedom that enabled him to work in the interest of other colleagues, who were more strongly constrained by the political rigidities. His efforts, motivated initially by the situation in the socialist countries, soon became global, driven by the scientific foresight that decades ago led him to recognize that Earth science must be a fully international endeavor. Some of his seminal research in Canada on the reconstruction of climate changes from subsurface temperature records was truly farsighted, and provided a solid foundation for today's broad international effort addressing this topic in the context of global warming and its possible causes.



Henry N. Pollack and Vladimìr Cermák (left to right) (Photo provided by Joseph D. Weber)

"After 1989 and the literal crumbling of the walls of separation, one might imagine that Cermák might feel his special assignment had been completed and that he would focus his efforts more fully on a personal scientific agenda. However, that was not to be. He was chosen to restructure the Geophysical Institute in Prague in the difficult transition from the old to the new patterns of authority, scientific directions, integrity, and accountability. He brought the institute through very hard times, much leaner, but with new levels of scientific commitment and competence. In the environment of an open Europe he has continued his facilitating role by promoting through the European Geophysical Society and the European Union of Geosciences a strong, continent-wide Earth science community. He currently serves as Vice-President of the EGS and Vice-Chairman of the International Heat Flow Commission of the International Association of Seismology and Physics of the Earth's Interior.

"I am sure that all AGU members, if they had the opportunity to read through the letters submitted in support of the nomination from colleagues all over the world, would realize the heartfelt esteem in which Vladimìr Cermák is held by the international geothermal community for his efforts on their behalf, and on behalf of strong global science. His extraordinary career of research and service clearly epitomizes AGU's motto of 'Unselfish