



Operations Research

Publication details, including instructions for authors and subscription information:
<http://pubsonline.informs.org>

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To cite this article:

(1972) Reactions to the Guidelines for the Practice of Operations Research. *Operations Research* 20(1):205-246.
<https://doi.org/10.1287/opre.20.1.205>

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Letters to the Editor

Reactions to the Guidelines for the Practice of Operations Research

The September 1971 issue of *Operations Research* (volume 19, pp. 1123-1258) was devoted entirely to a report of the ORSA Ad Hoc Committee on Professional Standards entitled "Guidelines for the Practice of Operations Research." In addition to the main body describing the guidelines (pp. 1127-1137), the report contained four appendixes, the first on the nature of operations research (pp. 1138-1148), the second reproducing a report of the Senate Subcommittee on National Security and International Operations devoted to conflicting testimony of scientists involved in the 1969 ABM debate (pp. 1149-1173), the third setting forth an analysis by the Ad Hoc Committee of the treatment of certain operations-research questions in this 1969 debate (pp. 1175-1245), and the fourth presenting relevant correspondence and comments (pp. 1246-1258).

The report has attracted considerable attention: the Editor has received a substantial number of letters dealing with it, and the press has given considerable coverage to the ABM-debate analysis in Appendix III; Congress has also paid considerable attention to the report, as the amount of material on it introduced into the *Congressional Record* testifies.

Our principal purpose here is to record the main points made by our many thoughtful correspondents; since the press response has already received fairly wide circulation (and all of its principal items have been reproduced in the *Congressional Record* since October 1, 1971) we shall only list them. The letters received by the Editor have, without exception, been unsolicited.

After a brief section listing the main points in the letters, and where they can be found, the letters appear, arranged alphabetically by the author's surname, with four exceptions. (Since a number of persons circulated their letters to others, several replies were submitted; thus, in four cases replies—starred in the following list—are placed directly following the letter being responded to.) The authors of the 19 letters published here are:

Truman Botts
Douglas L. Brooks
* Leslie C. Edie
T. E. Compton
Richard L. Garwin
* Amoretta M. Hoeber and David C. McGarvey
Saul I. Gass
* Hugh J. Miser (I)
W. A. Golomski
Paul Gottfried

Robert D. Kaiser
 George B. Kistiakowsky, Herbert Scoville, Jr., and Herbert F. York
 * Hugh J. Miser (II)
 R. Stanley La Vallee
 Philip M. Morse
 Thomas W. Sikes
 Walmer E. Strobe
 Gifford H. Symonds
 Michael J. White

Finally, there is a chronological listing of the principal news stories and articles that have appeared in other publications, including references to those that have been reproduced in the *Congressional Record*.

THE MAIN POINTS OF THE LETTERS

The letters received make many penetrating and valuable points; the selection printed in what follows is designed to exemplify the main points of all by including effective presentations of them.

For the letters printed, the following listing—while not complete—gives one a quick guide to where the main points are discussed (the letters, of course, make many other points not included in this listing).

- The value of the guidelines to the operations-research profession: *Botts, Brooks, Compton, Sikes, Strobe, Symonds, White*.
- The value of the analysis of the professional standards exhibited in the ABM debate: *Botts, Gottfried, Hoeber/McGarvey, Kaiser*.
- How the ABM-debate analysis was handled administratively: *Brooks, Garwin, Gass, Miser (I)*.
- Whether ORSA should have investigated the professional activities of nonmembers: *Brooks, Edie, Gottfried, Kaiser, Kistiakowsky et al.*
- The adequacy of the ABM-debate analysis, and whether or not it should have been extended: *Brooks, Garwin, Hoeber/McGarvey, La Vallee, Miser (II)*.
- The implications of peace-time analysis for war-time effectiveness, and hence for such strategic estimates: *Morse*.
- Whether adequate safeguards were in place during the ABM-debate analysis: *Garwin*.
- Some participants committed the faults they accused others of: *Garwin, Hoeber/McGarvey, La Vallee*.
- Should the Committee have developed guidelines for professional practice and applied them as part of a common effort: *Gass, Kaiser*.
- The professional standards of the Report: *Kistiakowsky, et al., Miser (II)*.
- The public impact of the report: *Morse*.
- Whether, as contended by the “Minority Statement” (Appendix IV H, pages 1255-1256), the report should have been ‘refereed’ further: *Brooks, Gass, Kaiser, Miser (I)*.
- The adequacy of the “adversary-process” portion of the guidelines: *Kaiser, Symonds*.
- Whether the entire effort was of interest and value to the operations-research profession as a whole: *Golomski*.

- The need for guidelines of wide applicability to govern the activities of scientists testifying on public-policy issues: *La Vallee*.
- How the Society and its Council should approach professional-standards matters in the future: *Brooks, Edie, Gass, Gottfried*.
- The Society's open posture with respect to its future behavior in professional-standards matters: *Edie*.

THE LETTERS

Truman Botts, *Executive Director, Conference Board of the Mathematical Sciences, Washington, D. C.*

(a). From a letter dated October 19, 1971:

... the ORSA committee has taken the occasion of a dispute arising out of conflicting operations-research testimony on the ABM to produce, as the main body of its report, an impressively balanced, conscientious and responsible set of guidelines for the professional practice of operations research.

Confronting head-on the difficult problem of the proper role of the operations analyst in an adversary process of the sort that arose in the ABM testimony, these guidelines will unquestionably prove of great value to the practicing operations analyst, who is increasingly called upon these days not only to perform analyses illuminating major decision-problems of business, industry and government, but also to present the results of such analyses in debate within government agencies or in such formal adversary proceedings as a court case or a Congressional committee hearing.

... In publishing these guidelines along with a detailed appendix analyzing how they applied, or should have been applied, in the instance of the 1969 ABM debate, the Operations Research Society of America has performed a valuable service to its profession. It took courage to do it.

(b). From an editorial in the *CBMS Newsletter* (October 1971, page 9):

... In undertaking this study and developing these guidelines the Operations Research Society of America has confronted with integrity and wisdom a difficult issue of vital professional concern.

Douglas L. Brooks, *Bethesda, Maryland*.

... I find myself in general agreement with the ... report proper, [the guidelines on] ... pages 1127-1137. It is thoughtful, sound, and should be of value to both practitioners and clients of O/R in judging professional conduct and forming realistic expectations. Its publication and wide dissemination by ORSA are appropriate. The Committee deserves applause for its preparation and the Council for its publication.

Appendix III ... is an entirely different matter. *I wish most strongly to disassociate myself from it and from the proceedings that led to its publication.*

My reasons are similar to those expressed by the statement (pages 1255-1256) of the Council minority that opposed its publication. I feel that:

- The results of any investigation of Wohlstetter's charges of professional

misconduct on the part of his opponents in the ABM debate should have been reported only to the Council, as the Council had originally directed (page 1248).

- Since his opponents were not members of ORSA, nor professional operations analysts, nor did they claim to be, the investigation of their testimony should have been restricted to that necessary to derive and test a clearly stated code of professional conduct and guidelines to members of the profession for avoiding any mistakes or types of malpractice evidenced by the participants in the ABM debate. [See the Editor's Note.]

- Wohlstetter should have been invited to argue the case for the ABM as a technical issue appropriate, when properly refereed, for the pages of the Journal or of some other professional publication. Committee members could have done the same, *as individuals*.

Among other things, this would have avoided the wholly gratuitous and—to my mind—disgraceful attack in public on experts in other fields in the name of a profession, and of a professional society, to which they did not, and did not profess to, belong on the basis of trivialities which, as discussed below, were only peripheral to the ABM effectiveness assessment at the time.

I hope someone, preferably a member of the Council, will introduce a resolution, on which the membership can vote, aimed at preventing a repetition of this lamentable exercise.

In addition to gravely overplaying its role, the Committee badly muffed its own evaluation of the ABM effectiveness debate. They did so by committing the cardinal sin in operations research, choosing an inappropriate framework for their analysis, thereby (ironically) imitating the participants in the debate. By choosing to evaluate the debaters in terms of whose calculations of the vulnerability of Minuteman to the SS-9 threat were most official, honest, and accurate, they lost sight entirely of the reason for making the calculations in the first place; namely, to assist planners in determining how much, if any, additional protection in the form of Safeguard should be provided the Minuteman component of the nation's deterrent weapon system.

This choice is all but inexplicable, since the "requirement for the defense of Minuteman and bombers as part of the total U.S. deterrent force" is explicitly mentioned on page 1179 as one of three major issues in the debate the Committee originally considered for possible examination. In deciding merely to "touch upon" this issue and to make the "vulnerability of minuteman and bombers without additional protection" the major subject of discussion, the Committee made a professional misjudgment of the first magnitude.

There is evidence that at least some of the members of the Committee may have recognized this. I refer to the section on pages 1236-1237 of the Report which starts: "A particularly important interaction that has not been treated properly in this debate is the problem of the amount of defense of Minuteman believed adequate to preserve, *in combination with other survivors of a Soviet first strike*, a believable surviving U.S. second-strike force," and ending: "The failure of this public exchange of views to deal with this question, whether in conformity with the wishes of the Executive

or Legislative branches of the government, is a basic failure to deal with a most fundamental issue.” (My italics.)

I only regret that the Committee, having gone that far, did not go a little farther and look in more detail into the significance of this failure. If they had, I believe they would have found that in this perspective the small differences in the computations of Minuteman vulnerability were of no real significance. . .

The virtual impossibility of making precise predictions of the performance of hypothetical weapons at some fairly remote future time led me to conclude long ago that measures of merit for deterrent weapon systems must, among other things, be “free from dependence on precise details of the weapons systems performance.”¹ Our deterrent-system mix had this property a decade ago, thanks most prominently to the fleet of Polaris submarines, and I believe it has it now. The important job for planners and analysts is to keep it that way.

Choosing the appropriate framework for an evaluation is such a critical step for an operations analyst, and particularly so in the strategic weapons area, that the failure of the debaters in this respect makes the issues examined in the remainder of Appendix III seem petty and the great attention paid them by the Committee sheer scholasticism. If the Committee had worked the measure of merit issue through and given it its proper weight, I believe it would have been tempted to recommend on these grounds alone that the Council take “a-plague-on-both-your-houses” position regarding the debaters. This would have been still another reason to refrain from any official sponsorship of the evaluation’s publication, and another strong argument for throwing the ABM debate back to the debaters to handle as a technical matter like any other subject of professional interest about which views differ.

I hope no irremediable damage has been done either the individuals involved or the Society. With regard to the latter, I again recommend a Council-sponsored resolution concerning the proper handling of future incidents of a similar nature.

Notes

¹ D. L. Brooks, “Choice of Pay-Offs for Military Operations of the Future,” *Opns. Res.* 8, 159-168(1960).

Editor’s Note: At least one of the “opponents,” George W. Rathjens, claims “operations analysis” as a professional specialty; see *American Men of Science*, 9th Edition, page 1574(1955), 10th Edition, page 3301(1961), and 11th Edition, page 4340(1967). This claim is not changed in an amended listing in the 1970 supplement, page 574.

Leslie C. Edie, *Vice President of ORSA, New York, N.Y.* (a reply to Douglas L. Brooks).

I cannot agree that public criticism of professional work, whether made by individuals, representatives of a professional society, or a committee appointed by a professional society, should be restricted to those who claim to be members of the profession. I believe it quite proper for a committee of the American Statistical Association to criticize the statistical methods of Kinsey or Rhine whether or not they claim to be statisticians, or for astronomers to have publicly criticized Velikovski's theories about the solar system when he didn't claim to be an astronomer.

Because the opponents of the ABM system were not members of ORSA you say "the investigation of their testimony should have been restricted to that necessary to derive and test a clearly stated code of professional conduct and guidelines to members of the profession for avoiding any mistakes or types of malpractice evidenced by the participants in the ABM debate," I agree with you completely, and believe that your statement above is an excellent description of what was done . . .

It is my impression that all members of the Ad Hoc Committee recognized the limited nature of the material covered and its relative insignificance to the broader picture of second-strike deterrence in combination with other survivors of a Soviet first strike. You point out a statement to this effect on pages 1236-37. Since the report was unanimously approved by the committee, I believe all members recognized this. I'm also sure they all realized that the objective function of our national defense is certainly not the percent of Minuteman missiles surviving a Soviet first strike. Those suggesting that the committee erred in this way failed to read the report as carefully as you did.

I'm sure you realize that had the committee taken on the greatly expanded investigation of testimony given on all aspects of U.S. defense systems, it would have had a task far beyond its means. The investigation had to be limited in some way, and even with this limit, a 63-page critique resulted. You and others still may have a good point that it should have been broadened, but I doubt that the reactions to the report would have been any different had it been made more comprehensive.

The important question it seems to me is how we, as a professional society, should deal with future charges of unprofessional conduct of operations research by members and nonmembers of the Society, particularly when public issues are involved. I share your hope that no irremediable damage has been done either the individuals involved in the ABM debate or the Society. But what has been done will not be changed by any amount of discussion. I hope the Society will not be split into bolsheviks and mensheviks by what has taken place. I would like to see both groups work together in finding a policy on critiques of professional standards, a policy that most of our members will support.

The reason for the questionnaire used at the Anaheim meeting was to get an expression of members which could be used in determining the policy to be followed by incoming Councils of the Society. I feel that before the Council, or some member, drafts a resolution or some other statement such

as a Constitutional amendment, a better sense of how most members feel is needed. I don't think it would be wise to rush into print with some resolution prepared during the heat of controversy. It is not immediately needed, no similar question is pending, and the Council will certainly not embark on another investigation until the policy question has been dealt with.

I favor plenty of opportunity for debate of this subject in publications in *Operations Research* and *OR/SA Today* and at meetings of the Society. I don't believe, however, there is much to be gained by extensive rehashing of the 1969 debate and Appendix III. No doubt there will be some papers offered for publication to *Operations Research* on the subject of the ABM system, and I think we can all agree that they can be handled as normal technical papers.

Letters to the editors of the journal and the newsletter on the subject of critiques will, I believe, be dealt with generously by the editors in an effort to give readers balanced comments pro and con various aspects of the subject.

Since we are a democratic Society, these and other procedures are open to the membership, including petitions to the Council as provided for in our Constitution. I hope that you and the others who wish to influence future policy will take full advantage of them, will bring your views before the membership, and will participate in the preparation of a resolution, petition, or Constitutional amendment.

T. E. Compton, *Vice President, ORSA Student Chapter, Kansas State University, Manhattan, Kansas.*

The September 1971 issue of *Operations Research* was engrossing. Your points about the quality of work of some operations researchers are well taken; more so-called analytical discussions should be so well scrutinized.

Thank you very much for an outstanding issue.

Leslie C. Edie (this letter follows the one from Douglas L. Brooks).

Richard L. Garwin, *Yorktown Heights, New York.*

I. Introduction

As one who has not figured publicly in the Safeguard controversy, I hope I may be allowed to comment on the form and substance of your special issue of *Operations Research*, September 1971, "Guidelines for the Practice of Operations Research."

For more than a decade, I have had continuing official contact with the Department of Defense and other interested parts of the government in regard to both offensive and defensive strategic forces. I know and have worked with almost all of the individuals named in the "Guidelines . . ."

study, and I was a member of both the Defense Science Board and the President's Science Advisory Committee during 1969. In making these comments I speak only for myself.

In general, I favor far more analysis of issues, both technical and nontechnical, than is now available, and I have urged this publicly. However, I believe that the ORSA Council was unwise in committing the Society to this review, and especially to the publication of the appendixes of the "Guidelines . . ." issue.

In commenting on the "Guidelines . . .", I don't want the details of this comment to obscure the fact that "Guidelines . . .", as well as most of the proponents of Safeguard, dealt primarily with the possible vulnerability of Minuteman and hardly at all with the question before the Congress, namely the utility, necessity, and value of Safeguard in preserving Minuteman. I discuss this later in Section VI.

II. The ORSA "Guidelines . . ." Is a Quasi-Judicial Action without Adequate Safeguards

Obviously, I would have no objection to the publication by an individual or a group of individuals of a scholarly or popular article analyzing the actions of the individual proponents or opponents of the ABM. I think, however, that it was predictable that the publication of Appendix III, although opposed by five members of the Council and the content not endorsed by the Council, would be given in the popular press the authority of the Society. In fact, I think the operation, with its possible impact on the careers of the individuals involved, bears a strong resemblance to a court proceeding, but without prior limitation of jurisdiction, promulgation of rules, or provision of safeguards, all of which are thought to be necessary in a continuing and formal court and which would be even more necessary to guard against abuses of an *ad hoc* proceeding. For instance, if the ORSA is to have done more than to promulgate a "bill of attainder" from an informal court, I believe that it should institutionalize this procedure and allow it to be convened by nonmembers as well as members. Surely, ORSA does not intend to allow only members of the Society to call for investigations into analyses and presentations of nonmembers. The particular investigation of the ABM testimony seems inadequate, of itself, for the promulgation of general "Guidelines . . .", and for this purpose I believe that it would be useful for the ORSA to provide a dispassionate and detailed discussion of the adequacy of the systems analysis and operations research, and of the honesty of presentation, in the case of the supersonic transport testimony, as well as for the generally uncontested budgetary submissions to Congress by various agencies of the government including the Department of Defense.

On the general question of a court proceeding without rules, jurisdictional limitations, or safeguards, one might argue after the fact that the investigation and conclusions were fair and that justice was indeed done. This argument I believe is unacceptable, since there is always a probability, no matter how good the court, that an error will be made in its investigations or analysis. If an individual were able to convene a court repeatedly (and the argument holds with lesser strength even for a single case) against certain

other individuals, there would be substantial expectation of damage to those individuals, no matter how good their behavior. I believe that safeguards important enough to have been stressed in the Constitution of our nation should be regarded more highly by the ORSA.

III. Does Some Knowledge of Science and Engineering Necessarily Make One Unfit to Judge the Merits of Defense Programs?

I am a strong supporter of operations research and systems analysis and have had substantial experience with the conduct and results of such studies. I think in the present case, both the review and the position of the plaintiff suffer from over-professionalism.

Choosing the best direction for our strategic forces involves a whole chain, or even a tree or graph, of processes, from alternative conceptions through analyses, program planning, etc. To make any single link stronger and more highly polished may contribute little to the overall strength of the chain. In medicine, the operation can be a brilliant success, but the patient may die if the diagnosis was wrong. There are many examples of disservice to the nation (and I believe to the profession) resulting from sophisticated OR techniques applied to shaky assumptions, with further simplifying and constraining assumptions made to enable utilization of existing OR methods.

For instance, I have great regard for Alain Enthoven, with whom (and with whose systems analysis staff) I had frequent contact during his service in the Pentagon, but his comments in connection with "Guidelines . . ." and quoted in the *New York Times* of October 1, 1971, regarding his "... great difficulty in getting strictly scientific advice from the scientists and strictly military advice from the military . . ." reflect a serious misunderstanding of the world and an attempt to pigeonhole an individual according to a label that may describe only a small fraction of his competence and experience. That one has once received a Ph.D. in physics or chemistry provides no basis for ignoring the competence and judgment that may have been acquired through decades of management and decision-making in technical/military fields.

Furthermore, in my experience a prime deficiency of defense-oriented OR/SA arises from excessive depth of analysis of a single or few alternatives, with complete neglect of other possibilities, which may have been less attractive to the military services and which systems analysts may not have invented for themselves. For instance, it is clear that at the time of the C-5 analysis from the Office of the Assistant Secretary of Defense, Systems Analysis, alternatives such as the fast-deployment logistic ship (FDL) should have been considered from the first. Similarly, for years analyses were made regarding the effectiveness of different mixes of ASW forces, to the almost complete neglect of mines. Other examples are detailed analyses as to the relative merits of different kinds of radar for blind bombing, to the total neglect of bombing by accurate navigation. In my experience, these major alternatives to proposed programs, as well as revelations of previously unremarked deficiencies of the proposed programs, have often come from physical scientists.

I think that individuals in the Congress want to hear judgments from those

they respect, and that it does a disservice for a scientist with long experience in military technology and strategy not to give the Congress the benefit of his best judgment. If I were a Congressman, I would far rather trust an honest personal analysis than the product of a bureaucracy, since we are all too familiar with the suppression of information that has been practiced by almost every administration, and with testimony by official spokesmen that neither represents the point of view of the individual nor is in accordance with the facts at his disposal. In concentrating on the analysis of the DOD Safeguard proposals, the ORSA *Ad Hoc* Committee begs the question of the validity of the assumptions involved, both technical and strategic. Perhaps because of the mode of initiation of the investigation, the ORSA report ignores certain readily verified misstatements of fact; e.g., information presented by DDR&E to the Senate Committee on Foreign Relations to the effect that he had discussed Safeguard with the President's Science Advisory Committee before the decision, when in fact he had only brought it to PSAC on March 17, 1969, after the announcement by President Nixon on March 14, 1969.

IV. Is the "Guidelines . . ." Verdict on Rathjens, et al Justified?

The body of "Guidelines . . .", though innocuous, is somewhat peripheral to the investigation of the *ad hoc* committee, being oriented toward the client-analyst relation and not addressing some of the important questions that it raises (page 1134—"An analyst who wishes to disagree publicly with a client is placed in a difficult ethical position . . ."). Appendix III is the troublesome part of the volume. The differences between the results of Wohlstetter and those of Rathjens, Weinberg, Wiesner, and Panofsky are largely due to three points of disagreement: (1) the military utility of reprogramming ICBMs to replace failures before launch or during flight, (2) the feasibility of simultaneous missile attack on US ICBMs and bombers, and (3) the robustness of the Safeguard system; i.e., the range of enemy force levels over which it would (a) be necessary and (b) contribute substantially to the goal of preserving a specified number of Minutemen.

1. With regard to reprogramming a missile force to compensate for those missiles unready to launch, for failure during launch, and for failures after launch, there is no doubt that it is feasible in principle to do this. As Dr. John S. Foster, DDR&E, has stated, such a proposal was made for Minuteman, under the program name TAPS, but was rejected. In fact, the technical feasibility of reprogramming depends on the details of the MIRV system, and on the availability of a reliable and adequate communications and computing system for assigning reserve missiles to replace those that have failed. For instance, it is perfectly possible that a particular MIRV design requires mechanical readjustments for retargeting, and that, unlike our current Minuteman missiles, the SS-9 might have the capability to attack only a single set of three targets without hours-long readjustments and verification.

Even should an SS-9 be able to accept designation of a new target or new set of targets via electrical signals from the outside, its guidance mechanism might be such that, not only the target coordinates (and, of course, its own

fixed silo coordinates), but also the desired flight profile, might have to be computed and transmitted to the missile. This flight profile would be dependent upon the particular target complex and on the reserved silo location. Either one might have to compute some hundreds of trajectories within a few seconds or to choose among very many precomputed and prefired trajectories. Some of these questions as to the nature of the MIRV implementation are amenable to observation and analysis. Consideration of these technical difficulties, I suppose, led Panofsky, Rathjens, Weinberg, and Wiesner to judge that the USSR would not rely on reprogramming for a first-strike capability against Minuteman. My own judgment is that if I were the USSR and were attempting to achieve a first-strike capability against Minuteman, I would require a force that would put two warheads on each Minuteman silo, in order that an adequate kill probability might be obtained without the complex communication, computation, and control problem involved in reprogramming. This technical-military judgment deserves more than misstatement and dismissal. The *ad hoc* committee (page 1200) "... finds no basis for Foster's statement that the book [Chayes-Wiesner] suggests that half of the silos should be attacked twice rather than all of them once"

2. The *ad hoc* study (pages 1215-1217) takes Wiesner and particularly Panofsky to task for noting that an SLBM attack on bombers gives Minuteman at least 30 minutes firm warning of ICBM attack, and an ICBM attack on Minuteman (since we shall know of the launch of ICBMs) gives additional warning time to allow the bombers to be flushed. This simultaneous launch of ICBMs and SLBMs minimizes the time available for US decision. Thus, the bombers would be flushed when our sensors detected launch of a vast number of Soviet ICBMs. Our capabilities for detecting Soviet ICBM launch in the Safeguard time frame will be extremely reliable, and, although the bombers can return to base if the missiles do not impact in the United States half an hour later, this would be a most unlikely eventuality. So long as the bombers have reasonable assured-destruction capability, we need not energize a capability to launch Minuteman on firm warning of ICBM launch. Should SLBMs be launched against the bombers, this launch will also be detected, and some bombers flushed, but the detonation of nuclear warheads near the air bases will then give confirmation and hard information upon which to base the launch of Minuteman. So goes Panofsky's argument.

The *ad hoc* committee quotes DDR&E's answer in 1970 that the SLBM can be used not only to attack our bombers but also simultaneously to attack the Minuteman force *and to prevent the launch of Minuteman*. Both blast effects on the Minuteman in flight through the atmosphere and X-ray effects on the upper stages outside the atmosphere should be considered in setting a maximum separation in time and space for such an attack to be an effective means of pinning down the Minuteman force. It needs only a single Y-class submarine outside of firing position or unable to fire to cause such an attack to fail, if it were otherwise feasible. The command-and-control problems in coordinating such a simultaneous attack by submarines are very difficult, as is reprogramming from boat to boat for failures during launch. I think the "Guidelines . . ." has it backwards in stating (page 1216): "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." Not one reasonable tactic was proposed during the discussion, and to analyze all unreasonable tactics obviously requires resources even beyond those of the Department of Defense.

3. The question of robustness of the proposed Safeguard system is an important one. Below a certain numerical offensive force level, Safeguard is obviously not needed to ensure the survival of a minimum number of Minutemen. There is some number of SS-9's in the Soviet force that clearly overwhelm Safeguard so that it can no longer ensure that the minimum number of Minutemen survive. If the difference between these two offensive force levels should be small, say 10 percent, it would be very difficult for the Soviet Union, even if it wanted to, to build a force which would fall precisely into the range where Safeguard would be of some value to the US. Indeed, that is the case, not only because the Soviet Union can use the SS-9 multiple warheads, if it so wishes, to destroy the two Safeguards radars which defend any Minuteman at all, but the Soviet Union can use also some of the many SS-11's or could build some SS-13's, or could allocate some of the vast number of SLBMs (which are supposed to be useful during a pin-down attack) actually to destroy the two MSR radars without which Safeguard is useless in defending Minuteman. For instance, "Guidelines . . ." on page 1235 states:

However, the opponents failed to treat the issue adequately and fairly on two counts:

(a) They failed to discuss adequately efficient means by which the United States could expand Safeguard to restore the desired level of effectiveness.

(b) They failed to discuss adequately the difficulties and costs the Soviets would encounter in implementing some of their proposed means of 'cheaply' overcoming the defense.

One of their proposals was to use the SS-11s for attacks against the radars in order to exhaust the interceptor stockpiles

The text goes on to say that the SS-11s are not a "free good," etc., etc. The criticism misses the point. It is up to the *proponents* of Safeguard as well to face these issues squarely. Furthermore, in the same way that Secretary Packard stated that the SS-9 has the capability of transporting three 5-megaton independently-targeted warheads, the SS-11 has the capability of carrying several radar-busting warheads; and a very few SS-9s, if they make use of the technology that we already have in Minuteman III or Poseidon, could be fitted with very many warheads and could do this job as well. In considering the cost of destroying Safeguard, the *ad hoc* committee should use as a reference the cost of the SS-9 force that is supposed by DDR&E to have been built especially for first-strike attack on Minuteman.

It is precisely the question whether Safeguard *as proposed* is effective against the threat that is important to the Congress. We are not talking about *development* of a technology of uncertain cost. The question was one of authorizing deployment and paying the bill. If a mountain climber is sold a 200 foot rope for making a 300 foot descent, he has every right to feel

cheated, although the technology is such that paying more would have bought a system adequate to the task. Indeed, Panofsky and others claimed that for the Minuteman defense role an expansion of Safeguard would not acceptably meet the threat but that a less expensive, more tailored “hard-point” defense would be possible. I think that it must be admitted that the Department of Defense has now come to the same view, and I believe that they would have reached this conclusion several years earlier had they not been committed to selling the deployment of Sentinel-Safeguard technology.

V. The Ad Hoc Study Is Guilty of the Same Faults of Which It Accuses Others

While chastising Wiesner, *et al*, for unsubstantiated statements, “Guidelines . . .” speaks *ex cathedra* as follows (pages 1222 ff):

(5) Suggestions of Impractical Alternatives

Several examples are found in the record of the debate, including the following:

(a) *The proposal to launch Minuteman on warning.* The possibility of preventing the destruction of substantial parts of the Minuteman force by merely launching it on the basis of warning of an impending attack, has been proposed by, among others, Wiesner and Weinberg in the Chayes-Wiesner book.

(b) *Armed air alert . . .*

(c) *The proposal to disperse B-52s widely to airports including those commercial airports near large cities . . .* would appear to create conditions conducive to nuclear accident that are more serious than those of the proposal to deploy ABM sites near cities, which created public opposition to the Sentinel System.

(d) *Launching Minuteman . . .*

I can find no further discussion in the study on (a), the proposal to launch Minuteman on warning. I see no reason to regard this as ‘impractical.’ The hardware capability is easy to obtain, in comparison with the capability to reprogram missiles to account for failed missiles. Such a capability need be energized only if numbers and dispositions of Soviet submarines, numbers and accuracies of MIRVs, etc., appear to give the USSR the possibility of having a first-strike force. Moving at that time to a launch-on-warning posture (by which I mean a launch on *verified* warning, perhaps including the observation of actual nuclear weapons explosions in the Minuteman fields or at air bases) could be an effective deterrent to a first-strike on Minuteman fields. Why does the *ad hoc* committee regard this as impractical, so impractical that they give no analysis or reason for their views? In any case, in order to contemplate a strike against Minuteman, the USSR would have to be confident that we would *not* launch Minuteman on warning. I don’t see how the USSR could ever obtain such confidence.

As for (c), surely the committee knows that it is quite feasible to produce nuclear weapons that can give no nuclear yield at all until a special code is set into the weapon during the flight of the aircraft. Such safety locks are widely deployed on US nuclear weapons abroad. Furthermore, weapons containing only uranium 235 and thermonuclear fuel need not constitute a radiation hazard following an aircraft crash or other accident. If this is the

strongest criticism the *ad hoc* committee could make against this proposal of dispersal, the proposal should be reexamined.

VI. "Guidelines . . ." Notes That the Administration Appears to Have Avoided the Basic Issues in the Analysis of Defenses

The report, pages 1236 to 1237 at the end of Appendix III, finally gets down to the fundamental issues, noting that:

Except for Foster's statements in his May 12 speech, the Administration appears to have avoided addressing the basic issue in the analysis of defenses—how easily can they be countered and what must we do if the opponent attempts to do so? . . .

If this principle is accepted, an argument that Minuteman must be defended (by Safeguard or any other defensive system) against a heavy Soviet SS-9 attack cannot be conclusive, unless, as a minimum, it shows that:

- (i) The undefended Minuteman survivors as well as all other surviving U.S. strike forces will not suffice to present a believable and adequate second-strike force, and,
- (ii) When Minuteman is defended, the total surviving U.S. strike force is sufficient to inflict unacceptable damage on the Soviets.

Even such an argument does not suffice to demonstrate conclusively that Minuteman should be defended. It merely provides (if accomplished) an existence proof of one possible procedure to employ for preserving the credibility of the US deterrent posture. Whether there exists some other procedure that may be superior has not been discussed definitely in the same context as the pro and con Safeguard arguments . . .

The failure of this public exchange of views to deal with this question, whether in conformity with the wishes of the Executive or Legislative branches of the government, is a basic failure to deal with a most fundamental issue.

These fundamental questions were touched on repeatedly by Wiesner, Weinberg, Rathjens, and Panofsky. I believe it is demeaning to the operations-research profession for its Society to support the publication of Appendix III, 63 pages of detailed criticism of the actions of individuals ". . . all public-spirited men who gave generously of their time in performing extensive analyses and presenting them without remuneration to Congress. We owe them all a debt of gratitude that must not be obscured by any flaws to which this report calls attention . . ." (page 1124), while devoting less than two pages (pages 1236 to 1237) to a brief statement pointing out that the Administration avoided addressing the basic issue.

VII. The Final Prescription of "Guidelines . . ." for the Conduct of Such a Debate Is Defective

Finally, Appendix III ends with the following topic, which I have read and reread (page 1237):

C. Logical Deficiencies of the Debate

It is disturbing that the following simple logical approach was not stated or followed by both sides of this debate.

The proponents of the ABM should show that there exists a reasonable set of future circumstances for which the ABM is desirable. The opponents must show that for *all*

reasonable sets of future conditions the ABM is undesirable. Desirability in both cases should be represented in terms of cost-effectiveness or cost-benefits. The postulation of a particular set of future conditions for which the ABM is undesirable cannot show that the program should not be pursued.

Try as I have, I cannot make sense out of this conclusion to the study. Certainly it is the number and fraction of the circumstances which count. Although insurance against damage to one's automobile certainly would be desirable for some sets of future circumstances, it is the cost of that insurance, the probability of being repaid, and the probability of those future circumstances that should determine the decision. In the ABM problem, one is not building a defense against random future circumstances. The question is whether the opponent can react at what he judges a reasonable cost to predictably negate the effectiveness of the ABM. The last sentence of this conclusion is a statement of principle, it has nothing to do with the cost of the USSR reaction. I read it as stating that even if the opponents had shown how the USSR could readily react to negate the ABM (rendering the ABM undesirable in that particular set of future conditions), this would not show that the program should not be pursued!

Note that Appendix III (although "not refereed" in the usual sense) according to Appendix IV, pages 1257 and 1258, has been through extensive review. Furthermore, in the Foreword (page 1124) the President of the ORSA, speaking of the reservations of five members of the Council (pages 1255-56) that the report should have been refereed, notes that this reservation is "specious." Perhaps the members of the Society will see more sense in this concluding proclamation than I do, and perhaps in any case it might not have been caught by the referee, but one hopes that it might have.

VIII. Conclusion

I support the views of the five Council members (pages 1255-56) who feel strongly that "the *Society* itself should not take on the quasi-judicial function of investigating and reporting on professional behavior of individuals." I think that the appendices to "Guidelines . . ." raise far more questions than they answer and, with the exception of pages 1236 to 1237 (already discussed above) in concentrating on the detailed analysis of one aspect of the decision, compound the failure with which they charge the Administration, namely of avoiding the basic issues. I regret very much these actions of the ORSA Council majority. Now that ORSA has established the precedent of quasi-judicial analysis of the performance of individuals, is it out of the question to ask for a Council-appointed committee to criticize the content of Appendix III? I find it to be full of those faults for which it criticizes others, both in its conduct and its content. I think that it is now up to the Society to decide whether it wants to make such quasi-judicial reviews widely available, or whether it wants specifically to disown this one. I think that the appendices cannot be allowed to stand as a member-initiated, Society-published attack on individuals who are not members of the Society or of the 'profession.'

Amoretta M. Hoeber and David C. McGarvey, *Pacific Palisades, California, and Santa Monica, California* (a reply to Richard L. Garwin).

Dr. Garwin, may not, as he says, have figured publicly in the debate over Safeguard. However, he publicly opposed its direct predecessor, Sentinel, including the option to defend Minuteman. He argued then that, since the option of developing and deploying a launch-on-warning capability "... would be available to us at any time, it would not be necessary to spend scarce money this year on ABM to defend the Minutemen."¹ And in 1969 he circulated a letter opposing Safeguard itself to Senators.

What should matter to the reader, however, is not Dr. Garwin's position on Safeguard, or whether he was a "nonparticipant" in the debate, as the December F.A.S. *Newsletter* suggests,² but the merits of the arguments made. In this spirit we would like to comment on Dr. Garwin's substantive criticisms of the technical analyses in the recent ORSA Report.³

Dr. Garwin restates most of the arguments that have been levied against the technical quality of Appendix III by those criticized in that Appendix.⁴ It is good to have them put forth, as Dr. Garwin does, shorn of abuse. But, as we develop below, they include instances of the very practices that the ORSA Report correctly criticises; and they are unfounded. Moreover, it is worth noting that Dr. Garwin found no place in his long letter to mention some of the more blatant examples of unprofessional analysis dealt with in the ORSA Report. For instance, in "A Critique of Pentagon ABM White Paper," by Drs. Rathjens, Wiesner, and Weinberg, the cost of the *whole* 12-site Safeguard deployment was allocated to the function of defending Minuteman (ORSA Report, p. 1232), and Dr. Wiesner made a widely publicized but completely unwarranted claim that he used the official DOD kill probability but that Dr. Foster of DDR & E did not (ORSA Report, pp. 1193-1199).

To support his claim that the findings of Appendix III are not justified, Dr. Garwin concentrates on three issues: the potential vulnerability of Minuteman; the feasibility of coordinating attacks on U.S. ICBMs and bombers; and the contribution of Safeguard to the protection of Minuteman. In each case his supporting arguments are in error.

1. The Potential Vulnerability of Minuteman

Dr. Garwin accepts the claim that reprogramming was a key—if not *the* key—difference between proponents and opponents of Safeguard, as reprinted in *Nature* and by the F.A.S.:

The chief point of disagreement between the opponents and proponents of Safeguard in 1969 was whether or not the SS-9s could be reprogrammed . . .⁵

and,

[Reprogramming] became the crucial assumption on the vulnerability of Minuteman for the mid-seventies.⁶

This claim was first made by Dr. Rathjens in June of 1969:

The major difference between Mr. Wohlstetter's analysis and mine is with respect to the extent to which the Russians could retarget some of their missiles to take account of failures of others.⁷

No matter how often it is said, this claim is simply not true. Appendix III carefully pointed out that Professor Wohlstetter, early in the debate, stated results using intelligence estimates of the number of one-megaton warheads (the yield used by Dr. Rathjens in his calculations) that could be carried per SS-9 and *no* reprogramming. These results were essentially the same as those using five-megaton warheads and partial reprogramming. These calculations were never dealt with by the opponents, as is pointed out in the ORSA Report, nor have they been dealt with now.

The method of merely reiterating claims as if they had not been responsibly answered was one of the tactics of confusion that thoroughly justified forming the *Ad Hoc* Committee.

Dr. Garwin and other critics of Safeguard make another serious error in treating possible enemy forces. They use whatever Russian force helps their argument of the moment. When they argue that there is no reason for the U.S. to be concerned with the threat to Minuteman in the late 1970s and beyond, they make much of limitations perceived in *existing* Soviet systems, in particular the three-warhead SS-9 configuration. Yet, when arguing that Safeguard is inadequate, these same critics are willing to assume Soviets who, far from being limited to existing hardware, and free to use whatever seems technologically feasible.⁸

Thus, after arguing that some "particular MIRV design"—presumably that observed in Soviet SS-9 tests—may not be amenable to reprogramming for attacking Minuteman silos, Dr. Garwin proceeds to argue that Safeguard could be easily overcome since "... a very few SS-9s, if they make use of the technology that we already have in Minuteman III or Poseidon, could be fitted with very many warheads ..."

2. Coordinated Attacks on ICBMs and Bombers

Opponents of Safeguard claimed in 1969 that the problem of coordinating missile attacks on U.S. ICBMs and bombers meant that "... the combined strategic bomber and land-based missile force *assure* survival of one or the other from attack."⁹

In justification of this strong claim, these opponents discussed only two modes of attack: (1) a near-simultaneous launch of SLBMs against bombers only and of ICBMs against Minuteman missiles, and (2) an attack with the same targeting but timed for near-simultaneous impact rather than near-simultaneous launch. They did not debate the feasibility of a third—and extensively studied—mode of attack, but ignored it completely: a near-simultaneous launch of SLBMs against bombers and ICBMs against Minuteman, coupled with a launch of SLBMs targeted so as to hamper or deter the launch of Minuteman missiles before the ICBMs could arrive.

Dr. Garwin attempts to defend the position of the opponents by arguing that this third mode of attack would be infeasible. But his statement "It needs only a single Y-class submarine outside of firing position or unable to

fire to cause such an attack to fail, if it were otherwise feasible” greatly overstates the consequences of the failure of a single submarine.

Even if the U.S. knew that a particular submarine were unusable for the attack, it would be impossible to determine when and where any resulting gaps in the attack might appear. Hence the problems faced in launching the Minuteman force through such an attack would be only slightly reduced by the failure of one or even a few of the submarines.

Furthermore, it is not necessary to reprogram from boat to boat for failures during launch, as Dr. Garwin suggests. About the same effectiveness would be achieved with reprogramming within just the pool of sixteen missiles on a boat.

Strike coordination problems for such an attack are stringent, but we agree with the DOD position that “. . . the timing of such an attack, although critical, is by no means infeasible.”¹⁰ The problems certainly appear to be no more difficult than those solved by Japan in coordinating strikes against 29 targets separated by up to thousands of miles using over 2000 aircraft and over 200 surface and subsurface ships on December 7, 1941.

The severity of the problems of launching Minuteman missiles through such an attack depends on many parameters, including the size of the SLBM force available and its performance characteristics. Our own calculations of the effectiveness of such attacks show that they warrant serious concern. Indeed, we believe the Committee was overly mild in its criticism of Professor Panofsky on this issue in light of his claim that such an attack would require “an enormous increase” in the Soviet SLBM force.

3. The Contribution of Safeguard to the Defense of Minuteman

It is essential to Dr. Garwin’s position on the robustness of Safeguard that he adopt the rigid view, “It is precisely the question whether Safeguard *as proposed* is effective against the threat which is important to the Congress.” Dr. Garwin’s mountain climber analogy is defective. Unlike the height of Dr. Garwin’s cliff, the threat is not fixed; the U.S. response should not be either.

Dr. Garwin follows exactly a line of argument pointed out as faulty in Appendix III when he evaluates the effectiveness of a static defense against a nonstatic offense. This approach is as defective as that of the Administration when it looked only at effectiveness against a “design threat.” Both approaches and more acceptable alternatives are treated in detail in Appendix III (ORSA Report, pp. 1228-1230 and 1234-1236).

Aside from treating the above three issues, Dr. Garwin charges the *Ad-Hoc* study with being “guilty of the same faults of which it accuses others.” He bases this on the brevity with which the report dismisses as impractical two alternatives that he believes are practical: the use of a launch-on-warning policy to ensure Minuteman survivability, and the wide dispersal of B-52s to civilian airfields. Dr. Garwin’s comments indicate that the deficiencies of the alternatives he suggests may not be as obvious as we, and apparently the *Ad Hoc* Committee, thought.

The ORSA-Report criticism of the launch-on-warning proposal as “impractical” is too gentle; ‘reckless’ would be more accurate. Senator Edward Brooke of Massachusetts, an ABM opponent, has gone further and

labeled such a policy “obvious lunacy.”¹¹ To be sure, it is technically feasible to launch on warning, and the Soviet Union could never be certain that the U.S. might not do this. However, it is one thing for there to be such a residual doubt in the Soviet mind and quite another for the U.S. to be forced to depend upon this tactic as the principal means of assuring survivability of the Minuteman force.

It almost goes without saying that such a policy would increase the danger of a nuclear accident—not just the accidental detonation of a defensive missile, which has been emphasized by some ABM opponents—but the accidental initiation of a large-scale nuclear attack against Russian cities. Dr. Garwin apparently recognizes this danger in his suggestion that launch be only on “verified warning, perhaps including the observation of actual nuclear weapons explosions in the Minuteman fields or at air bases.” But this ‘solution’ to the false alarm problem would put the United States squarely back in the position of being vulnerable to an attack designed to hinder the launch of the Minuteman missiles until they are destroyed. It would not be launch-on-warning but launch-too-late.

There are other less obvious but also serious defects of a launch-on-warning system:

- The system would have to be set to respond to even a light attack, possibly including an attack by a third country, so that it could not be circumvented by an attack designed to destroy the sensors or other links necessary for the system to function or by an attack using small initial strikes designed to prevent launch until the full attack arrived.

- Although the launch decision might nominally rest with the President, it would in effect be made by military officers, technicians, or computers, since the limited information and short warning time available would, at best, permit the President time only to accept or reject the system’s evaluation without questioning.

- The U.S. would be committed to but one response option, presumably an all-out attack on Russian cities, no matter what the nature or country of origin of the attack that triggered the system.

- Such a ‘hair-trigger’ policy is likely to increase tensions, particularly in times of crisis.

In sum, such a strategy of desperation would be dangerous and destabilizing.

Dr. Garwin’s comments on the proposal to disperse B-52s widely to airports, including commercial airports near large cities, are that safety devices exist to prevent accidental nuclear explosions and that it is possible to construct weapons which need not constitute a radiation hazard following an accident. But it is not necessary for an accident to result in a nuclear explosion or release of radiation for it to have serious repercussions. In fact, it is not even necessary to have an accident. Consider, for instance, the possible reactions if, as a result of a false alarm, SAC flushed the bomber force from airfields across the country such as Kennedy, Logan, and O’Hare on a busy Friday afternoon.

There are other points in Dr. Garwin’s treatment of technical issues that we believe to be defective, such as additional flaws in his treatment of reprogramming. We have dealt with only the more important ones.

Contrary to Dr. Garwin's assertion, the problem with expert testimony in the debates over ABM has not been over-professionalism. Instead, careless calculations and grossly unprofessional analyses have been presented with a cavalier disregard for the elementary standards of reasonable discourse that are so important to science. Until the recent ORSA Report, such actions were conducted with near impunity.

We believe that the Report is sound in all major respects and that the test of time will show it to have withstood the very severe attacks that have, not unexpectedly, been directed at it.

That is not to say that the Report is without defects. The last several paragraphs (ORSA Report, pp. 1236-1237) are not up to the quality of the rest of it. They seem to waffle in the familiar way of committee writings.

But these are minor lapses. In the large and in practically every detail, the Report is outstanding. Considering the prominence of the persons dealt with and the abusive nature of much of the debate, it took fortitude rarely found in a committee to publish such a forthright and objective document. It is a credit to the wisdom of the ORSA Council and to the diligence, fairmindedness, and competence of the Committee.

Notes

1. Letter to the Editor, dated October 13, 1967, *New York Times*, October 22, 1967.
2. Federation of American Scientists, *F.A.S. Newsletter*, "Special Issue on ORSA ABM Report," Vol. 24, No. 9, December 1971.
3. Appendix III of the special issue of *Operations Research*, "Guidelines for the Practice of Operations Research," September 1971.
4. Including those in "Comments on the *Ad Hoc* ORSA Committee Report on Professional Standards," George W. Rathjens, Steven Weinberg, Jerome B. Wiesner, September 29, 1971; the *F.A.S. Newsletter*, *op. cit.*; and *Nature*, "Fighting the Great ABM Battle Again," October 15, 1971.
5. *Nature*, *ibid.*, p. 443.
6. *F.A.S. Newsletter*, *op. cit.*, p. 2.
7. Letter to the Editor, dated June 5, 1969, *New York Times*, June 15, 1969.
8. See Rathjens, Weinberg, and Wiesner, *op. cit.*, and the sophisticated Chinese technology assumed in Dr. Garwin's article with Hans Bethe, "Anti-Ballistic-Missile Systems," *Scientific American*, March 1968, pp. 21-31.
9. Statement of Dr. W. K. H. Panofsky, April 22, 1969, Hearings before the Senate Armed Services Committee, Authorization for Military Procurement, Research and Development, Fiscal Year 1970, and Reserve Strength, Part II, p. 1128 (emphasis added).
10. Hearings before the Senate Armed Services Committee, Authorization for Military Procurement, Research and Development, Fiscal Year 1971, and Reserve Strength, Part III, p. 2333.
11. Speech, April 1969.

Saul I. Gass, *Mathematica, Inc.*, Bethesda, Maryland.

I am writing with respect to the September, 1971 issue of *Operations Research*, Vol. 19, No. 5. I would like to preface my comments by noting that I have been a member of ORSA since 1954, am presently a member of six professional societies, and have concurrently 'over a century' of active

and passive participation in the affairs of these societies. The following are my comments with respect to the cited publication and the fact that the ORSA Council established an *ad hoc* Committee on Professional Standards and published the Committee's report in the official Society technical journal.

1. Few if any professional societies are in a position to investigate the ethics and professional activities of any of its members or other persons involved in the society's area of interest. Those that are capable of doing this have, unlike ORSA, established basic rules and governing regulations approved and acknowledged by their members. In addition, these societies, unlike ORSA, have developed a formal, recognized procedure for conducting such activities.

2. I believe that the ORSA Council overstepped its jurisdictional and authorized responsibilities by the establishment of the *ad hoc* Committee. The proceedings under investigation by the Committee and the conduct of the protagonists had no relationship to the professional society called ORSA. It certainly had relationship to the individual members of ORSA as citizens, but not collectively as a Society.

3. The "Guidelines" which were developed by the Committee and printed in the cited issue have no official ORSA status. One can hardly argue with the golden thoughts of the "Guidelines," but it is rather unusual to write the rules for a game that has already been played, especially when the players were not participating.

4. Where does the ORSA Council now draw the line on what to study next by some future *ad hoc* Committee? If we are to be an investigatory organization, please do inform the membership.

5. The publication of the report without the normal refereeing and submission procedures is lamentable and deplorable. It is incomprehensible that the scientific backgrounds of the Council members allowed them to purposely circumvent accepted ORSA professional publication procedures and sully the objectivity of our technical journal. And to take over a complete issue for that purpose further compounds the error.

6. If the Council felt that the *ad hoc* Committee's report should receive wide distribution, it could have authorized its publication in a different form. Better still, it would have been cost-effective if Senator Jackson had it published as a Senate document by the Government Printing Office and thus saved ORSA some money and a build-up of its queue of papers to be published.

7. Refereeing of the paper would have at least caused a proper title to be used for publication like "The ORSA *Ad Hoc* Committee on Professional Standards Report on the Analyses Presented to Congress During the 1969 Safeguard Debate," not an innocuous title like "Guidelines for the Practice of Operations Research." I wonder why that title?

8. I believe that the ORSA Council needs to explain to its members on what basis future studies like the one under discussion will be undertaken; and what are the present rules for publication of an article in *Operations Research*.

9. I agree fully with the minority statement published in the cited edition.

Hugh J. Miser (I), *Editor, Operations Research, Windsor, Connecticut* (a reply to Saul I. Gass).

I should like to call your attention to the following facts, of which you may not have been aware when you wrote your letter:

1. The early days of the *Society* saw much discussion of professional standards; see, for example, R. F. Rinehart's retiring presidential address [*Opns. Res.* 2, 229-233 (1954)]. Too, in May 1954 the Council established a Committee on Professional Standards [*Opns. Res.* 2, 346 (1954)] that continued until the ORSA Constitution adopted in 1961 provided in Article XII for an Ethics and Grievance Committee, a specification that persists until today. Since the business of this Committee was, at least until 1969, mainly to be ready in case of need, the Council of the Society adopted an arrangement whereby the President would serve as the Committee's Chairman, and would coopt others to assist him as the needs arose. This is the arrangement that was used in the present case, as explained in the "Guidelines . . ." report's Foreword (page 1123). This machinery has existed to carry out the duties that might devolve upon the Society pursuant to its charter, filed in the District of Columbia on August 20, 1952, with respect to the purpose, stated alike in the charter and the Constitution until today as "the establishment and maintenance of professional standards of competence for work known as operations research" [for the charter, see *Opns. Res.* 1 (Supplement), 6-7 (1952-53); the currently adopted Constitution appears annually in the Society's Directory (pages 123-128 in the 1971-1972 edition)].

2. President Machol's "Foreword" to the report (which the Council, on May 6, 1971, requested him to write) says: "At its meeting of May 5-6, 1971, the ORSA Council accepted the committee's report, released it for publication, and approved *Operations Research* as a medium of publication. Implicit in this resolution (Appendix IV, Part E) was the adoption by the Council (and thus by the *Society*) of the body of this report as its position. It was neither possible nor appropriate for the Council to endorse the appendixes in the same sense."

3. The editorial review procedures for this issue consisted of four parts: (1) review by all the Committee members, (2) review by the ORSA officers and Council members (not all participated, of course, but several reviewed the document, and at least one gave it a very rigorous and detailed scrutiny), (3) an independent review, and (4) a review by those whose names appeared in the report. Normal publication procedures call usually only for step (3), so that considerably more was done in this case than usual. As President Machol put it (page 1258): "It seems clear on its face—and the two men who have been Editors of *Operations Research* over the last decade assure me it is so—that this report has received far more review and editing than is usual for papers published in our Journal." The record is clear that neither the Committee nor anyone else attempted "to purposely circumvent accepted ORSA professional publication procedures."

4. The cost of the special issue in which the "Guidelines" appeared was covered by a special Council appropriation—it did not come from the budget for *Operations Research* (the actual cost of production and distribution was

about \$7000). Thus, the special issue did not change the content, order of papers, or size of the 1971 volume of *Operations Research*, which contained 1685 pages (exclusive of the special issue), 409 more than any previous annual volume.

W. A. Golomski, *John Morrell & Co., Chicago, Illinois.*

I have just finished reading the September '71 issue regarding the ABM controversy. As one who has spent 20 years in nonmilitary industrial operations research, I find the report of little use or value. (I know none of those in the controversy.) The rhetoric and the description of the process seem to suggest a myopia based on a one-sided point of view.

Perhaps we can get more space devoted to the broader problems that confront all of us. The members of ORSA have too much in common, and have too good a track record to be distracted by such diversions of human energy and emotional reserve.

Paul Gottfried, *Silver Spring, Maryland*

I should like to express my full support of the ORSA Council's majority decision to undertake the study documented in the September 1971 issue of *Operations Research*. I also feel that congratulations and thanks are due the *Ad Hoc* Committee for achieving an evenhanded presentation of explicit findings, under obviously difficult conditions.

Further, I should like to rebut the contention by Rathjens/Weinberg/Wiesner (Appendix IV D) and by the Council minority (Appendix IV H) to the effect that ORSA should not have undertaken the "quasi-judicial function" of the study. My argument runs as follows:

- The protagonists in the 'debate' (Rathjens, Wiesner, Wohlstetter, *et al.*), in relying explicitly or implicitly on their professional reputations as qualifications for congressional testimony and for contributions to the public press, incurred an obligation to conduct themselves as professionals.

- The conduct of professionals, to the extent that it has actual or potential impact on the public interest or on the standing of the profession, is subject to review and 'policing' by their peers and/or by governmental agencies.

- If it can be made effective, peer review is to be preferred to governmental review on the ground that the latter is less likely to provide the joint requisites of technical competence and objectivity. When, as in the present instance, no mechanism for governmental review exists, peer review is the only available mechanism for the enforcement of professional standards.

- ORSA is the only professional body competent and qualified in the area of the subject study, MORS being subject to possible charges of lack of objectivity (on the ground of intimate association with the 'military-industrial complex') and TMS being oriented primarily toward other OR areas.

- The fact of nonmembership in ORSA of some of the protagonists is irrelevant to ORSA's jurisdiction. An individual acting, or allowing himself

to appear to act, in a professional capacity places himself under the scrutiny of the profession concerned. As has been argued above, ORSA is both by qualification and by default the only body equipped to represent the profession in this instance.

The minority contention (Appendix IV H) that "...the traditional scientific publication process...is adequate to monitor professional standards" to me appears inapplicable here. The process is valid and effective when the protagonists address each other and their professional colleagues in publications intended primarily for professional audiences. The process is inadequate when publication occurs in a lay environment. The contents of the *New York Times* and of congressional hearing transcripts are not refereed, nor is professional discourse likely to protect the public interest when advocacy is unrestrained elsewhere. In its impact on the public interest and need for restraints beyond those placed on formal professional discourse, the subject of the study to me resembles more the problems of engineering design than those of promulgation of scientific hypotheses.

A somewhat similar argument applies to the alternate suggestion of critique by individuals rather than by appointed committee. No individual can command recognition of his objectivity, nor probably the public lay forum, required to counteract widely publicized contentions. The same difficulty attends critique by any self-appointed group of individuals; the public, being unable to judge a critique on its technical merits, would view such a group as merely another contender in a battle among more-or-less equally qualified experts.

I should like to urge the Council not only to establish professional review activities on a permanent basis, but also to seek broad coverage of the *Ad Hoc* Committee findings in the mass media.

Amoretta M. Hoeber and David C. McGarvey (this letter follows the one from Richard L. Garwin).

Robert D. Kaiser, *McLean, Virginia*.

The September 1971 issue of *Operations Research* was the most stimulating ORSA document I have ever read. The Council deserves praise for its willingness to consider and publicize the issues. I am sure the Council was acutely aware of the inevitable storm of controversy to be provoked by its action and I recognize the degree of professional and personal courage it required to carry out this action. Likewise, the *Ad Hoc* Committee members, once having accepted the onerous and perhaps distasteful responsibilities conferred upon them, deserve the gratitude of all ORSA members for their devotion to the conscientious performance of the tasks appropriate to these responsibilities.

Having found the challenge for "wide public review" to be irresistible, I am submitting a number of questions and a comment. I preface these with the remarks that: (1) I have thoroughly perused and digested both the ORSA report and the Senate hearings¹ that gave rise to this controversy in the

hopes of improving my perspective and the quality of my contribution; (2) I find the report of the *Ad Hoc* Committee to be highly professional, with meticulous adherence to scientific discipline and indisputable findings; and (3) I, too, take no explicit or implicit position on the question of Safeguard deployment and make no judgment on the motives or ethics of any participants in the debate. Hence my questions and comment are directed not to the principal topics considered by the *Ad Hoc* Committee, but to collateral issues, a few of which were considered in the Council's Minority Report.

Question: Did the Council have adequate cause and sufficient authority to undertake this investigation?

The primary causative agent must be Dr. Wohlstetter's... letter of November 8, 1969, to Dr. Caywood (Appendix IV, A), which requested ORSA "to consider some aspects of professional conduct." Since the scientific controversy over the ABM dispute had received substantial publicity and tended to cast a shadow upon the reputation of operations research as a reliable discipline, the unsettled *professional* issues constituted a second but equal cause. Taken together, these constitute an adequate cause since ignoring the issues could have potentially adverse effects, either direct or indirect, on a substantial portion of ORSA's membership.

Articles II and XII of the ORSA Constitution set forth, respectively, "the establishment and maintenance of professional standards of competence for work known as operations research" as a purpose of the Society, and the designation of the Ethics and Grievance Committee as a standing Society committee. These two articles provide sufficient authority for the Society to examine the professional issues involved. They give explicit exposition to the implicit authority of any organization to police itself.

Question: Should the Society take on the quasi-judicial function of investigating and reporting on professional behavior of individuals?

This is undoubtedly the 'nut' of the procedural problems faced by the Council who, by majority vote, responded affirmatively. The form of this question is taken from the minority statement (Appendix IV, H). The answer should be determined, first, on the basis of cause and authority conferred upon the Council, and, second (assuming adequate cause and authority), on the basis of the alternative consequences. The first basis I have already concluded to be adequate; the second basis is more difficult to ascertain, but I shall attempt to discuss this with unaccustomed brevity.

If the answer is negative, then anyone who holds himself out to be an expert practitioner of the operations-research profession is free to apply to himself any standards of professional behavior that he sees fit. He is likewise free of any procedural review that might otherwise restrain him from employing methods not enjoying the confidence and approval of a substantial number of his peers. This is not necessarily an evil to be avoided; there have been significant advances in engineering and medicine by individuals who refused to heed the tenets and restraints of their professions. Nonetheless, the consequences of a negative response extend beyond our immediate professional boundaries. It could lead (and indeed has led) to public questioning of the reliability and responsiveness of OR in the real world. If public (and legislative) confidence in OR is weakened substantially,

no amount of personal debate in the *New York Times* or *Washington Post* will restore that confidence.

If the answer is affirmative, then a more regulatory Society can be uneasily envisioned. Who is to say that this function should stop with investigation and reporting? Might we not anticipate the prospect of a body analogous to those passing on the qualifications of attorneys-at-law and doctors of medicine to practice within their jurisdictions (although such a body would obviously have to be restricted by suitable 'grandfather' clauses to protect current members from any undesirable restraints)? This is admittedly far-fetched, but not impossible. Certainly we must consider what limitations must be implanted within the Society to prevent over-enthusiastic performance of this 'quasi-judicial' function.

I am convinced that the answer must be affirmative for these reasons: the consequences of a negative answer are repugnant to me, and carry with them insufficient compensatory benefits to offset the potential for the increase in the number of 'mavericks' whose professional liberties might not only threaten our not-yet-firmly-established 'image,' but also not be *pro bono publico*. Additionally, it is difficult to conceive of the development of more regulatory authority within the Society that does not meet with the approval and consent of a majority of the ORSA members. My conclusion here leaves me with a dilemma that is formulated in the following question.

Question: Does this function (in the previous question) extend to those who are not members of ORSA?

Clearly, ORSA has no claim to any authority or rights over individuals who do not profess to be practitioners of OR, any more than the ABA has authority over real estate agents, or AMA over faith healers. What about those who do hold themselves out to be practitioners of OR, yet refuse or neglect membership in ORSA? We have no legal authority to discipline such individuals for 'malpractice,' yet how is the public (or the legislature) to distinguish between competence and incompetence, between the rational and the irrational, between the sound and the unsound? Indeed, what assurance do we have that ORSA itself is competent to execute this function, even upon its own members?

There is another facet to consider. As in the ABM controversy, any member may petition the Council to consider specific allegations against nonmembers who may be charged with violations of decorum. In such case the Council may (as was done here) invite those so charged to participate in the investigatory process, and those who do participate should be made aware of their burden to accept the findings of the responsible commission. In the instant case the invitation was rejected. Can ORSA now claim that the nonparticipating protagonists are in any way bound by these findings? I think not. And can ORSA now make any more than a routine statement of findings to its membership (in this sense, 'routine' would encompass such procedures as a normal report of Council and Society committee proceedings, perhaps to be included in *regular* issues of *Operations Research*, not special issues such as the September issue)? Obviously ORSA can because it has! But *should* it have done so? This last question is now probably moot, but I commend it to the Council for its consideration.

There remains yet one more facet. What about the rights, privileges and prestige of the ORSA members who might suffer if the Council failed to act? Is the Council's first duty to its membership, or to the scientific community as a whole, or even to the public at large? Are we as ORSA members compelled to sit in injured silence at any and every instance of public misuse of the OR discipline? Can we not do as a body that which we may do as individuals, as long as the body applies the same strict standards of behavior to itself as it urges upon its members? If so, does this right not extend to those actions that were in fact taken by the Council in this controversy? This final line of argument is compellingly persuasive to me, for it serves notice upon those who would use OR methods to use them properly or at the risk of such sanctions as may be imposed by ORSA.

I intend no adverse implication against Drs. Rathjens, Weinberg, and Wiesner for their refusal to participate in this inquiry. To do so would be analogous to permitting a prosecutor to comment on a defendant's refusal to testify on his own behalf, and is in either situation improper.

Question: Did Drs. Rathjens, Weinberg, and Wiesner hold themselves out to be expert practitioners of OR?

In Dr. Rathjens case the answer is clearly affirmative. In the Senate hearings his biography was presented to the committee members and printed in the hearings' report . . . His positions as Scientific Advisor to WSEG, Chief Scientist and subsequently Deputy Director of ARPA, and Director of IDA's Weapon Systems Evaluation Division and later Systems Evaluation Division mark him unmistakably as an operations analyst of great stature and high repute. His subsequent positions at MIT and Cornell indicate the enviable level of academic esteem he then enjoyed. [*See also* the Editor's Note after the Brooks letter.]

In the case of Drs. Weinberg and Wiesner the situation is by no means as clear. Inasmuch as they did not testify before the Stennis committee nor submit formal correspondence to the committee for its consideration (as did numerous other individuals and organizations, both for and against ABM deployment), their biographical information was not formally included for the record.

I have not had the opportunity to read *ABM: An Evaluation of the Decision to Deploy an Antiballistic Missile System*, edited by Abram Chayes and Dr. Wiesner, but I assume that the key sections of this book have been adequately identified, both by Dr. Wohlstetter (in his supplementary statement to the Stennis Committee on May 23, 1969) and by the Chronology (Annex to Appendix III of the *Ad Hoc* Committee Report). A few paragraphs quoted from the sections authored by Drs. Weinberg and Wiesner are admittedly slim evidence on which to base a conclusion, but they do present the image of refutations to acknowledged OR results (i.e., the analyses that Drs. Wohlstetter and Seitz offered to the Stennis Committee) apparently using similar methods and ground rules. On this basis I would conclude that Drs. Weinberg and Wiesner had at least purported to have employed mathematical models, probability estimates and sensitivity analyses. These are, of course, tools not proprietary to OR, but are indeed the proper tools to be used in many scientific inquiries. But in the context of

the operational effectiveness of Safeguard and the Soviet ICBM system, the principal topic, such statements and findings would lead a reasonable man to conclude that they were using operations research. Whether or not they were using it in an expert manner is a substantive question which I will not address here.

I would suggest for the Council's consideration that the book edited by Chayes and Wiesner might have been considered by ORSA for such review as occurs from time to time in the Analyst's Bookshelf section of *Operations Research*. In such case a critical review of the assumptions and calculations therein would have been entirely within the bounds of propriety.

Question: Was it ethical and proper for the *Ad Hoc* Committee to develop "Guidelines for Professional Practice" and then apply them *ex post facto* in this case?

That this was in fact done cannot be denied, and tends to detract substantially from the otherwise admirable devotion to traditional and familiar standards for inquiry into controversy. If "Guidelines" as published in the *Ad Hoc* Committee report was only a reiteration of principles previously enunciated and widely accepted (even if only within our own community), then no real impropriety could be assigned. But there can be no such assertion in view of the Committee's statement, "This document is the product of that Committee's endeavor and has been approved by the Council for widespread dissemination" (Introduction). Nor is there any assertion by the Committee that "Guidelines" is merely a compendium of rules already propounded.

Granted that the Committee was charged by the Council to develop "Guidelines," both the Council and the Committee should share joint responsibility for its *ex post facto* application in this inquiry. It would have been more in line with fair play had the Committee and the Council openly acknowledged this dilemma and admitted that, realistically, they had no other practical alternative (unless they were to adjudicate the controversy independently and with ill-defined rules, an even more distasteful alternative). On the other hand, it is easy to take offense at the procedure employed when one does not have responsibility for selecting the procedure. For this reason the Council and the Committee should not be admonished out of hand. I recommend to the Council that they openly acknowledge this breach of propriety. I am also quite pleased that ORSA now has clearly-defined standards by which to judge professional behavior, and heartily endorse them. These standards should be proposed by the Council to the Society for formal adoption so that, if in the future similar cases should arise, there can be no doubt of their applicability.

Question: Is "Guidelines for the Use of OR in the Adversary Process" necessary and sufficient?

I believe their necessity should be established on the basis of practicality, i.e., would they provide useful resources to the analyst who enters the adversary process? No doubt most of us are quite comfortable while engaged in the fascinating process of scientific analysis. But when we make the subtle and frequently unrecognized shift from analyst to advocate (a role for which our scientific training provides no rules), we enter a territory for which OR provides no landmarks and little protection. If we lose our way or are forced

to defend ourselves on our opponent's grounds, then it is little solace to us that our scientific analysis was superb, but we failed dismally to convince. In the adversary process, reason is no longer the ultimate authority. The successful advocate is almost without fail a profound student of human behavior and reaction, and understands that emotions are involved. For rank amateurs to enter into this process, be they highly distinguished professionals in OR or not, is temerity indeed. Some have learned this lesson and have quickly adapted themselves to the new ground rules, and have been eminently successful in either role. Most of us have not been as fortunate, yet we may be called upon to become an advocate, so it is to our advantage to be prepared. For this reason I deem the guidelines to be necessary.

As for their sufficiency I am far less persuaded. They give no warnings of the pitfalls that await us, no aids to recognize, avoid or combat these pitfalls. They merely hark back to the platitudinous caveat that we be sure our analysis is scientifically sound, and tell us to make our analysis available to our adversaries. They do not prepare us for conflict on a nonscientific basis, which is really what they should do. I conclude therefore that these guidelines are *not* sufficient, and recommend that the Council seriously consider their revision and expansion.

Question: Is the minority position of refereeing tenable?

I am quite convinced that the review proceedings discussed were extremely thorough and extended well beyond the usual refereeing of scientific offerings, in line with the controversial nature of these topics. Therefore, I conclude the answer is negative.

Question: Does the scope of the Committee's castigation of Rathjens, *et al*, on the basis of *de jure* violations of professional standards contain an inherent (albeit tacit) castigation of their professional ethics?

I think not. No doubt there are those who will draw their own conclusions as to the motives and ethics of the anti-ABM protagonists, and they are certainly free to do so. The Committee was quite careful to avoid such action, confining its criticisms solely to the methods used and assumptions made. I note that *Time* magazine capsulized the content of this report in a footnote to an article on the declining fortunes of congressional 'doves,' thus technically providing only factual background to the article. In the context, however, it emphasized this decline and seemed to hint faintly that ORSA's report was adverse on more than merely professional grounds.

Comment: I remain distinctly uncomfortable over the *ex post facto* nature of this review. In my ten years' association with ORSA I do not recall any specific concern expressed in *Operations Research*, either editorially or by specific contributors, over professional standards or discipline. Certainly "Guidelines" was long overdue, but its application in this controversy has unhappily blurred the focus of these proceedings. We, the members of ORSA as well as the successive ORSA Councils over the years, must share the responsibility for failure to foresee the possibility of such an event along with its detrimental effects. In other words, we failed to examine all the reasonable alternatives—a damning indictment for operations analysts! [See paragraph I of Miser's reply to the Gass letter.]

Very well, let us admit what has been done was necessary, perhaps not altogether satisfying, but acceptable. We should now take the essential step

of ratifying the most important product of the *Ad Hoc* Committee: the "Guidelines." Its impact will remain long after the furor over its violation has passed, hopefully to serve in a preventative capacity rather than a judicial one.

Note

1. *AUTHORIZATION FOR Military Procurement, Research and Development, Fiscal Year 1970, and Reserve Strength*, Hearings before the Committee on Armed Services, U.S. Senate, Ninety-First Congress, First Session on S.1192, S.2407, and S.2546, Part 2.

George B. Kistiakowsky, Herbert Scoville, Jr., and Herbert F. York;
Cambridge, Massachusetts; McLean, Virginia; La Jolla, California.

Editor's Note: This letter was originally addressed to the Editor of the *Washington Post*, and appeared in its columns on November 17, 1971, along with a letter from George W. Rathjens, also on the subject of the "Guidelines" report. The first of these letters was submitted to *Operations Research* for publication, but the second was not.

In September 1971, an *Ad Hoc* Committee of the Operations Research Society of America (ORSA) issued a report censuring, as not being up to the standards of the Society, the Congressional testimonies in opposition to the Safeguard ABM system, of Dr. Jerome Wiesner, President of MIT, Drs. George Rathjens and Steven Weinberg, Professors at MIT, and to a lesser extent, Professor Wolfgang Panofsky, Stanford University. Predictably, this was put in the *Congressional Record* by Senator Jackson, the strongest proponent of Safeguard in the Congress. On October 13th, Donald Rumsfeld, Counsellor to the President, wrote from the White House a letter to Robert Machol, president of ORSA, stating that the report had been discussed personally with President Nixon, that Admiral Zumwalt, Chief of Naval Operations, discussed the work in a most favorable way, and that "you and the Society have performed a magnificent service." To cap it off, on November 8th in a column which you [the *Washington Post*] carried, the columnist, Joseph Alsop, extolled the report and decried the dishonesty of the opponents of Safeguard.

Just to keep the record straight, we think it should be known that five members of the ORSA Council issued a minority report questioning the propriety, impartiality, and ability of the *Ad Hoc* Committee to carry out such an investigation. This minority report was not mentioned by Senator Jackson and others. More importantly, this unprecedented investigation was carried out at the request of Professor Albert Wohlstetter, the leading nongovernmental protagonist for Safeguard, confidant of Senator Jackson, and consultant to Admiral Zumwalt. Professor Wohlstetter who was praised in the report, is a member of ORSA; the other witnesses who were censured are not members and did not participate in the work of the Committee.

Although the primary purpose of the Congressional hearings was to determine the need for Safeguard, and whether, indeed, it would work, the ORSA Committee study never even addressed the testimony on these central

issues, but instead concentrated its attention on several subissues such as (1) the vulnerability of our Minuteman deterrent to an administration postulated Soviet threat, and (2) the Soviet ability to destroy both our Minuteman missiles and bombers in a simultaneous attack. These subissues were suggested to ORSA by Professor Wohlstetter, one of the protagonists in the debate whose standards the ORSA Committee was, in theory, investigating. This action by the Committee would appear highly unethical, and the ORSA Council's sensitivity to such a charge is demonstrated by the fact that it excised from its published version of Professor Wohlstetter's letter those portions directing their attention to these subissues.

We think everyone should ask whether a group that employs such standards in its investigations has demonstrated the competence to evaluate the standards of such eminent scientists with a record of dedication to public service as Drs. Wiesner, Rathjens, Weinberg and Panofsky. We think one must conclude that the ORSA *Ad Hoc* Committee—and those Council members who endorsed the report—have just become additional protagonists in the continuing ABM debate along with Professor Wohlstetter, Senator Jackson, the White House, and Joe Alsop.

Hugh J. Miser (II), *Editor, Operations Research, Windsor, Connecticut* (this reply to the Kistiakowsky, Scoville, and York letter was also addressed to the *Washington Post* and appeared in its columns on December 22, 1971).

With respect to the report of the *Ad Hoc* Committee on Professional Standards of the Operations Research Society of America discussed in your [the *Washington Post*] letter columns on November 17, one has to agree with George B. Kistiakowsky, Herbert Scoville, Jr., and Herbert E. York that it is important “to keep the record straight.”

To this end, however, from facts obvious from the report (which bent every effort to be open and candid) they selected four on which to base attacks on ORSA's ethical and professional standards: (1) that five of the 13 members of ORSA's Council issued a minority report, (2) that Albert Wohlstetter had requested the investigation, (3) that an appendix reproducing Wohlstetter's November 8, 1969, letter omitted some portions of it, and (4) that Wohlstetter is a member of ORSA while George B. Rathjens, Jerome B. Wiesner, and Steven Weinberg (all at MIT) are not.

However, one wonders why other equally obvious and more relevant facts prejudicial to their theses were omitted. To keep the record straight, here are several: (1) One of the purposes for which ORSA was chartered is “the establishment and maintenance of professional standards of competence for work known as operations research;” (2) ORSA's investigation was limited to this aspect of the 1969 ABM debate; (3) the three MIT nonmembers were sent the complete text of Wohlstetter's letter on November 14, 1969; (4) on the same date they were invited to participate in the investigation; (5) this invitation was contemptuously refused; (6) four months before the issuance of the ORSA report, all persons significantly mentioned in its findings were sent complete draft copies of it and invited to comment, with an offer to publish the result and to share proof sheets prior to publication; (7) the MIT

group also refused this offer; (8) a majority of the ORSA Council voted on May 6, 1971, to publish the report; (9) the minority report was filed on August 19, 1971, over two months later; (10) this minority report did not question any of the *Ad Hoc* Committee's substantive findings; (11) on the day the ORSA report was issued, the MIT group released a major critical statement to the press, which they did not share with ORSA officials. All of these facts but the last were published in the ORSA report.

The portion of the ORSA report devoted to the 1969 ABM debate is a severely analytical inquiry into the professional standards exhibited by participants in carefully selected portions of that debate, and the findings relate rigorously to this subject. Regrettably, the report finds numerous lapses from high professional standards, all carefully documented. *As I write, none of these findings has been shown to be in error*, either by the November 17 correspondence or by other publications.

On the other hand, the authors of your November 17 letters (Rathjens, as well as Kistiakowsky, Scoville, and York) use the press to fault the report for not conducting wider analyses of ABM (a purpose specifically disavowed as inappropriate for ORSA), to renew an unprofessional *ad hominem* attack on a member of the committee, and to mount an unfounded assault on the standards of the committee and the Society as a whole.

I would be the last to argue the technical or moral perfection of the ORSA report or its preparers. However, if the report is to be discredited, surely one must show by rigorous analysis that its important findings are incorrect; shabby or peripheral arguments cannot do this job.

The central thrust of the ORSA report is that there is a need for professional standards to govern the activities of scientists who give advice based on either their technical specialties or general scientific background. For the arena of public-policy debate, certainly the letters of Rathjens and of Kistiakowsky, Scoville, and York in your November 17 issue add emphasis to this finding.

R. Stanley La Vallee, *Alexandria, Virginia.*

I was surprised but pleased to read the September issue of *Operations Research*. I think it appropriate and timely that the ORSA council addressed this problem and publicized the results. Before I make other detailed comments, I wish to state that I have not been involved in ICBM or ABM work and did not follow the public debate closely.

Senator Jackson's question is a most proper one, and one that bothers many concerned laymen. Those who still believe in science as a truth-seeking process are confused rather than enlightened by scientific experts arguing opposite sides of a problem. Thus it behooves scientists on both sides of a debate to make explicit in so far as possible their assumptions, sources of data, and methods of analysis. In this way the sources of disagreement can be better pinpointed and the public should be in a better position to judge whether the scientist is an expert in the particular area of disagreement. All disagreements ultimately are, or should be, judgments, e.g., the importance or relevance of a qualitative factor, or the accuracy or estimate of a quantitative parameter.

Thus I believe that the task the committee undertook was proper, and the scope, with some minor exceptions noted later, was adequate to the purpose.

The letter of Wiesner, et alii (Appendix IV D) makes the valid point that OR is only a part of the total ABM debate, and that other issues, like the validity of technical data, are of more importance. But, as an MIT alumnus, I was very disappointed that they refused to assist the project, and they did a disservice to the technical community. As scientists they should be trying to get at the truth of the matter, or at least assisting those who are. The guidelines for OR set down in the main body are basically only the scientific method particularized to operations research. Surely, Wiesner, et alii wouldn't object if only it, and not the appendices, were published?

Operations research is the appropriate discipline for the ABM problem and its total evaluation. But OR, as any other technical discipline, requires and depends upon good technical data. These data, in turn, are generated by other disciplines, engineering design, nuclear weapons effects, aerodynamics, electrical engineering, etc. I believe that most of the real differences lie in the supporting-analysis areas. The OR part of the problem is reasonably straightforward; the models and their biases are well known. Mr. Wohlstetter performed a useful function in comparing his and Rathjens's analyses and pinpointing the specific differences in assumptions and data.

I was particularly dismayed by some of the arguments presented by Wiesner, et alii in their letter. They do the same thing they accuse their opponents of doing. They too shift their arguments. They don't address the particular differences or errors of data interpretations but bring up new and extraneous points, such as the use of Air Force aircraft for proponent transport. They also use the same McCarthy tactics they so deplore in insinuating base motives to the committee—or at least bias—in their references to the Spanish Inquisition and an attack on civil liberties.

I believe that part V of Appendix III is very useful and if developed early in the debate might have furnished some useful guidelines to the debate. But I have a quarrel with section V C and also with parts of section IV of Appendix III. I believe the committee should not have gone so far as to make a technical judgment on the use of reprogramming missiles, or the coordinated attack.

First, on reprogramming, I agree that it is technically feasible to reprogram. Telemetry or radar can rapidly detect a launch or booster failure and send signals to a computer, which could reassign targets to as yet unlaunched missiles, and reset the target coordinates into the guidance computer in each missile or ground control point as the case may be. However, this can be done very fast only if everything is automated. But if the Russians, for any one of a number of reasons (cost, control, etc.) put a man in the loop (e.g., to approve the target reassignment, or to physically change the target settings in the guidance computer) the time required increases, and the probability of retargeting in a given time decreases. *Technical feasibility is not a sufficient argument.* This is a worst case, and the intelligence community several years ago was forced to change its practice of presenting the worst-case threat as a basis for requirements analyses. Thus, the degree to which the Russians provide an automatic retargeting system is a matter for technical judgment where hard data are not available, of the degree of risk we, and the Russians, are willing to assume.

There are many precedent examples where the Russians have opted for simple equipment in large numbers, rather than technically feasible but more complicated and expensive equipment.

The analysis of coordinating strikes on missiles and bombers has similar defects. The committee should not have judged it to be unrefuted by ABM opponents. Again, technical feasibility is not enough, and the requirement that the opponents must examine all reasonable tactics and prove that none of them succeed is too stringent and one-sided. No side can examine all the possibilities because of time and cost constraints. And even one tactic requires many variables whose values are subject to question and technical judgment. The success of such an attack depends upon such factors as the timing error in launch coordination, the variance of warning time available from detection to impact for targets, response launching times, vulnerability of ICBM's as a function of distance from warhead detonation, and ICBM altitude and speed. All these parameters have variances. Even with perfect knowledge of our target locations, detection-sensor locations and capabilities, the Russians may decide that the risk of premature warning may be unacceptable. After all, this very real technical and coordination problem is the justification for our building and maintaining a mixed strike force, rather than putting all our resources in the most cost-effective system.

My quarrel with section V C of Appendix III rests on similar grounds. It puts too much burden of proof on ABM opponents to say that they must show for all reasonable sets of future conditions that ABM is undesirable. It is unrealistic in the first place because of the time and effort required; secondly, it assumes that there is one dominant choice for all conditions. To address the first objection, it seems to me sufficient if the opponents could take the same reasonable set of future conditions that the proponents did and prove that the ABM was not the most desirable. They might do this by assuming different missile reliabilities, vulnerabilities, response times, etc., which are not strictly OR questions, but ones of engineering judgment.

Assuming they can't do the above, the opponents, however, might find another set of reasonable conditions for which something else, say more Poseidon, was preferable to ABMs. And if they make a case that their set of future conditions is more likely than those of the ABM proponents, who wins the argument? While the ABM might not be disproven, at least a mixed strategy is proven, and perhaps both systems should be bought.

I believe the drug analogy, somewhat oversimplified, is appropriate here. Prior to new testing requirements promulgated a few years ago, a drug company had to prove the drug was useful, i.e., provided the only, better, or cheaper, cure for a specific illness or malady, before it could be licensed to sell it. Adverse-, or side-effects testing requirements were comparatively small. The new law greatly expanded the requirements for demonstrating that side effects were minor. Since the number of things that might go wrong with a drug, and affecting parameters, outnumbers the benefits by an order of magnitude, the amount of testing required goes up tremendously. This has significantly slowed down the introduction of new drugs and increased their cost. If your standards were applied, either all drugs would be licensed or none would, depending upon which side you apply your argument to. If a drug is useful for one particular disease and type of person, then obviously

the opponents can't show that the drug is bad under *all* conditions. Conversely, if the drug has one bad side effect under certain conditions, then the drug would not be approved because it can't be proved beneficial under *all* conditions.

This is my second objection to your proof requirements. Since no proposed system is all good or all bad, I believe that the *all* requirement of the debate should be relaxed, and replaced by some risk analysis, preference criteria, or game theoretic measure appropriate to the problem.

Now let's look forward a minute. A good step has been taken, although it is probably disputed. Since it is important, I suggest that the ORSA Council request some sufficiently prestigious organization, such as the National Academy of Sciences, or the AAAS, to review this effort and its products. Surely the Council would have no objection to such a review, and it should meet Wiesner's objections. Such a review committee would set their own scope, but I would hope that it would address the following questions:

- (1) Are the proposed guidelines useful, satisfactory, desirable?
- (2) Was the effort resulting in the report adequate, complete, unbiased, overdone, worthwhile?
- (3) Should the Appendixes have been published, or just the guidelines?
- (4) Are the various appendixes and their components adequate, biased, erroneous, useful, essential, uncalled for?
- (5) Is it desirable or necessary for other technical disciplines, via their professional societies, to draw up similar guidelines?
- (6) Is it desirable for some group to draw up guidelines for debate on important national issues, including evaluation criteria and logic rules of debate? If so, what is the mechanism?

I believe the answer to (6) is yes; it would be a positive, dynamic, living result of this inquiry. The group drawing up the guidelines should be organizationally independent, perhaps part of GAO, or set up in similar fashion. Either on their own, or upon request from either side of an important national issue, they would define the issues, important parameters, and evaluation criteria. While these obviously wouldn't be binding on anyone, it should improve the quality of the debate, even from the politicians. At least the guidelines will assist others in deciding whether someone's statements are relevant.

Hugh J. Miser (his letter I follows the one from Gass; his letter II follows the one from Kistiakowsky, Scoville, and York).

Philip M. Morse, *First President of ORSA, Massachusetts Institute of Technology, Cambridge, Massachusetts.*

I wish to dissociate myself from the action taken on behalf of the Society by its Council in approving the Report published in September, 1971. I do not approve of the Society criticizing, by name, nonmembers of ORSA and I disagree vehemently with the implications, which the general public, the Defense Department, and the Congress inevitably read into this Report, written as it was.

My reaction is much broader than the minutiae which fill the appendices to the Report. My criticism has to do with the dangers inherent in ascribing reality to the results of a mathematical analysis of a situation, without first examining carefully the assumptions on which it is based. It also has to do with the way supposedly technical reports can be used for nontechnical purposes.

The basis of my criticism lies in my experience in operations research, which goes back to the beginning of World War II. I was Director of the US Navy Operations Research Group then. We learned from hard experience that the results of mathematical analyses could be of immense value in tactics, *only* when we were reasonably certain as to the behavior of our own weapons *and* of the tactics and weapons of our enemy. During a war there are tough but necessary ways of gathering data on these two points, data from the battle itself. And if the data-gathering is *scientifically* done and efficiently processed, one can hope to obtain accurate-enough parameters for the analyses, so that correct tactics can be initiated before the enemy catches on.

We soon learned that the estimates of effectiveness of our weapons, given us by the designers of the weapons, were nearly always overoptimistic, compared with their effectiveness when used by soldiers in war-time conditions. In fact the discrepancy usually turned out to be a factor of three in degradation of effectiveness. We also learned, the hard way, that estimates of enemy intentions and capabilities, obtained from 'intelligence,' could be just as badly off, unless tested in battle or checked against past experience of enemy behavior. Until these uncertainties in *assumptions* could be narrowed by operational data, the predictions of our beautifully accurate *analyses* could be wrong by a factor of as much as ten. Only by applying the pessimism of experience could we arrive at *safe* predictions, to apply before operational data were available to check our assumptions (and the important goal was to get this operational data as quickly and cheaply as possible).

But in peace time these operational data are not at hand, happily for the country but unhappily in regard to predictions of weapons effectiveness. This I learned (again the hard way) when I was first Deputy Director of the US Joint Chiefs' Weapons Systems Evaluation Group (WSEG) in 1949. When we were trying to predict the effectiveness of the B-36, we were given the optimistic estimates of the B-36 designers and the 'intelligence' estimates of enemy capabilities, which varied widely from month to month. Our first task was to reduce these guesses to some set of realistic *assumptions* to put into our calculations to predict operational losses and successes. Luckily, at the time, we had enough officers and civilians with war-time experience so we could get agreement on the degree of operational degradation we had to apply to these guesses, and could come up with realistic assumptions. And I believe our resulting predictions of the inadequacies of the B-36 were eventually agreed to by everyone (including General LeMay!).

By now a new generation of officers and analysts has come on the scene, many of whom have never had the sobering experience of seeing their optimistic predictions disproved by deaths on the battlefield. They too often are willing to take the assumptions given them by designers and by 'intelligence' as gospel truth, and to base their calculations on them without

adding any correction factors for 'the fog of war.' This is why I believe the arguments regarding details of the analysis of ABM effectiveness are beside the point. Strategy is still an art, not a science. Science can help in many ways, but final decisions must be based on experience and judgment.

At one point in the ABM hearings I understand one senator asked how it happened that scientists, who were supposed to know 'the truth,' could so convincingly come out with such widely different predictions. To this sensible question I can offer two suggestions—perhaps answers. The first is that the scientific method is dual, the use of both experiment and theory to predict future phenomena. The experimental data result in parameters (assumptions) to be inserted in the theory (analysis) to produce the predictions. The immensely complex calculations needed to land the Apollo missions accurately would have been useless without precise values of the experimentally determined data on the gravitational fields of the earth and the moon. Anyone who argues about a possible 5 or 25 per cent error in the calculations of missile vulnerability without first making sure of the input parameters, of *realistic* values of our weapon capabilities and the potential of our enemy (to at least that same accuracy) is not arguing scientifically. And, in the present circumstances (thank God) we have no means of experimentally determining those input parameters to that accuracy.

Therefore the argument must be based on experience and judgment, as to the most likely values of these input parameters. I, for one, would rather place my bets on the judgment of those who have had broad experience in strategic weapons evaluation, such as a Presidential science advisor, rather than on the opinions of an analytic specialist or a protagonist of some new weapon. The argument here is not about the truth or falsity of some scientific law, it is about the plausibility of the assumptions on which the calculations are based. And I submit that Congressmen are as capable of judging the experience and realism of the protagonists as are any technical expert. These are *not* questions of science, they are matters of judgment, based on experience. Anyone who tries to argue his case for the ABM on the grounds of the accuracy of his analyses is either scientifically naive, or else he thinks it doesn't matter, that the time will never come when hard reality will provide a check on his assumptions.

My second suggestion is more tentative. There is a whole gamut of 'sciences,' from the 'hard sciences' to the 'soft sciences,' which can be graded in regard to the ease or difficulty of determination of their assumptions. To a physicist or chemist the experimental determination of the theoretical assumptions, the basic parameters, is as important to him as is the mathematics of the theory which uses the parameters: it is all a part of his science. The economist or the sociologist has a much harder task in verifying his assumptions quantitatively. In many cases agreement on assumptions must as yet be based on polemic or on philosophical arguments rather than on verifiable, quantitative experiment. Unfortunately, when studying military strategy or pollution or energy needs for the country, when both 'hard' and 'soft' scientists participate, this distinction between assumptions based on measurement and assumptions based on judgment is often forgotten, or misunderstood. And at times a nonexperimentally-verifiable assumption, or the result of analyses based on such an assumption, is

presented as a scientific conclusion. In such cases one of course will find scientists reaching widely varying conclusions, depending on the assumptions they have made; the differences are not in the science but in the experience and judgment of the protagonists.

This is of course not to say that science is of no help in cases where assumptions cannot be verified experimentally, under realistic conditions. Analysis can work out the consequences of the assumptions, and often can work out ways whereby some assumptions can be verified with minimum effort and disruption. But both scientists and Congressmen must distinguish between the verifiable, scientific part of the study and the 'unscientific,' as yet unverifiable assumptions, which must be decided, hopefully, by the best judgment available. That best judgment may be that of an experienced scientist or it may be that of a Congressman or a general; it shouldn't be called science.

As I said at the beginning, I am not an expert on ABM's, but I have had experience in many similar problems, both military and nonmilitary, and I am sensitized to the crucial importance of distinguishing between science and assumptions based on judgment. I am not ashamed, as a scientist, to disagree violently with a fellow scientist on matters based on experimentally unverifiable assumptions. I might not think much of my opponent's judgment, but I certainly would not say he was unscientific.

Thomas W. Sikes, *Thiensville, Wisconsin* (in a letter to Robert E. Machol, *President of the Society*).

All those of our profession who wish to promote intellectual honesty in public debate and who wish to foster high professional standards in operations research owe a debt of gratitude to you, to the ORSA Council, and to the members of the ORSA *Ad Hoc* Committee on Professional Standards for the work you have done, as recorded in the September 1971 ORSA Journal.

Walmer E. Strobe, *Falls Church, Virginia*.

The report of the ORSA *Ad Hoc* Committee on Professional Standards contained in the September issue will be a landmark in the upgrading of professional conduct. As a member of ORSA, I take pride in the courage of the Council in undertaking the inquiry and in the careful manner in which it was carried out. Let us hope that the quality of 'expert' testimony on important public policy issues shows improvement in the future.

Gifford H. Symonds, *University of California, Berkeley, California*.

The Operations Research Society through its *Ad Hoc* Committee has (1) defined operations research, (2) set guidelines for its professional practice,

(3) presented an analysis and review of a case of questionable operations-research practice in a major national issue, and (4) considered the OR professional person in an adversary role.

Despite the statement that the Society has not taken a position with regard to the case of operations-research practice cited, it must be recognized that the activities of the *Ad Hoc* Committee are official pronouncements of the Society unless voided by the Council or the Membership. The definition and guidelines for practice seem to be in accord with the objectives of the Society for the advancement of operations research. The analysis and review of the case study would seem to be appropriate to the objectives of the Society for the establishment and maintenance of professional standards, primarily because of the wide publicity given to the operations-research practice in the important ABM issue. The OR professional person in an adversary role is a most important matter. These activities of the *Ad Hoc* Committee definitely set a new precedent for improved control of the profession of operations research involving the relationship between the OR practitioner and the client or the public, and the quality of the OR work performed.

It is also to be expected that the Society will place more emphasis on the designation of members who have demonstrated professional competence in operations research so that the clients will have more confidence in the practitioner. The profession of operations research requires such standards and protection because of the fiduciary nature of its practice. On the other hand, the development of the science of operations research, the basic and fundamental knowledge underlying the professional practice of operations research, whenever separate from professional practice, does not require the same limitations needed by the profession. However, all new work in operations research proposed for publication, such as manuscripts and proceedings, whether in science or in practice, continue to be subjected to the referee process and all significant work already in the public domain, such as books, reports, and important public testimony, undoubtedly should be subjected to the review procedure. For greatest support for the advancement of operations research, the Council should accept full responsibility for the work of its officers, editors, and committees, not merely provide a forum for their individual opinions.

The question of adversary professional work does not appear to be adequately treated by the *Ad Hoc* Committee. Of course, it is most desirable that there should be no difference in OR professional work on any given problem regardless of which side of an adversary case is represented. Usually the problems and assumptions are different for the different adversaries, and sometimes the different sides in an adversary case do not even consider the same subject. However, the OR professional person should be concerned with the problem and the assumptions as much as with the results in an attempt to seek the truth. Any professional person who would not seek the truth or who would subvert or even pervert an operations-research study to gain a preconceived result is doing considerable harm to the operations-research profession. This should be made clear. Some further consideration of adversary professional work seems to be needed.

Michael J. White, *Department of Political Science, The University of Georgia, Athens, Georgia.*

I have just seen . . . [the special "Guidelines . . ." issue] of *Operations Research*. As one who has studied the OR profession for several years now, as part of Mike Radnor's program at Northwestern, I find the issue very significant in the emergence of OR as a profession equal to the classic ones of law, medicine, architecture, etc.

NEWS STORIES AND ARTICLES FROM OTHER PUBLICATIONS

(All dates are in 1971)

October 1: William Beecher, "Report on Safeguard ABM Testimony Finds Unprofessional and Misleading Comments on Both Sides," *The New York Times*, page 23.

Michael Getler, "Colleagues Criticize Anti-ABM Scientists," *The Washington Post*, pages 1 and 16.

Senator Clifford Hansen of Wyoming introduced the previous item into the Congressional Record, pages S15634-S15636.

October 2: Michael Getler, "Report Says MIT Scientists Misused Facts in Opposing ABM," *The Boston Globe*, page 2 (a copy of the *Washington Post* story of the previous day).

Andrew F. Blake, "Wiesner Says Personalities Played Role in Critical Report," *The Boston Globe*, page 2.

Philip M. Morse, "Prof. Morse Protests ORSA Report," *The Boston Globe*, page 8.

Editorial, "Science and McCarthyism," *The Boston Globe*, page 8.

October 4: Senator Henry M. Jackson introduced into the *Congressional Record*, pages S15720-S15737, the October 1 *Washington Post* and *New York Times* news stories, plus all of Appendix III of the "Guidelines . . ." report.

October 9: "ABM Foes Employed Misleading Data, Study of the Debate Contends," *The National Observer*.

October 11: *Time* magazine carried a two-sentence footnote, page 21.

October 12: Robert L. Bartley, "When Science Tangles with Politics," *Wall Street Journal*, editorial page.

Senator Gordon Allott of Colorado introduced the Bartley article into the *Congressional Record*, pages S16173-S16174.

October 15: Senator Allen Cranston of Rhode Island introduced into the *Congressional Record*, pages S16332-S16338, three items: the October 2 editorial from the *Boston Globe*, Professor Morse's letter published in the

Boston Globe the same day, and a long statement by Rathjens, Weinberg, and Wiesner that had been issued to the press on October 1, and on which most press stories had commented.

Nicholas Wade, "ABM Debate: Learned Society Split by Old Grievance," *Science*, pages 276-277.

Our Washington Correspondent, "Fighting the Great ABM Battle Again," *Nature*, pages 442-443.

October 19: Senator Edward M. Kennedy of Massachusetts introduced into the *Congressional Record*, pages S16457-S16463, two items: the *Washington Post* story of October 1 and the Rathjens-Weinberg-Wiesner memorandum (the first of these items had already been introduced on October 1 and October 4, and the second four days earlier on October 15, as listed above). The memorandum was accompanied by a transmittal letter signed by Rathjens, not previously introduced.

October: Truman Botts, "Professionalism in the Practice of Operations Research," *Newsletter* of the Conference Board of the Mathematical Sciences, page 9.

November: "Judgment," *Scientific American*, page 48.

November 7: Victor K. McElheny, "White House Praises Group that Hit ABM Critics," *The Boston Globe*, pages 29-30.

November 8: Joseph Alsop, "Misleading Scientists," *The Washington Post*, editorial page (also published in other papers on November 8 and 9 under other headlines).

Editorial, "Fallibility," *The Baltimore Sun*, page A10 (appeared on the same page as the Alsop column).

Senator Clifford Hansen of Wyoming introduced the Alsop column just listed into the *Congressional Record*, page S17764.

November 12: Senator Stuart Symington of Missouri introduced into the *Congressional Record*, pages S18320-S18323, the letter by Richard L. Garwin that appears above (the version appearing above has been slightly revised by the author).

November 17: Under the heading, "On the Operations Research Committee Report and the ABM Debate," *The Washington Post* published two letters, one by G. W. Rathjens, and one by George B. Kistiakowsky, Herbert Scoville, Jr., and Herbert F. York (this latter item is reprinted above).

November 19: Senator Adlai E. Stevenson, III, of Illinois introduced into the *Congressional Record*, pages S19073-S19074, the Alsop column of November 8 and the two letters just listed.

November 23: Crocker Snow, Jr., "Can Professor Be US Consultant and Adversary at Same Time?," *The Boston Globe*.

William F. Buckley, Jr., "Scientific Objectivity Yields to Twaddle," *Chicago Daily News*, page 11 (also published in other papers).

December: "Special Issue on ORSA ABM Report," *F.A.S. Newsletter*, Federation of American Scientists, 203 C St., N.E., Washington, D.C. 20002.

"Professionalism vs. Super-Computer," *Technology Review*, Massachusetts Institute of Technology, Cambridge, Mass., p.68.

December 22: Under the heading "Further on the ORSA Report and the ABM Debate," The *Washington Post* published a letter by Hugh J. Miser replying to the November 17 letters that had also appeared in the *Post* (this reply is reprinted above).

January 1972: Leslie C. Edie, "'Guidelines for the Practice of Operations Research' Discussed in Special Session at ORSA Meeting in Anaheim," *OR/SA Today*, Operations Research Society of America, Baltimore, Md., pp. 6-10.

Wolfgang K. H. Panofsky, letter to the editor, *Scientific American*, p. 6.

Edward Shils, "The Obligations of Scientists as Counsellors," *Minerva*, pp. 107-111. This journal also reprinted (pp. 111-157) a substantial portion of the "Guidelines . . ." report.