so, but nevertheless constructive. There is now, in this country, too much fractionalization and dissension between groups of one sort or another. Perhaps we can be the first to call a halt to name-calling and petty bickering, and get on with the business of creating some kind of future for this country. In this spirit, then, let me be the first to open with an honest salvo and close with some tentative, but hopefully constructive, ideas.

As the ranking minority member of the Appropriations Subcommittee charged with funding the NSF and NASA, and as a member of the Senate Defense Appropriations Subcommittee, permit me to observe that the admitted lack of expertise on the part of a majority of members of Congress in areas relating to scientific achievements is, regrettably, matched only by the lack of appreciation on the part of many research scientists, engineers, and technical managers of congressional processes and problems. On occasions so rare that I can scarcely recall, have I ever received comments from those in the scientific community relative to how their operations might possibly be improved, where the waste is, where the duplication is, where the inefficiency lies, what the real difficulties and problems are and how they can help. Quite frequently, however, I receive mail from individuals asking for more and more funds from the federal treasury, and one theme is fairly dominant—cut some place else in the budget, but do not cut my research project. Gentlemen, we have only so much money to expend. We are limited, to a great degree, by revenue taken in by the Treasury if we are to make the financing of our national debt manageable. Within our admitted lack of expertise, coupled with an appalling lack of national goals or a system of priorities, I think we do a fair job of spreading out the federal dollar. We could do better, though, with some constructive help from the scientific community from an objective and realistic appraisal of the circumstances and of existing realities, and we could benefit from the establishment of some system, either a Joint Committee or something similar, which would view research on an overall basis, which would review national goals and aspirations and which might have an opportunity to make a stab at setting up some type of priority list, insofar as funding needs are concerned. I would think, also, that the country might well benefit if, paraphrasing both Donald Hornig and the "now" generation, the scientific community would become "involved," would drop the cloak of mystery, and take the time to explain, not just to us in the Congress, but to Mr. Taxpayer as well, just what it's all about. This would be a tremendous contribution and definitely a forward, positive step in the national interest.

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Reducing Hail: The U.S. Plan

In telescoping news of a research program on the suppression of hail into a single paragraph in the 6 September issue (p. 995), *Science* may have misled its readers concerning the research in this field currently being carried out under NSF sponsorship by the National Center for Atmospheric Research, the Environmental Science Services Administration, and Colorado State University.

It is true that the Russians have reported success in reducing hail damage to crops in certain areas by firing antiaircraft shells containing silver iodide directly into hailstorms. But the implication that a similar program is now underway in the United States is misleading.

The American operational effort is likely to be quite different, and it is several years away. The most likely vehicle for getting the silver iodide into the clouds now appears to be lightweight rockets fired from aircraft. Before such an operational test can be carried out, however, additional research is required to answer questions concerning (i) the mechanics of Great Plains hailstorms and specifically the nature of the "hail accumulation zone" in such storms; (ii) radar techniques for identifying potential hailstorms and for timing the rocket firings; (iii) techniques for measuring the extent and intensity of hailfall from seeded and unseeded storms; and (iv) development and test of the rockets. It is on these subproblems that NCAR, ESSA, and CSU are engaged.

Meanwhile, a plan for a national program to proceed from research to tests of operational methods is now under consideration by the Interdepartmental Committee for Atmospheric Sciences, a subgroup of the Federal Council on Science and Technology. The details of such a plan will have to be discussed

thoroughly, not only among the participating agencies and research groups but also with public officials in the area, before large-scale field operations are carried out.

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Inventions Beg Application

The high level of abstraction employed by Emmanuel G. Mesthene ("How technology will shape the future," 12 July, p. 135), in discussing the ways in which technology will affect the future may have led him to neglect some down-to-earth economic considerations. For example, he writes: "There is nothing in the nature or fact of a new tool, of course, that requires its use . . . but there is a high probability of realization of new possibilities that have been deliberately created by technological development, and therefore of change consequent on that realization." Is there *really* a high probability of application of newly invented tools?

A careful review of the evidence would, I believe, indicate that the probability of eventual application of any invention is, on the average, rather low as many disappointed inventors will testify. Ray Vernon, Tom Schelling, or any of his other economist colleagues at Harvard could have pointed out to Mesthene that invention is not enough: there must also be a mechanism which guarantees that opportunities will be recognized and seized. Knowledge and enterprise must be present. The merits of a new technology to businessmen are related to costs and profits. History is filled with examples of inventions that went without commercial application for years, or were introduced but failed, or were never adopted because market conditions were not propitious. Many patents on new ideas have never been exploited commercially and probably never will be.

Expanding on Mesthene's borrowed analogy, I would say that a new device may be like a newly opened door which invites one to enter, but the anticipated costs and satisfactions of crossing the threshold are a key factor in shaping one's decision.

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