

A lively, accurate and misleading view of the scientific community

THE POLITICS OF PURE SCIENCE.
By Daniel S. Greenberg. 303 pp.
New American Library, New York,
1967. \$7.95

by Raymond Bowers

Daniel S. Greenberg was, until recently, news editor of the magazine *Science* and is now its foreign editor. Those readers who enjoy his articles in *Science* will not be disappointed in this book; nor will his detractors be disappointed, for the book manages at one and the same time to be lively, interesting, penetrating, superficial, distorted, perceptive, accurate and misleading. The title of the book is a very good indication of its content and style. The subtitle on the dust cover, *An Inquiry into the Relationship Between Science and Government in the United States*, is somewhat misleading as some readers might be led to expect a balanced and critical analysis. The blurb on the dust cover, which says that the book sets "a standard of clarity, objectivity, and intimate knowledge not likely to be surpassed," is nothing short of ludicrous.

In his own words, Greenberg has attempted "without reverence or cynicism to look into the politics of pure science to determine what it is and how it works." The book ranges over the character of the scientific community, treats extensively the development of the present relationship between the scientific community and the federal government and discusses in detail some episodes involving "Big Science." Greenberg recognizes the achievement of science and scientists but his recognition of these achievements tends to be overwhelmed by his preoccupation with weaknesses. He loves a conspiracy or a fiasco, and better than that he loves a conspiracy that leads to a fiasco. There is both strength and weakness in his book: Viewed as an objective and balanced appraisal of the relationship between the federal government and science, the book fails; but considered as the highly personal view of a journalist looking at particular aspects of science, it is very successful. Good journalism

should be lively, provocative, raise important questions and appeal to a wide audience, and I think that this book meets all the requirements. The standards and criteria for journalism are not the same as for scholarship.

There will be many people in the scientific community who will be irritated by this book because of its flamboyance and preoccupation with the frailty of scientists and scientific institutions; such readers will use its journalistic flavor as an excuse to belittle the book. I think this would be a profound mistake. Notwithstanding its shortcomings, the book is important and worth the careful attention of the scientific community. Substantial sections of the book are of very high quality and touch upon some very difficult questions concerning the relationship between science and the federal government. Moreover, it is important for the scientific community to know how others look at science, especially if the observer, although not a scientist, has a great deal of contact with the scientific world. We cannot expect to be immune from journalistic criticism, and while criticism will sometimes be destructive, it can also be healthy. Self-adulation also has its problems. One of Greenberg's themes is that scientists should not assume that the society at large shares their own convictions about science. He implies that US science is becoming like a great tree with profuse and beautiful leaf structure, possessing a handsome sturdy trunk but suffering from weak feeder roots. He does us a favor to remind us of our insecure political base.

The book is divided into three parts. The first part, consisting of chapters I and II, contains a brief discussion of the character of the scientific community, the system of dispensing research funds and the peer system of evaluation. Greenberg describes some of the attitudes of scientists, especially their attitudes to people outside the scientific world. He likes strong language, but the reader may make the appropriate compensation; throughout the book the English vocabulary is subject

to a skillful beating. The title of the second chapter is "Chauvinism, Xenophobia and Evangelism." It turns out that he uses the word evangelism to mean the concern of scientists for lay appreciation of science. Xenophobia turns out to mean the more extreme aspects of scientific conservatism, especially that directed to people without scientific credentials. Chauvinism turns out to mean a scientist's reverence for science and scientists, frequently exaggerated. Greenberg has correctly perceived some of the attitudes of the scientific community and gives a brief and unsophisticated description of them, although the truth is often displayed in caricature and there is frequently the implication that the whole scientific community shares the more extreme attitudes of a few. I am surprised that he left out any discussion of competitiveness among scientists with its occasionally ugly aspects; the real scientific world can not be explained to the layman without some discussion of competitiveness.

The second part of the book consists of six chapters that describe how the present relationship between science and the federal government developed. After a brief discussion of the period before World War II, Greenberg recounts in a highly interesting fashion the mobilization of science for the war effort. He then turns to that critical period immediately after World War II when plans were made to insure that the vigor of American science, so critical to the war effort, would not be dissipated through lack of adequate support. In many respects this is the best part of Greenberg's book: There is evidence of a great deal of painstaking analysis of original sources. His account provides yet another tribute to the extraordinary role played by Vannevar Bush.

In tracing the genesis of the National Science Foundation Greenberg discusses the early Kilgore Plan, which proposed a new but conventionally structured agency to support research and development. He also discusses the more radical Bush plan for a re-

search agency presided over by a part-time board of nine private citizens appointed by the President. The board would then appoint and supervise the director of the agency. Ultimately legislation proposing the formation of the National Science Foundation and containing the provision for the civilian board of directors passed Congress in 1947. It was subsequently vetoed by President Harry S. Truman because, in Truman's words, it would "in effect, vest the determination of vital national policies, expenditures of large public funds, and the administration of important government functions in a group of individuals who would be essentially private citizens." It is certainly ironic that in the middle of arguments about the special need of science to be insulated from the normal processes of government, there should emerge from within the Department of Defense a most enlightened program for the support of basic research.

In addition to tracing the major currents, Greenberg illuminates a number of interesting side issues. The attitude of physical scientists to the inclusion of social sciences in the fledgling Science Foundation was a strange mixture of prejudice and political realism. In discussing science advisory committees, Greenberg comments

on the development of an "incredibly intricate structure of interlocking relationships whose most notable characteristic was the ubiquity of its limited *dramatis personae*." Obviously there are dangers that must be carefully watched, but it is worth pointing out that in this respect science does not really differ very greatly from the advisory structure of many parts of the federal government or indeed from the world of business.

The third part of the book consists of five chapters with the following titles: The Government of Science; Mohole: The Anatomy of a Fiasco; High Energy Politics; MURA's Last Stand; and The New Politics of Science. In the first chapter of this section, Greenberg discusses mainly the support of chemistry and he uses this example to conduct the reader "on an intimate exploration of the government of science." It turns out to be more like a sight-seeing tour than an intimate exploration. (His language reminds me of the man who, having attended a burlesque show, tells his friends that he has had "intimate explorations" with 20 women.)

The second chapter deals with the Mohole affair. To quote Greenberg, "In the entire postwar partnership of science and government, no research

enterprise has produced greater political embarrassment, anguish or defeat for the scientific community than the grandiose and lately departed project known as Mohole. . . ." Mohole possesses the distinction "of being the only basic research project ever terminated by Act of Congress." The third and fourth chapters in the section deal with the building of very large high-energy accelerators. The principle concern of these chapters is the accelerator of the Midwest Universities Research Association (MURA) and the Stanford Linear Electron Accelerator (SLAC), but the shadow of the projected 200-GeV accelerator is already present. One senses Greenberg's great zest in uncovering the complex maneuverings that took place in the growth of these projects; his zest is especially apparent when he describes the attempt of the MURA proponents to use the political muscle of the Midwest. It is easy to understand the preoccupation of Greenberg with these large, expensive and politically controversial projects, but it is worth remembering that these three chapters are devoted to an extremely small and highly unrepresentative part of the total program in basic research. Taken together the accelerator projects and Mohole account for less than 10% of the funds spent on basic research. Greenberg does not adequately point out this fact for the general reader.

The critical question about these chapters is: Do they provide an accurate description of the events described? Unfortunately, my lack of involvement in such projects prevents me from providing an independent judgment of the accuracy of Greenberg's account. I have tried to form some opinion by consulting some informed people but this action has not taken me very far; some say that Greenberg is highly accurate and others say his account is highly distorted. Perhaps this discrepancy is not surprising—one man's truth is another man's distortion. If I apply my own highly subjective feelings, I conclude tentatively that most of the material is factually correct but that Greenberg's own strong preoccupations concerning the matters under discussion must necessarily result in distortion. For example his preoccupation with the American Miscellaneous Society, in the chapter on the Mohole, has surely resulted in an account of the origins of this project that greatly

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underplays the serious scientific support both at the outset and throughout the program. Moreover Greenberg seems to be willing to exaggerate any evidence that supports his contention that "scientists accept without reservation the proposition that free and open discussion is the life blood of science, but when it comes to relations between society and tax-supported science they frequently see virtue in settling their affairs behind closed doors." This intemperate remark is connected with the alleged refusal of the Stanford group to identify the members of a group that had made a technical appraisal of the SLAC project. Greenberg does not mention that the group in question was not appointed by Stanford but consisted of a number of individual experts consulted by the Atomic Energy Commission concerning the original SLAC proposal. It was the Atomic Energy Commission who refused to disclose names and not the scientists at Stanford. Greenberg's evidence for a kind of public-bet-damned attitude of secrecy on the part of the scientific community is not well documented and is particularly weak in the case of the SLAC project. Quite apart from the public hearings in Congress, the report of which runs to 650 pages, the project was widely discussed and was considered by a number of panels whose reports and membership were made public. Just because some anonymous experts are consulted by the government in a project subject to a great deal of public scrutiny does not convert the issue into a Star-Chamber proceeding. Does Greenberg analyze the changing attitude would be better served if the agencies of the government could never turn to anonymous experts for evaluation of a project? It is ironic that Greenberg should not understand that there is a proper place, subject to appropriate safeguards, for anonymous evaluations and opinions; he relies on them frequently in his book.

In the last chapter of the book entitled "The New Politics of Science," Greenberg analyses the changing attitude towards science exhibited by the public and Congress within the last few years. An early manifestation of this changing attitude was the 1961 report by Representative Fountain on the National Institutes of Health. Greenberg discusses the impact of this report and describes some of the changing fortunes of NIH. (Incidentally,

I am somewhat surprised that Greenberg did not give much more attention to the area of biomedical research; the subject is little mentioned except in a brief portion of the last chapter.) He then turns to the increasing emphasis placed on the geographic distribution of research funds and the increasing consideration of geography in the selection of members of influential advisory committees such as the President's Science Advisory Committee. He attributes much of this change to "the presence of Lyndon Johnson in the politics of science." I think he is correct in this respect. He discusses very briefly the location of the 200-GeV accelerator at Weston and sees the ghost of MURA playing a role in the decision. He deals with the slowing down of the rate of growth of support for science.



DANIEL S. GREENBERG of Science, author of "The Politics of Pure Science."

This is a good chapter and he ends it with sane advice to the scientific community. He counsels less hysteria in response to the prospect of declining budgets. He urges the scientific community to do something about the extravagancies and excesses of some of its members and to take steps to reassure the public that science is not careless with the taxpayer's money. He advocates that the community should "encourage scholarly examination of just what it is that pure science contributes to society and what are the conditions that are most likely to foster valuable contributions." He

addresses some remarks to the government: "Thus, in the twenty-seventh year of the war-born partnership between science and government, it is time for Congress to make NSF what it was intended to be: the principal fount of government funds for the conduct of academic research and scientific training."

The chapter is on the whole sober and perceptive. For a brief moment, in this final chapter, Greenberg unintentionally exposes something of the man behind the mask. Those who see only the mask see a man almost pathologically preoccupied with the frailties of the scientific world. But behind it is someone much more thoughtful—someone who respects science and the scientific community and, while acutely aware that many do not see it that way, is persuaded that in the long run he is helping to achieve a more stable relationship between science and the society that supports it. I once saw an unforgettable and deeply moving mime performed by the great Marcel Marceau; it concerned a theatrical performer who one night found he could no longer remove his mask. It was at first funny, then pathetic and then frightening.

Many of my friends in the scientific community are critical of this book because they fear that its exaggerations and distortions will be quoted and repeated selectively to the detriment of public understanding of science. This is a real danger. A kind of Gresham's Law operates in this kind of critique, such that the more thoughtful and balanced sections tend to be forgotten, while the distortions tend to get repeated and embellished with the passage of time—particularly at this time when the climate of opinion seems to be increasingly hostile to science and to technology.

Many people, and some in influential positions, will quote the book to reinforce their own preconceptions and prejudices against science and scientists. On the other hand, it should not be forgotten that in both the executive and legislative branches of government there are many people who have a sufficiently sophisticated understanding of science and the scientific community to be able to put the book in proper perspective. In the meantime, those members of the scientific community who deal with the political structure and with the public should not be so put off by the exaggerations and distortions in Green-

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berg's book as to dismiss the serious and important issues that it raises.

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The unity of mechanics

THEORETISCHE MECHANIK: EINE EINHEITLICHE EINFÜHRUNG IN DIE GESAMTE MECHANIK. By Georg Hamel. 798 pp. Springer-Verlag, Berlin, 1967. \$21.00

by Rolf Landshoff

The laws of mechanics can be formulated in a variety of ways. The most common approach is to focus on one branch of mechanics at a time, to formulate a set of concepts that is most suitable for dealing with this branch and to develop the appropriate laws. If one proceeds in this manner, the ties that connect the various branches of mechanics are peripheral and do not occupy a very important position.

Georg Hamel approaches mechanics with an entirely different philosophy. In the spirit of Lagrange he stresses the unity of this science and proposes a structure in which its various branches appear as special cases. The starting point of this method is a small set of axioms that make it possible to derive all the relevant laws in a mathematically rigorous way. The pillars of this axiomatic structure are the principle of virtual work, d'Alembert's principle and the principle of release (*Befreiungsprinzip*). The last of these is not found in other books. It is invoked if one wishes to calculate forces of constraint. If the connections responsible for the constraints are absolutely rigid, these forces cannot contribute to the virtual work. When applying the principle of release one thinks of the connections as not being entirely rigid but as giving a little. The reaction forces can now be interpreted as elastic ones that are indeed capable of doing work, and it becomes possible to calculate them by applying the principle of virtual work.

From the axioms the reader is led to the mechanics of mass points, rigid bodies, strings, ropes, membranes,

plates, elastic bodies and fluids. It is obviously not possible to cover much more than the basic laws of each one of these many applications. The one exception is the theory of the gyroscope that the author follows up in considerable depth.

The book also contains an extensive discussion of analytical mechanics, that is, the Hamiltonian formulation, the use of variational principles, transformation theory, the application of the theory of Lie groups, and so forth.

The serious student of mechanics will appreciate the approximately 260 pages of problems that form part two of the volume. As Hamel puts it in his forward: "Certainly thinking is more important than calculating. Nevertheless it is essential that one knows how to calculate as well and one can't learn it if one avoids it. Nothing is more dangerous than to think that a problem is solved when one knows the method for solving it."

The book developed out of the courses taught by Hamel during the 25 years he served as professor of mathematics and mechanics at the Institute of Technology (T.H.), Berlin. It appeared originally in 1949 and has now been reissued with minor corrections 13 years after the death of its author. Two other books of his deal with integral equations and continuum mechanics.

Theoretische Mechanik should probably not be used as a primary text for studying mechanics but it is, in my opinion, very valuable for anyone who wants to become thoroughly familiar with the field.

* * *

Rolf Landshoff is a senior member of the research laboratory of Lockheed Missiles & Space Co.

Perspectives of spectra

ATOMIC SPECTRA. W. R. Hindmarsh, ed. 368 pp. Pergamon Press, Oxford, 1967. Cloth \$7.50, paper \$6.00

by Harold Mendlowitz

Many reprint volumes containing classic papers in a given field are now on the market, and this book belongs in that general class. *Atomic Spectra* by William R. Hindmarsh is different from most, however. It contains about 100 pages of introduction and interpretation of some of the

most important papers on atomic spectra. The papers include the works of Balmer and Rydberg at the close of the 19th century and Hans Bethe's interpretation of the Lamb shift. Hindmarsh, a professor of atomic physics at the University of Newcastle upon Tyne, not only edited this volume but also contributed a substantial part as author of what might be called an introduction to atomic spectroscopy of one- and two-electron spectra. By design this portion is comparatively brief and telegraphic, but it treats in modern perspective the advances in our under-



standing of a field that was considered a closed subject about a decade after the invention of quantum mechanics.

With the newly revived interest in this field because of the current interest in plasma physics and astrophysics, this book should find a wide audience. It could serve as a basis for an honors course or a seminar on the senior undergraduate level or beginning graduate level.

The choice of papers in any reprint volume is a personal one. In this case I would agree with the majority of the choices made. The author-editor was extremely considerate of his audience in that he translated all the papers that were not published in English. He also took the trouble to cull from the reproduced papers those parts that are relevant to the subject treated here.

I think that Hindmarsh is to be commended for his effort and the result.

* * *

The reviewer is professor of physics at Howard University and has been calculating atomic-transition probabilities.