Discussion

CAN INVESTIGATIONS IMPROVE SCIENTIFIC ADVICE? THE CASE OF THE ABM

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I

. . . Not since Franklin Roosevelt's draft law cleared the House of Representatives by one vote in the summer of 1941 had a President been put to so stern a challenge by Congress on a major question of national defense. Richard Nixon had staked his prestige on a no-compromise commitment to the view that a beginning on the Safeguard anti-ballistic-missile (ABM) system was " absolutely essential " to America's security. Precisely half the U.S. Senate said he was wrong. In the showdown last week, Mr. Nixon won. . . . But the hairbreadth margin of his victory—51 to 50 on the critical test vote—put the President and the military on notice that their will in defense matters, unchallenged for a generation, would no longer pass without question.

So began the *Newsweek* story [1] in August 1969 telling of the dead-heat climax of a controversy which had been growing in United States governmental, defence and scientific advisory circles for a decade; within the year prior to the vote in the Senate, the public debate had reached a level of intensity and acrimony which was unparalleled for a discussion of a weapons issue. Now, in the spring of 1972, two and a half years and $4 billion later, the issue remains muted, awaiting the outcome of the Strategic Arms Limitation Talks (SALT). It is, however, certain to arise again, either in questioning decisions provisionally reached at SALT or in reopening the debate over whether or not to continue deployment of the ABM system.

The long history of debate which preceded the Senate vote and the certainty of the continuation of the controversy are part of the context within which the report of the Operations Research Society of America (ORSA) on the conduct of this debate, reprinted in somewhat abridged form in the previous issue of *Minerva*,[2] must be examined. The report is the product of an investigation of an *ad hoc* committee of operational research specialists appointed by the council of ORSA to investigate the professional conduct of the debate, at the instigation of Professor Albert Wohlstetter, one of the most vigorous participants in the debate. The report was accepted by the council of ORSA on 5 May, 1971, and published in the September issue of the society's journal. In a minority statement five of the 12 members of the council objected to the society's " quasi-

[1] *Newsweek*, 18 August, 1969, pp. 20–22.

[2] " Guidelines for the Practice of Operations Research ", *Operations Research*, XIX, 5 (September, 1971), pp. 1123–1258. Reproduced in abridged form as " The Obligations of Scientists as Counsellors: Guidelines for the Practice of Operations Research ", *Minerva*, X, 1 (January, 1972), pp. 107–157.

judicial function of investigating and reporting on professional behaviour of individuals ".[3]

Putting aside for the moment the ethical and legal aspects of an investigation of professional conduct, it is evident that the debate preceding the vote in the Senate was a milestone in the history of scientific and technical advice in matters relating to military decision-making. Consequently, a definitive investigation which was impartial and comprehensive would be of great interest and value. It is possible that the aim of the *ad hoc* committee was no less than this. But when one takes into account the length and heat of the debate and the fact that it will continue as an issue for some time to come, an investigation which would have been reasonably satisfactory to all the contending parties would have been a superhuman achievement.

My own impression of the reception of the report in the four months following its publication is as follows. Of all those persons known to me, who have intimate knowledge of the substantive questions, all who supported the ABM have, with one exception, favoured the report, and all who were opposed to the ABM have been critical of the report. Comment in the press has been equally partisan: Mr. Joseph Alsop's praise of the report [4] and the White House letter of congratulation to the president of ORSA [5] were roundly criticised by Professor George Rathjens and by Drs. G. B. Kistiakowsky, H. Scoville and H. F. York.[6] The *Wall Street Journal* saw the report as a vindication of Professor Wohlstetter.[7] Professor Philip Morse, one of the founders of ORSA, expressed strong objections to the report,[8] as did Professor W. K. H. Panofsky.[9] Apart from a privately circulated set of comments by Professors Rathjens, Weinberg and Wiesner, the only detailed criticism of the report which has yet appeared is that by Dr. Richard Garwin.[10] Some members of the Senate have requested written replies to a series of questions from a number of other specialists. These will be published in *The Congressional Record* shortly and will represent the most extensive evaluation of the report thus far undertaken.

Such a rough summary of early responses would be even less complete if it did not include two other types. Several senior scientific advisers who have not taken public positions on the ABM and several senior government officials in relevant departments have remarked that the report is biased and constitutes a further stage in the ongoing debate; they consider that it avoids the main issue and will, therefore, be quickly judged irrelevant and drop from view. The second type of response has been from several persons experienced in policy-making in other areas but not

[3] "The Obligations of Scientists as Counsellors: Guidelines for the Practice of Operations Research", Appendix IV-*H*, *Minerva*, X, 1 (January, 1972), pp. 154–155.
[4] Alsop, J., *The New York Times*, 9 November, 1971.
[5] McElheny, V. K., *Boston Globe*, 7 November, 1971.
[6] Letters to the Editor, *Boston Globe*, 19 November, 1971.
[7] Bartley, R. L., *Wall Street Journal*, 12 October, 1971.
[8] Letter to the Editor, *Boston Globe*, November, 1971.
[9] Panofsky, W. K. H., *Scientific American*, January, 1972, p. 6.
[10] *The Congressional Record*, 12 November, 1971, pp. S-18320–24. To be reprinted in the forthcoming issue of *Operations Research* (the journal of ORSA), together with a specially prepared rebuttal.

acquainted in a specialised way with the substantive details of ABM technology. From a careful reading of the report, these persons concluded that a strong case had been made for the shortcomings of the scientists criticised in the report. These two types of response—negative and positive —have not been put into written form and my impression of the distribution of these unwritten responses might have been different if I had encountered different persons. They are, however, probably representative of the types of view held outside the polarised groups of congressmen, members of the executive branch of the federal government and scientists who have been closely involved in the debate, and they therefore deserve consideration.

These impressions of the early response to the report are not meant to be a guide in any way to a more definitive evaluation of the report. But they do suggest the range of possibility and the difficulties which face anyone attempting to assess what might be learned from this effort to define " the obligations of scientists as counsellors ".

II

Two features make the assessment of the ABM debate, and the ORSA report as well, particularly difficult and complex. Yet it is these two features which make this a classic case for studying the interplay of scientific advice and governmental decision-making. The first feature is that none of the experts could be considered neutral in 1969 when President Nixon's administration proposed the deployment of the Safeguard system and the Senate was faced with the decision of whether or not to provide the necessary financial support for it.

Most disputes in public policy involving scientific evidence or prediction are settled at low levels. Others rise in public visibility and are sometimes judged by allegedly neutral bodies of experts such as committees of the National Academy of Sciences. A few others cannot be contained and eventually explode into public view as full-fledged conflicts. By this time most, if not all, experts have taken sides. The question of how to proceed beyond this point in the most rational manner has not been well answered and the examination of the ABM conflict may offer some suggestions.

The ABM controversy had its roots in the 1959 decision of Mr. McElroy, Secretary of Defense, to make the army responsible for missile defence. Many talented and dedicated officers were thereby committed to this mission. Since it was the only licence the army had in the new dimension of space it was certain to be pursued with alacrity. Despite the growing opposition of many technical experts in responsible government positions and on scientific advisory councils, successive systems were developed and tested. By 1966 the controversy had become a pitched battle. It culminated in a meeting which took place in the White House in January 1967. In addition to President Johnson, Secretary of Defense McNamara and the Joint Chiefs of Staff, there were present all past and current Special Assistants to the President for Science and Technology and all past and current Directors of Defense Research and Engineering.[11] " The place

[11] York, H., *Race to Oblivion* (New York: Simon & Schuster, 1970), p. 194.

where the buck stops" had been reached. The question was: "Will it work and should it be deployed?" The context was that of a country-wide defence against a Soviet missile attack. The answer from the assembled experts was "no": there was no dissent. There was, however, a minority favouring a thin ABM system oriented towards a hypothetical Chinese attack. The question of a Minuteman defence alone was not posed.

The controversy receded but was suddenly rekindled by the surprise announcement by Secretary McNamara in September 1967 that the United States would build a light, country-wide ABM system, known as Sentinel, to cope with a hypothetical Chinese attack. The reasoning behind this decision has never been made public. Without much debate, Congress soon approved funds for beginning the deployment of the Sentinel system.

It was only some months later when representatives of the army began to acquire land for missile sites near Boston, Chicago and other major cities that a public awakening occurred. By this time many of the government experts had left office where they had been unable to oppose the deployment decision: they moved quickly to join with groups of private citizens in urging Congress to halt the building of Sentinel.

Subsequent developments hardly need recounting. The Nixon administration, sensing the magnitude of the opposition and being, initially, uncritically responsive to Pentagon advice, focused on the protective role of Minuteman; the administration was supported by the build-up of large Soviet ICBMs which could with years of further growth and development put the American land-based Minuteman system of ICBMs at risk. The result was a regrouping of essentially the same weapons as were to have been used in Sentinel to produce the Safeguard system. It was this system which was the object of the Senate vote of August 1969 and which was subsequently deployed.

Considering this long history, the sudden shift of policy in 1967 and the public involvement in the debate, it is easy to see how by the late 1960s all the experts had been drawn into a polarised contest of such intensity that neutrality could not be preserved.

We come now to the second feature which gave this debate its distinctive character: the complexity of the issue, coupled with the fact that the debate was carried on at different levels. In parallel with many major policy decisions, the problem faced by the Senate of whether or not to appropriate funds for the deployment of Safeguard for Minuteman defence required working through several groups of questions in order. It is useful to specify these in detail, since they also correspond to the different levels at which the debate proceeded.

(1) *Assessment of Need*: Here one had to ask if the American land-based ICBM force (1,000 Minutemen and 54 Titans) would become vulnerable to almost total destruction in the foreseeable future of roughly five to ten years. Clearly the answer depended on the outcome of several subsidiary questions. First, what different estimates or assumptions of the growth of Soviet forces, both in numbers of missiles and payload capability, should be considered for the next decade? Secondly, what would be the vulnerability of the American force to each of the assumed growth patterns of the Soviet force? Thirdly, to what extent would the bomber

and submarine parts of the American deterrent force become vulnerable and to what extent would the American tripartite deterrent force remain capable of launching a retaliatory strike of unacceptable intensity?

(2) *Assessment of the Adequacy of the Solution*: If the protection of some or all of the land-based missiles and some or all of the bombers is shown to be necessary, one must then ask if the proposed solution, in this case Safeguard, is adequate. The reply to this question requires careful analysis of the effectiveness of the composite system against the variety of attack options and tactics available to the Soviet Union. Such analysis must include the degradation in performance which may be produced by the environment of a nuclear attack, the special vulnerability of the radars, probability of maintaining the system in a state of instant readiness indefinitely and the ability of the attacker to exhaust the defence. After defining as well as possible the range of threat for which Safeguard may be effective, the cost must be estimated and compared with alternative proposals for maintaining a roughly equivalent deterrent force.

(3) *Assessment of the Political Value of Acceptance of the Solution*: The criterion at this level is largely political rather than technological; the aim is to estimate the net benefits of the solution in terms of national security, diplomatic advantage and domestic needs and pressures. In the context of the ABM decision the question is: will having Safeguard help attain a political goal which is worth the price and risk? This question remains urgent in all circumstances short of a decisive negative answer to questions (1) and (2). The less affirmative the answers to questions (1) and (2) are, the greater the burden which is placed on the political perception of the decision-maker and the more he risks turning diplomacy and military posture into bluff.

III

Viewing the ABM debate retrospectively in this framework one cannot avoid observing that the pro-ABM scientists concentrated on questions of the first type, *i.e.*, need, while the anti-ABM scientists focused on questions of the second type, *i.e.*, adequacy of the solution. Meanwhile, the administration was not seriously challenged on its political decision (type (3) question), the only articulated basis for which was the need for Safeguard as a " bargaining chip " in SALT.

The pro-ABM scientists argued that the American ICBMs could soon be in danger, that American security demanded the retention of all three components of the deterrent (ICBMs, submarines and bombers) and that Safeguard was the only available response to the growing Soviet SS-9 force which was endangering the ICBMs. By concentrating the force of their arguments on Minuteman vulnerability they left the technical merits and shortcomings of Safeguard essentially unexamined.

The anti-ABM scientists argued that a linear projection of the Soviet arsenal of SS-9s, coupled with increased accuracy and the use of MIRVs, would ultimately lead to the obsolescence of Minuteman, perhaps in a decade. But they insisted that this would not give the Soviet Union the equivalent to a first-strike capability since the other parts of the American

deterrent could not be simultaneously attacked: that enough was certain to survive to devastate the Soviet Union. More important, however, the opponents of the ABM concentrated their fire on the ineffectiveness of Safeguard in providing significant protection to Minuteman, even if it performed optimally.

Turning now to the ORSA report, and particularly to Appendix III which makes up the bulk of the report,[12] we find that the investigation does not deal with all these three levels of the debate but is limited almost exclusively to questions of type (1). Even its treatment of questions of type (1) is incomplete, for it does not examine the adequacy of the tripartite deterrent when one part fails. This limitation of scope not only imposes severe restrictions on what is being examined but prejudices the outcome because it cuts out most of the ground on which anti-ABM arguments were made and gives undue emphasis to the narrow terrain on which most pro-ABM arguments rest.

Although it is not my purpose to examine the many findings of the committee which seem to be open to question, two of them are sufficient to illustrate what appears to be a lack of impartiality and comprehensiveness in the investigation. Perhaps the most dramatic finding was the difference in the numbers of ICBMs calculated to be able to survive a Soviet first-strike attack using the 420 to 500 SS-9 missiles it was then predicted they might have in 1975. Clearly such a calculation depends on the assumptions one makes about the number of re-entry vehicles per missile, their accuracy, their megatonnage, the tactics employed by the attacker and the hardness assigned to the ICBMs attacked. Since the range of reasonable choice for these variables is considerable, it is understandable that no great weight should be attached to any particular set or the result which is derived therefrom. That two different assessments should produce figures as different as 5 per cent. and 25 per cent. surviving ICBMs is not surprising. If the assumptions had been the same the calculated results would have been the same: high school mathematics are sufficient for the calculation. Professor Wohlstetter considered the difference a matter of high principle: Professor Rathjens looked upon it as a "back-of-the-envelope" calculation. After identifying the assumptions used and some small errors made by Professor Rathjens, the report praises Professor Wohlstetter for doing his "homework" correctly and criticises Professor Rathjens rather severely for his errors and the bias which the committee found in some of his assumptions.

Having been treated in so much detail, this incident assumes the importance of a pivotal point in the debate. Yet the committee fails in its obligation to put the calculation in perspective. It does not suggest that the choice of 500 missiles made the best possible case for Safeguard effectiveness.[13] A significantly smaller number would leave too many ICBMs surviving and any significantly larger number would so overwhelm the Safeguard system that it would be useless. The critical role of the

[12] "The Obligations of Scientists as Counsellors: Guidelines for the Practice of Operations Research", *Minerva*, X, 1 (January, 1972), pp. 118–151.

[13] The "Guidelines" in the body of the report recommend that analysts should "check the sensitivity of the results to variations in assumptions and inputs. . . ."

choice of 500 goes even further. Since the effectiveness of Safeguard is sharply peaked at this number, then it is clear that an adversary could overwhelm it by waiting until he had a larger striking force. The main point is that, in an environment of constantly growing forces, Safeguard offers *at best* a brief period of marginal effectiveness. Whether this period would occur in about 1975 or in some later year is very uncertain. This is a situation which occurs repeatedly in technically based decision-making. The fine tuning of a calculation involving a number of parameters having substantial uncertainties is seldom justified, particularly if it obscures larger issues. Yet Professor Rathjens is taken to task in the report for being inattentive to small details, while a testimony which consciously avoids the main point—that Safeguard can be effective against only a very narrow band of transient threats—is praised.

Indications of dual standards can be seen in many other places. An interesting example is the report's treatment of two tactical options which might possibly be used by the Soviet Union if it were planning a first strike. One of these involves re-programming—the re-targeting of some missiles to place those which failed to function in the original salvo. The inclusion of such a tactic improves the hypothetical destructiveness of a Soviet first strike when large weapons are used. Since it supports the point Professor Wohlstetter was making, the inclusion of this tactic in his assumptions is understandable, although comparable choices by Professor Rathjens are criticised. But consider the justification which Professor Wohlstetter uses for assuming that the Soviet force has this capability. In this testimony he states: " There are very familiar, well-known methods of arranging it so that you can re-program missiles to replace a very large proportion of your failures. . . ." Professor Rathjens replied: " There is no basis that I am aware of for believing the Soviet Union employs such a technique, and I do not believe we do ". The report agrees, but allows the assumption since Professor Wohlstetter " does not claim that either we or the Soviets have such techniques now ". The standards which Professor Wohlstetter must meet could hardly be lower. To help him, the report devotes nine pages to suggestions of how re-programming might be done. Yet it does not admit the central point: that this " quite likely tactic " requires selection of standby missiles, the switching of their targeting instructions and possible internal readjustments, all within seconds. This very heavy additional burden for computer and guidance systems would require extensive tests in salvo firings before the confidence level of 95 per cent. single warhead kill probability assumed by Professor Wohlstetter could be achieved. It is the great cost and the near impossibility of achieving and maintaining high reliability for this tactic which makes it so unlikely; the necessary but insufficient numerical criteria elaborated in the report to legitimate this assumption miss the point. It is systems analyses and good judgement, not operational research, which are needed.

Compare, now, the way that this assumption was handled with the treatment given to Professor Panofsky, who had argued that it was unreasonable to assume that in a first strike the Soviet Union could force the United States to hold Minutemen in their silos while bomber

bases were being attacked by exploding submarine-launched warheads over the Minuteman fields. Professor Panofsky concludes his argument by saying:

> [Such an attack] . . . would require an enormous increase in the numbers of Soviet missiles, their accuracy, and in the confidence the Soviets would have to have in their system. Moreover, the SAC fleet would have to remain on the ground as " sitting ducks " even in times of stress, that is, they would have to be not on airborne alert; moreover the SAC airfields would have to be within reach of the Soviets' SLBMs [submarine-launched ballistic missiles] a fact that we are presently changing. . . . We are giving the Soviets credit for a degree of performance and reliability of military systems which we could not dream of achieving ourselves.

The committee was unimpressed and formulated the unrealistically high standard which Professor Panofsky (and Professor Wiesner) should have met: " Those who wish to challenge the possibility of a pin-down attack by the Soviets must treat *all* reasonable tactics that might lead to pin-down, and demonstrate that *none* of them will succeed." Just as ABM systems can always be exhausted by being presented with more incoming warheads than they can handle, so can scientific witnesses if they accept the charge to evaluate all kinds of " contrived threat which totally ignore the kind of realities the Soviet planner would have to face ".

The gap between the capability which the Soviet Union would have to have to employ this tactic with confidence and that which they are estimated to have in the foreseeable future is very great. Only arguments using classified data could elaborate this in detail, but one can be confident that the United States Department of Defence is not so derelict in its duty as to have allowed this kind of vulnerability to develop. This attitude of allowing maximum capability on one's opponent's side and minimum capability on one's own, like the asymmetry between capability and intent, permeates all strategic debates and is probably unresolvable by any professional committee. Being unresolved it offers a tool, perhaps unconsciously used, to impose an unacceptable demand for precise discrimination on technical witnesses.

IV

Let us now turn to the propriety of the ORSA investigation and examine the extent to which this effort might affect the conduct of public policy debates and the technical advice which they require. It is evident upon examining Appendix III that what took place in some ways resembled a judicial procedure. The findings read like a judgment and the potential impact on the careers of those " found guilty " could be substantial. Yet this was carried out without any prior communication of rules, without a prior limitation of jurisdiction and without any provision of safeguards. These features are considered necessary in a court and even more so in an *ad hoc* proceeding where the reputations of individuals are at risk. Even if one argues that justice was done in this case, the precedent is set for other investigations of this kind, any of which may bring substantial harm to individuals whose conduct and, hence, whose ethics, since ethics are

the complex of rules that govern conduct, are publicly judged by a group to which they do not belong and on which they never conferred such prerogatives. Only the state with its judicial apparatus can claim such powers.

The authority which ORSA and its *ad hoc* committee presumed to have in this investigation derives solely from itself. None of the six persons [14] whose conduct was found at fault was a member of ORSA; Professor Wohlstetter was. None of the six considers himself to be engaged in operational research. More importantly, almost none of the individual points investigated involved matters obviously within the domain of operational research. Instead the questions which led to the severest judgement dealt with how a graph was read or misread, or whether certain data or others were employed. Distinctions such as that between 500 and 600 are not matters requiring an expertise in operational research. Nowhere except in the 10-page section supporting Professor Wohlstetter's assumption on reprogramming can one see any requirement of the professional discipline. Hence one must ask why a group of persons who are not members of an operational research society should be investigated by a group who are, on matters which do not require an expertise in operational research.

Many, perhaps most, professional societies have faced the problem of propagating a standard of professional behaviour, particularly with regard to the professional–client relationship. The "Guidelines" which are set forth in the first 10 pages of this report—the rest consists of appendices—appear to be a sound statement of standards of procedure and conduct for operational research workers doing work on contract and serving a client as advocate. If the appendices dealt with the application of these standards in typical situations the result would be professionally useful and conform to the practice of other professional societies, which, in setting professional standards of conduct, confine themselves to issues which are sufficiently narrow and central to the discipline to command a nearly unanimous consensus in the profession. Thus the ORSA investigation is a radical departure from the traditional roles of professional societies and is in principle in conflict with the due process of law.

These considerations lead me to conclude that it is unlikely that the quality and effectiveness of scientific advice to government will be improved by investigations of the type undertaken by the ORSA committee, although some persons may indeed do their sums more carefully as a consequence. Even if such procedures were modified so as to avoid infringement of legal guarantees, they would be harmful to the process of advice and debate because they empower a professional group, no matter how narrowly constituted, to apply its particular standards to issues which are almost certain to be very much larger. Yet the general respect which professional societies still receive from the public is such that the condemnations arising from such procedures would be widely accepted and the persons who would be willing to be judged publicly by such standards would become fewer.

[14] At least one of the six, Professor Panofsky, received no notification of the investigation until he was sent the report.

V

How then, can the contributions of technical experts to such important policy debates be made more responsible? How can conflicting conclusions involving scientific concepts and analyses be fairly resolved or understood in time to be useful? And how can one ensure that a reasonable balance of attention be given to both the arguments which depend on detail and the wider issues which depend on experience, judgement and perception of the political context? Let us consider only those few major issues such as that concerning the ABM in which the debate becomes extensive and the technical component is substantial. The SST debate is in this category, as are the debates of the early 1960s over a nuclear test ban and their predictable recurrence in the near future when a complete test ban will be considered.

Ideally such conflicts should be resolved and some approach to a consensus reached before congressional hearings begin. This consensus should be performed in the relevant government departments or agencies and the scientific advisory committees, and through their interplay. When properly appointed, these committees form a

> parallel communication network within the federal government which to a very considerable extent circumvents the customary bureaucratic channels. In science and engineering no level of the bureaucracy has a monopoly on new ideas, and the loose nature of the advisory system provides one means by which ideas originating at a low level in the bureaucratic structure can be brought directly to the point of decision without going through regular channels, and new ideas from outside the federal structure (or its contractors) can be introduced quickly into governmental operations.[15]

When this system fails to bring about consensus, the administration generally recommends its choice among the alternatives to Congress and the debate develops in hearings before the appropriate committee(s). The content and value of the hearings is then shaped by the work of the staff of the committees and the choice of witnesses invited to testify. While experienced and competent staff members can arrange very useful hearings on many bills within their normal range of work, the issues of large scope, such as the ABM, involve matters, especially technical matters, with which they cannot deal adequately. The selection of witnesses can be done in such a way as to bring out the important differences and to aid in finding compromises. But in issues about which opinions have already become highly polarised and politicised the choice of witnesses is likely to be strongly influenced by those members of the committee who are acknowledged exponents of one or the other of the polarised viewpoints. This allows little opportunity for introducing less partisan testimony, it polarises the debate further and converts the taking of testimony into an adversary proceeding. Obviously it was this kind of situation, and the inevitable haste, which contributed to much of the unnecessary misunderstanding in the ABM hearings.

Since the hearings in such cases are so close to adversary proceedings

[15] Brooks, H., *The Government of Science* (Cambridge, Mass.: MIT Press, 1968), p. 82.

in spirit, there are recurring suggestions to go all the way and introduce the actual procedures of a court of law. The attractiveness of this diminishes, however, when one appreciates that this would require a congressional committee to immerse itself in the technical details as a judge does. Given the tasks and schedule requirements, such a process would be ludicrously cumbersome and would paralyse decision-making. Moreover, any reform of existing procedures should encourage convergence rather than intensify the polarisation of viewpoints.

Perhaps the most practical way of improving the advisory process at this stage is to build on the process which keeps scientists honest and relevant in their professional lives. That is, a means should be found to have witnesses confront peers of equal competence. To be specific, congressional committees, after being formed in each two-year congressional period, and assessing the major issues which are likely to come before it, could, with adequate advice, engage a small and balanced group of consultants of acknowledged technical competence and reputation for the remainder of the congressional session. As consultants they would agree not to engage in public discussion or serve on other advisory committees on subjects which would overlap with or touch on those which are expected to come before their particular committee.

In actual operation a consultants' panel would be selected for a given set of hearings. These consultants would advise the staff on witnesses, organisation and schedules. They could suggest the most useful form of testimony and specific questions which should be put to the witnesses. At the hearing itself they would be able to question each witness and require written answers to questions that could not be dealt with in the hearing. In cases of continued conflicting testimony they could recommend additional sessions and the questions which should be pursued further. After the hearings they could meet with the committee in both open and executive session to discuss their evaluation of the presentations and provide a written summary.

Of course, the success of such a course depends decisively on choosing and enlisting as consultants scientists of considerable experience and stature so that the witnesses will feel that they are being judged by their peers. In some circumstances it might be possible for the panel of consultants to meet with witnesses prior to testifying to resolve issues which are obviously due to misunderstanding or insufficient data and thereby avoid wasting time in the hearings proper.

Another device which might prove useful and which would rest on the judgement of peers is one by which the chairman of a committee of the Senate or the House of Representatives requests an external body, such as the National Academy of Sciences, to provide an advisory report in an area of potential committee concern. This was done in 1965 and 1967 by the House Committee on Science and Astronautics, which requested reports on *Basic Research and National Goals* and *Applied Science and Technological Progress*.[16] In both cases the panels were chosen to represent

[16] Reports to the Committee on Science and Astronautics, US House of Representatives, by the National Academy of Sciences (Washington, DC: US Government Printing Office, 1965 and 1967).

a balance of views. Individual members were asked to prepare papers outlining their own views on specific topics but were then required to present and defend them *in camera* before the entire panel. As a consequence there was considerable revision of papers as a result of this confrontation of peers. The conclusions prepared by the staff and the chairman clearly reflected the quality and conviction of the arguments before the entire panel. These recommendations eventually became the basis of legislation reorganising the National Science Foundation. Although the subjects in these instances allowed a more leisurely approach than issues such as the ABM might, the usefulness of such a procedure and the balanced and judicious quality of advice which it can produce justifies its being tried on other major issues. For example, one can anticipate that a decision on a complete nuclear test ban may come before Congress in the next session. As before, an enormous amount of seismological and other data will be involved and used. Early in the next congressional session, the appropriate committees could request an examination of this problem in the form just outlined. The panel which carries out this task, or a part of it, could then also serve as consultants on hearings dealing with the administration's proposals. Such a procedure could be much more effective and much less abrasive in dealing with an issue which may have the emotional potential of the ABM.

Both of these proposals have the additional virtue of increasing the contact between congressmen and scientific and technological advisers. Moreover, they do so in a manner which exhibits the procedures and modes of reasoning which are used in science itself. A scientist recognises that his standing in the scientific community rests on the degree to which his research is verified by subsequent events and provides results which other scientists can build on. A scientific adviser will function best if his influence in government rests on the degree to which his advice is vindicated by subsequent events and provides results which others can build on.

VI

We can now return briefly to the questions of type (3), referred to earlier. It was suggested that the ultimate form of the political decision in this case was: will having Safeguard help attain a political goal which is worth the price and risk? And it was further remarked that technical considerations would only affect this decision if such arguments decisively showed a lack of need—type (1)—and an inability to do the job—type (2). In my view this was indeed the situation until late 1967 when the invention of defence against a small Chinese attack so lowered the system requirements that Secretary McNamara could say: "There are marginal grounds for concluding that a light development of US ABMs against this probability is prudent."

Until this time the Soviet position had been firmly against any limitation of ABMs, on the grounds that it was solely defensive and could not produce the serious destabilising effects which many American strategists claimed. Ironically, it was just at this time that Soviet opinion began to shift, greatly aided, I believe, by private discussions which had taken place

over several years. By the spring of 1968 the possibility of negotiations on limiting ABMs and offensive missiles seemed to have become possible. Plans to do so were upset by the Soviet intervention in Czechoslovakia in August 1968: negotiations did not actually begin until November 1969.

While it is far too early to see events since that time in perspective, a subjective interim assessment may be of some interest. For simplicity, consider what each side has to its credit. Those advisers who favoured ABM can point to the deployment of Safeguard at two sites and the authorisation for two more sites. If a SALT agreement is reached it will probably permit the retention of some of the Safeguard system. If a SALT agreement is not reached, pro-ABM advisers will be able to recommend an improved system (hard-site defence) which would be more cost-effective in the protection of land-based missiles. And the option of returning to the advocacy of country-wide defence and a heavy urban defence will remain.

Those advisers who have opposed ABM have nevertheless won important contests. They played a major role in causing the country-wide defence to be abandoned. The first step was downgrading it from a major role in the Sentinel system to a lesser role in the original Safeguard system. The second step came in the revival of the ABM debate prior to the 1970 Senate vote on provision of funds. Here Senator Stennis was persuaded of the shortcomings of the area defence and took the lead in cutting out that part of the programme. Beyond these specific successes is the change in climate in the Congress with respect to proposals by the administration for new complex systems mentioned in the quotation from *Newsweek* at the beginning of this article. Indeed, the defeat of the supersonic transport in 1970 is claimed by many to be a consequence of the new sensitivity of Congress to the adequacy of the technological assessment of new complex systems—military or civilian.

But in the long run, those who have opposed investing in ABM in principle have further grounds for satisfaction. The consequences of deploying a heavy ABM system on either side—and then ultimately on both—are widely appreciated and accepted throughout the world now, whereas five years ago they were not. Although initial SALT agreements may preserve more ABM capability than existed when the talks began, it will still be a relatively small part of what might have been built eventually. And if SALT fails it is possible that these more realistic views will cause similar limits to be self imposed. If this interpretation is correct, the ABM debate has contributed to the precious store of world sanity. Those who invest their energies in such arcane pursuits can expect no more, regardless of their individual positions.

VII

In closing, it may be useful to try to look beyond the possible improvements in the methods of scientific advice to the more fundamental limitations which will keep it imperfect no matter how adequate the means or how honest the advisers.

Operational research was born and came of age in the Second World War. Its techniques were based on gathering operational data under battle conditions, using these data to narrow the uncertainties of assumptions or parameters so that an analysis of how a system or tactic was operating could be used to make militarily useful predictions.

These techniques were soon developed into one [17] which found wide utility in many diverse ways in both civilian and military problems where extensive operational data were available. But the transfer of this technique to strategic problems in the nuclear age has been another story. Nuclear weapons have not been delivered and exploded between nuclear powers. As a result there are no operational data from which to narrow the uncertainties of assumptions or parameters. This applies to the effectiveness of the other side's strategic forces, since intelligence is never complete or trustworthy, and to the effectiveness of one's own, because they have never been used in a nuclear war.

Without " battle tested " data, such as the space and time distribution of bombs falling in a given target area in the Second World War, the technique of operational research could not be applied in the traditional ways. The alternative which developed, often under the same name, was the use of assumptions instead of operational data. The mathematical analyses could proceed, increasing in complexity with each successive generation of computers, to highly quantified answers. The uncertainties in the assumptions increase again in analyses such as the ABM because of the range of tactics the attacker can use to defeat the defence. Ultimately, the question must be faced as to how useful are quantitative calculations with such highly uncertain assumptions. As an aid in design, in planning tactics and in giving some guidance to estimates of capacity, they do have value. But in the hard decisions—to build or not build the system—judgement based on experience and exercised without bias must be the final arbiter for rational and responsible men.

Yet, if all this were agreed, there would remain unresolvable differences in judgement. It is an inseparable part of the human condition that men are conservative or liberal, " hawk " or " dove ", hopeful or pessimistic, confident or suspicious. In this deeper, human sense, the contest in which the ORSA report is involved has been played before and will be played again. In the late 1950s, Professor Wohlstetter was a leader in the discovery of our new " vulnerability ", arguing this with great persuasiveness in a paper [18] entitled " The Delicate Balance of Terror ". One of his main points was that ". . . it takes great ingenuity at any level of nuclear technology to devise a stable equilibrium ". P. M. S. Blackett, among others, set about to demolish this " delicacy " thesis and ended his essay with these words:

More important is the possibility that the arguments which have been, in my view, falsely used to prove the balance unstable in recent years may be used in the future to prove it again unstable, in spite of the expected

[17] Morse, P. M. and Kimball, G. E., *Methods of Operations Research* (New York: J. Wiley, 1950).

[18] Wohlstetter, A., *Foreign Affairs* (January, 1959).

improvements of weapons. So the truth or falsehood of the delicacy thesis will remain for many years of vital importance.[19]

It was a perception of the delicacy of the balance and vulnerability of Minuteman which underlay much of the argument for the ABM. In this context I am led to conclude that, despite its concentration on matters of detail and its avoidance of the larger issues, the ORSA report, intentionally or unintentionally, entered into doctrinal dispute in the course of applying a quantitative technique based on quite uncertain assumptions.

Universities, if they are alive, are rife with doctrinal disputes. By long experience they have learned that their existence, their freedom and their usefulness depend on limiting investigation of their members to the most elementary and obvious forms of "grave misconduct". Professional societies should limit themselves in the same way. Of all professional groups only the church pretends competence in settling doctrinal disputes and even then with the expectations of divine guidance and, it would seem, with mixed results.

[19] Blackett, P. M. S., "Critique of Some Contemporary Defence Thinking", *Encounter*, XVI, 4 (April, 1961), pp. 9–17.