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THIS ISSUE:
SCIENTIFIC RESPONSIBILITY

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TO WHOM ARE PUBLIC INTEREST SCIENTISTS RESPONSIBLE?

"Scientific responsibility" has, in practice, two quite different and partly opposed meanings. The supporters of these different interpretations are often quite innocent of any realization that the other interpretation exists. Thus an unholy alliance advances the bare notion of scientific responsibility. But certain attempts to apply the concept risk the outbreak of open warfare between the two schools.

The problems arise with regard to the participation of scientists in the public debate. On matters within the scientific community, there is no important difference in point of view among scientists on what constitutes scientific responsibility. All oppose such traditional forms of scientific irresponsibility as falsification of data, plagerism, suppression of opposing points of view, etc.

The underlying question at issue is whether the traditional notions of scientific responsibility, developed within the community, can cope adequately with the entirely different problems posed in the interface between science and the public. At the heart of the difference in perspective is the question: "responsibility to whom".

Responsible Conduct Seen as Issue

The narrow school of interpretation prefers to use the concept "responsible conduct of scientists" as its interpretation of the phrase. In its view, the "responsibility" at issue is a responsibility to the scientific community: not to demean the community or to diminish the standing of colleagues, by acting in ways dissonant with the traditions of science or its popular image. It sees improper actions as threats to the integrity of science and, sometimes, even to its funding.

In particular, this school often considers it vaguely or flatly irresponsible to make public assertions which are imprecise or, worse, unprovable; to generalize without firm grounds; and/or to speculate. It is often considered questionable: to advocate policy decisions that involve science but go beyond it; to campaign for such policies; to ally oneself with non-scientists in such campaigns; to accept the undignified and inadequate conditions for presentation the media often require; to go "over the head" of the scientific community; and so on.

A broader interpretation of scientific responsibility conceives it primarily as a responsibility to society rather than to the scientific community; this school of thought prefers to use the phrase "social responsibility." It has acquiesced in the fact that virtually all arguable policy decisions inevitably go beyond science. It accepts as inevitable that scientists involved in public debate will have to go beyond discussing what is scientifically known for certain. In its view, the name of the public policy game is decision-making under enormous uncertainties; what is known for certain is usually uncontroversial and needing no exponents.

Perhaps the most important difference between these two interpretations of scientific responsibility is that the narrow view implicitly discourages involvement by scientists in public debate, while the broad view instructs them that such participation is their "social responsibility."

Let no one minimize the importance of this difference in perspective. At issue is the degree of participation in the public debate of hundreds of thousands of the most intelligent citizens in America, individuals whose special training and knowledge makes them especially well-suited to objective analysis of the issues in and around science.

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— Reviewed and Approved by the FAS Council

MEMBERS INVITED TO COMMENT ON SCIENTIFIC RESPONSIBILITY

Scientific responsibility is hard to define. And it is harder to practice than preach. But nothing is more important to FAS than an investigation of such issues; with the help of our members and others, we plan to turn our attention to this subject from time to time.

This preliminary discussion ponders the differences in meaning which "scientific responsibility" has in the ideological camps of other nations as well as the differences of view in our own debate. In a subsequent *Report,* later in the academic year, we plan to go somewhat further by discussing hypothetical but concrete vignettes to give substance to a discussion that is otherwise unworkably abstract.

We ask our readers to send us their reflections. What are the key issues of scientific freedom and responsibility? To whom is responsibility due? What kind of freedom is meant? Where are the contradictions between the different meanings? And what practical conclusions should FAS draw? Send your relevant complaints also — about FAS as well as others — and your commendations. □

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Standards

The central issue is how standards of responsibility in public communication should be maintained.

There should be standards. We do believe that scientists should hew to a higher ethical standard than that which need be obeyed, for example, by politicians. Scientists should: avoid dogmaticism; make their assumptions as overt as they can; qualify their remarks as well as conditions permit; be willing to surface, recognize, and admit weaknesses in their own argument; be ready to reason with those who disagree; and, in general, behave in a civilized fashion.

What we doubt, however, is the ability of professional scientific organizations to monitor and maintain these standards. Scientists involved in the public debate confront problems totally unfamiliar to these traditional organizations: unusual media conditions; the necessity to work from inadequate sources; enormous uncertainties about facts; pressures of time; tactical decisions concerning allies; controversies mixing values and facts; and many others. As a result, the traditional professional society really has no consensus, and hence no standing, with which to determine whether a scientist met his obligations to the public in a praiseworthy or censurable fashion. These are not questions of referees, of publication disputes, of methods of scientific argumentation. These are problems far more unruly.

Another method for maintaining standards is no better. This is the model known to lawyers, doctors, and engineers; these disciplines have codes of professional responsibility designed to monitor interactions between their professionals and the public. But codes of this kind have not worked well, often degenerating into self-serving efforts to protect the marketability of the scientific technology at issue. And, in any case, no formal code can resolve the multidimensional aspects of dealing with real problems in a real political world.

Marketplace of Ideas

What is left? In the first place, in the public arena, for the most part, the solution to poor analysis and scientific distortion is better analysis, and critiques of that distortion. In this sense, the solution to the involvement of scientists whose views or behavior one regrets is one's own involvement. We believe that, in the clash of scientific interpretations and opinions, those who apply the scientific ethos tend to prevail because those who apply that ethos most steadfastly enhance their credibility over time both with other involved scientists and with the public.

Moreover, in America, we have some faith that the societal methods of monitoring the public debate will be generally adequate to control scientific contributions just as they absorb the specialized contributions of many other kinds of experts. A competitive market place of ideas — including, of course, criticism by fellow scientists — will keep the discussion relatively honest.

To the extent to which the free market of ideas fails, it will be necessary for those scientists who are themselves involved in the public debate to evolve their own standards. Public interest scientists should have the right to be judged by their peers — by others who have run the societal gauntlets involved; by others who have appraised the options available. Through their own peer-group pressures — and their public service awards — scientists involved in public debate will provide role models for each other.

In sum, the solution to the interminable dispute over scientific behavior in the public arena is not to be found in merely repeating what scientists have preached as responsible conduct inside science but in what they come to practice collectively as social responsibility outside science. □

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SCIENCE AND THE THREE SOCIETIES

Broadly speaking, scientists face three kinds of societal working conditions. In the most difficult, they find themselves under right-wing dictatorships characterized by contempt for intellectuals, and fear of their libertarian tendencies. Examples are the governments of Chile, Argentina, South Korea, Thailand, Brazil, and Uganda. Here the scientists are neither prized nor free.

Typically the governments are ready to ignore the impact on their societies' development of repression of scientists. The result is often even less freedom for scientists than possessed by other members in the society.

A second class of governments prize their scientists and provide them with varying ranges of special perquisites but do not permit them scientific or political freedom. This is the condition of the communist world: the Soviet Union, the nations of Eastern Europe under Soviet hegemony, the People's Republic of China, Albania, Yugoslavia. Because these economies are planned, the scientific community's resources are allocated and directed. These states reject the notion that science for science's sake will maximize payoffs by permitting full rein to the scientists. Not only science but everything else (including chess and art) must have its purpose.

Marxism and Scientific Responsibility: Theory

The socialism of Marx could not be, in principle, more prone to favor "science." Marxists consider Marxism to be the "science of society"; in fact, Engels wrote that it was only with this scientific discovery that "the true history of mankind begins." This approach produces a faith in the social sciences that far exceeds that in the West, one which further enhances the popular faith in natural science.

In particular, also, the underdeveloped quality of tsarist Russia left little doubt in the minds of the revolutionaries that science would be critical to the salvation of the Soviet Union. The net result is prestige for scientists in the Soviet Union that is quite unparalleled in any other nation in the world. The members of the highest Soviet scientific rank (Academician), numbering about 400, are considered "immortals" with automatically commissioned biographies and special burial plots. They earn more than 10 times the average wage.

At the other end of scientific achievement, but also illustrating the principle, every chauffeur characterizes himself as "engineer." All in all, the Soviet scientific community is immense; one association of scientific workers has 7,000,000 members. A very large fraction of all higher education graduates are technical graduates in one sense or another and consider themselves scientists or scientific workers.

J. D. Bernal: Marxist Spokesman for Social Responsibility

What do Marxist scientists consider their social responsibilities to be? No better advocate of the theory of responsibility in European communist states exists than the late J. D. Bernal. Bernal was a committed Marxist ("For my part I can only understand the world as I have learned and experienced it, that is, largely in the light of Marxism . . .").

He was also pro-Soviet believing: that the Cold War had been deliberately fomented by the "privileged classes in America and Europe"; that Eastern Europe had been "liberated" and that the Sino-Soviet split was "bickering." He was also very able. His four-volume compendium, *Science in History* provides a remarkable Marxist analysis of the role of science from the Stone Age through the hydrogen bomb.

Bernal's approach to social responsibility can be seen in the Constitution of the World Federation of Scientific Workers, which he drafted, and for which he continues to be the patron intellectual saint:

> "The primary responsibility for the maintenance and development of science must lie with the scientific workers themselves, because they alone can understand the nature of the work and the direction in which advance is needed. The responsibility for the use of science, however, must be a joint responsibility of the scientific workers and of the people at large. Scientific workers neither have nor claim to have the control over the administrative, economic and technical powers of the communities in which they live. Nevertheless they have a special responsibility for pointing out where the neglect or abuse of scientific knowledge will lead to results detrimental to the community. At the same time, the community must be able and willing to appreciate and to use the possibilities offered by science, which can be achieved only through the widespread teaching of the methods and results of the natural and social sciences."

Bernal's major conclusion was that science had become too important to be left to scientists or politicians and that the "whole people must take a hand in it if it is to be a blessing and not a curse."

European Communism and Scientific Responsibility: Practice

Writing before, during, and immediately after World War II, Bernal was oblivious to the intellectual realities of Soviet life, in particular to the widespread apathy and cynicism. Other committed Marxists were more perceptive. Jean Paul Sartre, writing after the Czechoslovakia repression, remarked that "socialism has fallen back into the long night of its Middle Ages," and spoke of the "steady remorseless degeneration of Soviet socialism."

The scope for Soviet scientific responsibility, of the kind Bernal espoused, had been correspondingly limited by these practical realities. Scientists have had "primary" responsibility for the development of science but heavy pressure has been placed on them to avoid "bourgeois" abstractions. In a planned economy, all of the problems of bureaucratic direction and control have appeared.

Bernal's notion of "joint responsibility" for the use of science by scientists and the public at large cannot be recognized, much less vindicated, in the Soviet political process. The public has no voice, and no method exists for appeal to the public. A number of concrete ideological problems have arisen.*

We do see stirrings of scientific responsibility in the efforts of Sakharov to persuade Khruschev to sign a partial test ban treaty and, more generally, in the efforts of

*While Lysenkoism and its impact on biology is the best known example, Soviet scientists have had to wage continuing ideological struggles on other fronts, especially in coping with the philosophical demands of the official philosophy, dialectical materialism. Was the role of the observer in quantum mechanism a form of "idealism" opposed by materialists? Could relativity's appraisal of space and time be defended as having made them "forms of the existence of matter" or would the ideologues decide that relativity should be suppressed for having adopted the notion that space and time were products of "pure reason?" Was there a Marxist-Leninist notion that the Universe had to be infinite or could astronomers consider finite, closed, modeis? Did Marxist-Leninist materialists have to believe in some kind of spontaneous generation (at some level) to avoid the charge of religiousity?

the Soviet Pugwash participants to explain arms control to their government in the period between 1955 and 1970 before serious and sustained official talks began. No doubt there is much more that transpires within the permitted limits of discussion, struggles to clear up Lake Baikal and the like. But it is significant that real manifestos explaining science and social responsibility, such as Sakharov's *Progress, Coexistence and Intellectual Freedom* had to be smuggled to the West.

For the most part, the Soviet scientific community fights not for social responsibility but for unfettered foreign contacts, for free exchanges with other scientists.

To what extent are these problems arising from the nature of communism and to what extent from the distinctive cultural and historical conditions existing in the Soviet Union? Obviously views differ. One who traced the problem simply to economic planning was Friedrich A. Hayek, Nobel Laureate in economics. In *The Road to Serfdom* (1944) he argued that facism and communism wree merely:

> "variants of the same totalitarianism which central control of all economic activity tends to produce."

He believed that an unforeseen but inevitable consequence of socialist planning was to create a state of affairs in which totalitarian forces would get the upper hand.

Maoism and Social Responsibility: Theory and Practice

The Chinese go much further than Bernal. The responsibility for the use of science is not a "joint responsibility" of scientific workers and the people at large — instead, the ideology gives much more weight to the public. Indeed, the scientists do not even have the primary responsibility that Bernal advocated for science itself. Instead — to summarize a friendly review by SESPA *(China: Science Walks on Two Legs* Avon books, 1974) — the literature shows constant emphasis on cases where "the peasants were ahead of the theoreticians." Efforts are made to demystify science, to deny that science is "too deep" for ordinary people, to combine the efforts of specialists and non-specialists alike and, above all, to combine "theory and practice." It seeks, in short, to reverse the saying of Mencius:

> "Those who work with the heart shall rule. Those who work with hands shall be ruled."

The cultural revolution instructed researchers to avoid the three divorces: "between politics, practice and laboring people." It led to debates over whether scientific papers should be signed individually or collectively and how collectively. It sent scientists out to the farms.

The net effect of these doctrines in practice is not now known. In the first place it is not very well understood why modern science did not develop in China for the past few hundred years, and this undoubtedly reflects casts of mind and cultural traditions to which this ideological approach is directed. Furthermore, when Joseph Needham began his celebrated investigations into this first question, he uncovered still another related conundrum: why was Chinese science ahead of the West in the period before the West's industrial revolution? There is obviously much in the notion of science and society in China that we do not understand.

Americans tend to think of ideology as a superfluous contaminant of law, regulations and tradition. In fact, these ideological injunctions — as with all ideology in China — are playing an active coordinating role instructing 800,000,000 citizens how to conduct their business. What do you do when you don't have law? For example, under the notion of the social reponsibility of science, enterprises are encouraged to allocate a certain portion of their funds for anti-pollution measures where formerly they might have made all efforts to increase production and exceed quotas. Similarly enterprises would have to inform workers that excessive sound might impair hearing. The desirability of an ideology that presses for this kind of activity is in accord with thinking in the Western democracies of socially concerned scientists.

On the other hand, most FAS scientists would look with horror at the likely disruptions of the Chinese scientific community, in practice, when forced to confront interference in the workings of the scientific community itself. No doubt scientific careers have been destroyed from "wrong thinking." And certainly, Chinese scientists have fewer rights of expression and communication than even those Soviet scientists about whom FAS is concerned. No doubt, the Lysenko affair is being repeated many times over in China. On the other hand, again, do Western scientists of developed countries have the perspective on the needs of an underdeveloped country to chide it for insisting that science be developed with applications first and foremost in everyone's mind?

In short, China exemplifies the most thoroughgoing destruction of barriers between the scientific community and the public. The destruction in one direction looks somewhat better than the destruction in the other, but we lack, at present, a sense of having standing to judge.

Scientific Responsibility in the Western Democracies

The basic theoretical issue in discussions of "scientific responsibility" in democratic states is "responsibility to whom." The progressive view largely agrees with Bernal's formulation; indeed, our Constitution carried these sentiments before he drafted them for WFSW. Here the responsibility is to the *public.* But the traditional view believes that the responsibility of scientists is a responsibility to the *scientific community* to act in ways consistent with the scientific ethos.

The traditional view is worried about the effect on the public image of science of scientific involvement in public debate.

Thus in an October 11, 1976 speech, Dr. Philip Handler wrote:

> "We have learned that the scientist-advocate, on both sides of such a debate, is likely to be more advocate than scientist and *this has unfavorably altered the public view of both the nature of the scientific endeavor and the personal attributes of scientists."* (Emphasis added).

He went on to urge such scientists to be as "honest, objective, and dispassionate" in describing technological risks to the non-scientific public, as they would have to be in the self-policing scientific endeavor. (However, Dr. Handler was far from fulfilling his own charge in this speech; see page 7 of this Report for a number of examples.*)

*In fact, this individual's tendency to rhetorical exaggeration is notorious. For example, when the House of Representatives voted to require the National Science Foundation to let it review NSF grants before their final NSF approval, the Academy President charged the Congress with an action that was "tantamount to book burning" and to having adopted a procedure "appropriate only to authoritarian regimes." This can hardly influence Congressmen favorably in their assessment of "the personal attributes of scientists."

SAMPLE PROBLEMS PUBLIC INTEREST SCIENTISTS FACE

Most discussions of scientific responsibility avoid any illusion to dilemmas of responsibility. It is as if one were to discuss ethics without ethical dilemmas. Scientists reduce the problem to a few phrases: ("honesty, dispassion and objectivity") or assume away the problems ("We know when we speak scientific nonsense.") Obviously scientific responsibility, whatever it means, is a branch of ethics and does have dilemmas. Here are a few which members may wish to teeth on.

Speaking Out: Timing

E.g. — As a result of certain novel experiments you have undertaken, you believe that a common additive is, in reality, quite dangerous. It is impossible for you to be certain and a year more of tests are necessary. The health authorities are willing to do the tests but urge you not to discuss the implications of your work with the press lest "all hell break out." Do you hold a press conference or defer to established authority? And how do you decide?

Providing of Unsupported Opinions

E.g. — You have been voicing reasoned opposition to nuclear power for some years when an opportunity arises to appear on the NBC Today Show. After rather irrelevant questioning, the moderator says, "Well, now Dr. X, we have 30 seconds left, please tell us, all things considered are these reactors safe or unsafe?"

What do you say to the tens of millions of persons watching?

Problems of Allies

E.g. — You oppose the SST on a number of grounds but put less stress on others and consider still others wholly misleading. A leading Congressman asks for your help in preparing a paper opposing the SST but you discover that he cannot be dissuaded from emphasizing less important issues and at least one point you consider misleading. Do you assist him in preparing the speech or not?

Phrasing Conclusions

E.g. — You have read enough about the ABM, and had enough experience in Government, to believe that you understand one important aspect of the situation quite clearly and, indeed, that you can make a very plausible case for your position on the basis of bits and pieces of publicly released data. You are asked to testify. Should your testimony end by conveying the certainty you do indeed feel (for the reasons provided in the testimony) or should it end with assertions you do not really feel protesting that your failure to have all the data disqualifies you from reaching a conclusion?

Endorsements Under Uncertainty

E.g. — You are a chemist and, during testimony, you are asked whether all things considered you would endorse a certain toxic substances bill that has the best and only chance of passage. You have little certainty that the bill is really workable. You suspect that the problems could be worked out in practice and believe it is now or never for a toxic substances bill. But the bill is too complicated to be wholly grasped by you, and, possibly, anyone else. Do you endorse the bill?

Getting The Public's Attention

E.g. — You are persuaded that certain agricultural procedures are dangerous. You are convinced that, once attention is drawn to the issue, you will be able to persuade the relevant scientists on their own terms but you just are not being taken seriously. It becomes evident that no attention will be paid to you unless you appeal, in dramatic tones, to the public. Do you write a dramatic and somewhat sensationalized version of the situation to force the scientific comunity to investigate or do you suppress this impulse and keep plugging away?

This school of thought on responsibility is clearly more concerned with the effects on *science* of scientists participating in the public debate than in the effects on *society*. It wants scientists not to embarrass science by getting too involved. Here, for example, are the results of an interview with Dr. Handler in the *Wall Street Journal* of April 3, 1975:

" '. . . policymakers and the public must learn to use (science) properly and not expect more than it can reasonably produce.

At the same time, scientists must show greater restraint in their increasingly frequent forays into the policy-making world.'

These are the views of a man who's thought a lot about the subject: Philip Handler . . .

* * *

" 'Scientists must take some of the blame themselves for their recent image problems,' Mr. Handler agrees, — 'for pretending to expertness they don't have, for giving advice in areas far outside their own competence, for advocating policies with unbecoming heat and shrillness.'

* * *

" 'Once the scientific community has presented the facts, however, it must leave final decisions to the policymakers and the public,' Mr. Handler asserts. 'Science can contribute much to enhancing agricultural production,' he states, 'but American policy with respect to food aid is not intrinsically a scientific question'."

* * *

Similarly, science can study whether energy independence is technically feasible or whether Soviet underground nuclear tests can be detected, but, he insists, scientists must then let regular policymakers decide whether to try for energy independence or just what arms control proposals to put to the Russians."

The conservative *Wall Street Journal* concluded approvingly: "Both science and government seem well served by this reasonable man."

The Excluded Middle

But are they? This view *seems* defensible because it assumes away the entire problem. It is an over-simplification which might be termed that of the "excluded middle." On the one hand, science presents the "facts." On the other, "policymakers" and "public" decide what to do. It leaves out the scientific policy analyst and the scientist engaged in political action in or out of government. Are scientists to "drop out" of these middle roles lest science suffer "image problems?" (This would, in

particular, disfranchise hundreds of thousands of scientists from political rights accorded their fellow citizens).

This view of what scientists do and should do in a Western democracy is the scientific analogue of a civics book discussion of how democracy works.

In the first place, the policymakers need inferences since facts seldom go far enough. This was put well in a *Nature* editorial of October 14 entitled "More than Facts, Judgments":

> "The scientist is most unlikely to be able to deliver to the decision-maker any useful sort of factual statement, because he is hardly going to be allowed to perform the appropriately large experiment or observation. All he can generally supply in the way of facts is some results from pilot projects, some calculations which may be relevant and so on. What the good scientist should also be competent to provide, however, is inference, and this albeit tentative and hedged-about, is what the decision-maker needs and what the science court seems to avoid.
>
> "Factual statements of the highest presumptive validity would merely be about rats, about rocket samples, about tensile strengths. Those involved in public policy need to know whether, in the scientist's best judgment, such statements can be generalized. Intelligent customers for these sorts of judgments know full well that scientific 'truth,' being a whole level higher than facts, is often every bit as elusive and changeable as political and economic 'truth.' But they still expect the scientist to go beyond the solid ground of his facts."

In the second place, the policymakers need policy analysis. Kenneth E. Boulding put it this way in a *Science* editorial (October 31, 1975): "The decision maker wants to know what are the choices from which he may choose" and "bad agendas make it difficult to make good decisions."

Finally there is the all-important issue of political action by scientists. A *Science* editorial of November 28, 1975, observed:

> "If it is to be effective, the scientific community must learn to deal with Congress as it is, not as the scientist thinks it ought to be."

Branscomb Committee Takes Modern Approach

A more modern approach to scientific responsibility than that expressed in the *Wall Street Journal* was announced at the same conference at which Dr. Handler's speech was given, by a NAS Committee on "Science Technology and Society." It urged scientists not to view themselves "only as the custodians of knowledge, aloof from world affairs . . ." It said their role was:

> "not only to contribute new knowledge, but also to participate in the creation, evaluation, and application of the right technologies for societal use"

It urged scientists to "rethink their roles and the roles of scientific institutions."

The report said that the:

> "values by which scientists judge one another must undergo an evolution which elevates the incentives for responsible professional performance and high-quality research applied to problems of public importance . . ."

These were tasks that must be undertaken by professional societies, international unions and scholarly institutions and could not be left to legal or political institutions. (The 17 person Conference issuing this document was chaired by Lewis M. Branscomb of IBM and contained such American representatives as: Harvey Brooks, Roger Revelle, Stephen Schneider and Herbert York).

The traditional point of view in the scientific community has always feared too much emphasis upon the social ends of science. In 1945, Michael Polanyi called such emphasis "misguided generosity" that weakened the "autonomy of science." In 1949, he wrote that:

> "We scientists are pledged to a higher obligation, to values more precious than material welfare; to a service far more urgent than that of material welfare."

This point of view still exists, but in a muted form. Dr. Handler's October speech said that it was a challenge for the scientific community to "be seen as honestly responsive" to the needs of society. But he strongly urged scientists not to justify their research on social grounds except on the "historically valid argument" that science's benefits have come from permitting science what he earlier called its "own internal sense of direction."

He felt that scientists who emphasize the social utility of science:

> "force themselves to take a responsibility for technology which they should not have to take, because science is not technology and should not be held to account for those negative consequences which, rightly or wrongly, are being laid at the door of technology."

Responsibility For What?

But if scientists are not responsible for the technology that arises from science, what would they be responsible for? It is rare that science causes problems without an intervening technology. It is striking that this speech explained Pugwash not in terms of the social responsibility of scientists who built the bomb but simply because scientists were good at talking to one another:

> "Nor is it a problem in science that there is now in the hands of the military several hundred times more explosive power than was used in the totality of World War II. But because members of the scientific community, regardless of nationality, understand each other easily, the scientific arena offers a special platform for discussing the problems of arms control and disarmament, as the founders of the Pugwash movement recognized."

In fact, what they "recognized" was a sense of responsibility.

The NAS Committee is right. There is no safety today in a restraint that keeps scientists out of the debate. The scientific community that ignores the direct and side effects of its work on society is going to be blamed for them, all the more for its insensitivity. Conversely, the scientific community from which scientists emerge to take responsibility for, and to assist in managing, the implications of its work is going to be regarded with sympathy even when things go wrong.

To take a concrete example: What if the Federation of American Scientists (*nee* the Federation of *Atomic* Scientists) and the Bulletin of the Atomic Scientists, had not been created? What if atomic scientists had shown no interest in controlling the bomb or in the political and educational action required? What if the scientific community had provided "only the facts" and "only when asked" and had avoided being "shrill" and shown "restraint?" Would science and scientists be better thought of in Congress, among the press, in the media and in the public? Who can think so? □

IS ALL KNOWLEDGE GOOD?

"The Stone Age may return on the gleaming wings of science, and what might now shower commensurable material blessings upon mankind, may even bring about its total destruction. Beware, I say, time may be short."

— Winston Churchill

Since the atomic bomb, socially concerned scientists in the Western democracies have become slightly less sure about what had formerly been an axiom of scientific thought: the value of knowledge. This touch of ambivalence can be documented in the statements of two of FAS's most profound commentators on scientific freedom and social responsibility.

In a March 4 rally in 1970, Victor Weisskopf said:

"The main responsibility of a scientist was, and is, the development of knowledge within his own science by teaching and research. But in these days, when the detrimental effects accumulate so rapidly, scientists must be concerned about the physical and social effects of their work. It may turn out that it will be too dangerous to create new scientific knowledge. *The result of the scientists' concern may be a decision to stop scientific progress."* (Emphasis added).

This was a daring statement. Nevertheless, a few minutes later, Professor Weisskopf ended his speech with the sentence: "Whatever your viewpoint, it is good to know more."

In the AAAS Report on Scientific Freedom and Responsibility drafted by John Edsall there is a sentence:

"The Committee believes that the vigor and integrity of science require that all areas of potential knowledge be open to inquiry; but the means of inquiry are open to change, particularly where life processes and human behavior are involved." (Emphasis added).

However, Professor Edsall is less sure, himself, about this point of view and in a submission to NIH supporting the guidelines on recombinant DNA, he remarked:

"I should add that I do not hold the view that the increase of knowledge is necessarily good."

He believes, in particular that, if a general nuclear war occurs, the net impact of the last few hundred years of science on mankind could be negative despite the enormous benefits of science to date.

Philip Handler, President of the National Academy of Science, felt obliged to respond to this kind of question in his recent speech to ICSU. He remarked:

"Particularly troublesome is the ever more frequent expression of the notion that there are questions that should not be asked, that there are fields of research that should be eschewed because mankind cannot live with the answers. NONSENSE! No such decision can be *rational,* much less acceptable." (Emphasis added).

While acknowledging the possibility of temporary delay because of "uncertainty" concerning risks to the public or investigator, Dr. Handler said there could never be a time when "the avoidance of knowledge should be mistaken for wisdom." The "foolish" government which knowingly interfered with the course of science "will itself be the inevitable victim of that crime."

It is thought-provoking that these ultimate technological assessments, which are far from dispassionate, wholly beyond proof, and stated, at best, much too flatly, were contained in a speech which chided "scientist-advocates" for lack of dispassion. □

FBI CLEARED FAS IN 1950 AND PROMPTLY FORGOT ALL ABOUT US

FAS asked the FBI for its file on FAS and discovered that the FBI had investigated FAS from 1946-1950 during the period when FAS sought civilian control of atomic energy in the form of an Atomic Energy Commission. The conclusion reached by the FBI was that FAS was neither communist dominated nor had pro-communist policies. The FBI summary conclusion in full read as follows:

"The Federation of American Scientists has been active in opposing military control of atomic energy, supporting civilian and international control; critical of security procedures concerning personnel engaged in atomic energy; and in favor of less secrecy concerning atomic energy. This organization was the subject of a security investigation by this Bureau from 1946-1950. The investigation failed to disclose that the organization was communist dominated or that its policies were pro-communist although some of its members throughout this country, both on a national and local scale, have been described as communists or pro-communist."

As to what members the FBI has in mind, we find that the FBI has a report from that period on FAS provided by the Army and it listed the following past FAS officers as having engaged in "communist front activity":

J. Robert Oppenheimer — father of the atomic bomb

Harlow Shapley — the most eminent astronomer of this century and a former AAAS president

Edward U. Condon — former head of the U.S. Bureau of Standards in the Commerce Department

Harold Urey — Nobel Prize winner in Chemistry

John P. Peters — FAS records do not indicate that Peters, who was a Yale University medical professor, was ever an officer of FAS; Peters' name was cleared by the Supreme Court in a loyalty case decision in 1955.

The Army concluded, however, that association with FAS should "not in itself be construed as derrogatory information" since a "reliable Federal agency" (presumably FBI) has stated that there is no evidence that the FAS is "in any way dominated by the Communist Party." Signed by a Colonel in G2, this memorandum is undated.

The FBI had extraordinarily little in its 30 year old file on FAS after it closed its investigation in 1950. Only two crank letters asking about us in three decades were filed and only a few pages in the seventies, including a letter from FAS's director to Mr. Kelly. The entire file is only about one inch thick, of which about one-third is a copy of an FAS report mentioned below. (This does not, however, include the file on the investigatory period 1946-1950 for which we have not yet asked, accepting the summary memoranda as a surrogate at least for the present).

The FBI Freedom of Information Office — which cooperated cordially and with every indication of straightforwardness in all of FAS's requests — advised that this small bulk released does reflect the bulk of the file. In answer to our request, we were advised that while items can be withheld for reasons (classification, internal rules and practices, invasion of privacy, reveal sources, endanger personnel) these only involved small parts of documents, or scattered coversheets in our case. (FBI did

overlook clippings and, when we noted their absence, agreed to send them.)

The only complaint that comes immediately to mind involves the FBI summary conclusion about some FAS officials having been *"described* as communist and pro-communist." (Emphasis added). While this was at least literally true in the tense forties and early fifties when people were freer in offering descriptions of others in those terms (and when, before Khruschev's denunciation of Stalin and the suppression of Hungarian, and Czechoslovakian uprisings more people might have fairly been described in that way) it is certainly not an accurate observation today. Evidently the cost of not having the FBI investigate one's organization continuously is an outdated investigatory report.

Items in the file included:

Item: In 1960, the Director of FBI's L.A. office reported on our (now divested) L.A. Chapter and noted that he "feels certain that the degree of CP (Communist Party) membership" in the chapter was "negligible." He termed the 75 members mostly "liberal in their thinking and mainly interested in peace and prosperity."

A summary memorandum reviewed a substantial number of chapters briefly concluding, in each case, that none were communist dominated but remarking variously that "visionary liberals" did take part or that "some members were communist sympathizers" and so on.

Item: 1952, FAS was complaining that the security requirements for alien scientists was so high that they could not visit the United States. The visa division wrote FBI at some length saying:

> "if the scientists really made an issue of it, it was a matter which should be handled by the Interdepartmental Committee on Internal Security rather than unilaterally by the Department of State."

Item: 1950, a report to FBI details the demise of the New York FAS Chapter; its decline is said to have begun during 1948 at which time three scientists, whose names are given, were defeated for re-election to the Executive Council. These three, termed a "pro-communist minority" then dropped out. (Two of the three subsequently became officers of the World Federation of Scientific Workers described in our November Report). □

NUCLEAR WEAPONS: CHINA & U.S.S.R.

On October 17, the People's Republic of China detonated its 20th nuclear explosion; the fallout from this atmospheric test was detected in America.

FAS wrote the Administration urging it to offer to sell the Chinese such (excavation and instrumentation) equipment as might make it possible for the Chinese to move these tests underground. Such a decision would put the Chinese in effective compliance with the Partial Test Ban Treaty. Our proposal was received as an ingenious and constructive suggestion by a number of high officials.*

Research revealed that only three of the 20 Chinese tests have been underground. Two were less than 20 kilotons and one in the "low-intermediate yield range." Other tests have ranged up to 3 megatons.

Threshold Test Ban In Difficulties

Meanwhile, Soviet underground testing has become the source of controversy. The United States and the Soviet Union have signed but not yet ratified a ban on underground tests above 150 kilotons and have agreed to stay below the limit pending ratification. However, it now appears that the United States cannot estimate the size of the Soviet tests with sufficient accuracy to monitor the agreement by national means. At the moment, the size of the tests can be gauged only up to about a factor of "two". This means that a test which the Soviet Union knew to be 100 kilotons — well below the limit of 150 — might appear to some U.S. estimators as 200 kilotons or well over the limit. It was believed that further experience would lower the range of uncertainty somewhat. But the agreement — which FAS opposed on a wide number of grounds beside this one — obviously lends itself to nasty interagency disputes about compliance. □

*However, in a letter that Parkinson would admire, the Department of State eventually responded with two contradictory assertions:

> "The Chinese atmospheric testing cannot be attributed to technological deficiencies since they have already conducted three underground tests, the latest on October 17 this year. We will, none-the-less, bear your suggestions in mind in formulating our future policies in this field."

The first of these sentences is obviously false — that small tests have been underground does not establish that the Chinese do not have technological problems. The letter was signed at a low level. FAS wrote back expressing our bemusement.

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