Weather Modification

In his report "Weather modification: Colorado heeds voters in valley dispute" (News and Comment, 29 June, p. 1347), Luther Carter states that there was "irony" in my simultaneous support of the weather modification program proposed by the (San Luis) Valley Growers, Inc., and of the National Hail Research Experiment (NHRE). This statement indicates a failure to appreciate the important difference between the decision processes involved in a scientific experiment and those involved in a user group's consideration of whether or not to employ weather modification technology.

For a scientific experiment, the point of view is that of a (properly) critical scientist for whom a high confidence level (typically 90 percent or higher) is necessary before apparent effects can be accepted as real. User groups may not be able to afford the luxury of waiting to solve their problems until the confidence level becomes that high. Such user groups have adequate justification in asking for the use of available weather modification technology before *complete* answers are available.

I support the NHRE and its stated goals of gaining increased understanding of hailstorms and developing practical methods for suppressing hail (*1*). The NHRE has provided an opportunity for increased understanding of hailstorms (*2*) in far greater measure than is likely to be possible in any commercially oriented program. However, the NHRE, in spite of the relatively large scale of the program, cannot be expected to field test all possible techniques for hail suppression. Indeed, background planning for the NHRE provided for a "focus . . . on a single seeding technique . . . introducing artificial hail embryos directly into . . . the cloud" (*3*). A review of the results of other seeding procedures strongly suggests that such procedures can reduce damaging hail (*4*).

My appearance at the Colorado hearing and at a prior public meeting in Alamosa was at the request of Valley Growers, Inc., who asked me to review the experience gained in field experiments on the effects of cloud seeding on rain and hail. Recognizing the differences noted above in the decision processes involved, we are on record as early as 1966 as supporting both experimental programs and attempts at the application of weather modification technology where such projects were desired by local user groups (*5*). Our experience since, which shows that substantial benefit can result for the research and operational programs from exchanges of information, has substantiated the validity of this position.

Carter makes no mention of the data presented by the applicant, Atmospherics, Inc., which showed that five official rainfall reporting stations of the National Oceanic and Atmospheric Administration in the San Luis Valley experienced substantial positive departures from normal in July and August, from 1967 through 1972, the years when weather modification programs were conducted (*6*). An independent analysis of the program by Grant *et al.* (*7*) for the Colorado Advisory Committee on Weather Modification and the director of the Colorado Department of Natural Resources was also offered at the hearing by the applicant. It showed a high probability that the positive precipitation anomalies were associated with the seeding and also documented recent substantial increases in ground water pumping and surface water diversions. However, the report was not permitted to be entered into the hearing record because Grant was a member of the Colorado Advisory Committee on Weather Modification.

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In his report on resistance to weather modification projects, Carter does not mention an interesting historical precedent for voter suspicion of weather modification, even when it is sanctioned by the U.S. government.

In 1871 a Wisconsin civil engineer named Edward Powers published a book entitled *War and the Weather* (*1*) in which he produced new "evidence" for an old theory. It has been claimed since antiquity that rain tends to follow major military engagements, and in more recent times it has been argued that it is the concussion of the artillery that actually provokes the rain, usually within a day or two. To gather evidence for this hypothesis, Powers examined log books and wrote many (perhaps hundreds) of letters to survivors of the Civil War, asking them if they could recall rain after battles in which they had participated. Considering the sampling scheme he used, it is not surprising that Powers amassed a large body of "data" which he used to advance a proposal for a government experiment. Powers' experiment called for 200 siege guns of various calibers, to be obtained from the Rock Island Arsenal, with which a simulated battle would be conducted, perhaps with two rows of siege guns facing each other. The implementation of this agricultural policy-to-end-agricultural-policies was hampered by the cost, which Powers estimated to be about $160,000 for two experiments, although he guessed that perfecting the method would bring the cost down to $20,867 per rainstorm, probably less than modern methods.

As is often the case, Congress did not fully fund the proposal, but in 1890 they did vote $9,000 for a modified plan, and the following year a party of ten was dispatched to Texas with thousands of pounds of explosives to be ignited over the ground, suspended by kites and balloons (*2*, *3*). The expedition was headed by a Washington attorney named Dyrenforth, about whom the astronomer-economist-statistician Simon Newcomb subsequently wrote (*3*):

> This gentleman had the indisputable qualification of absence of bias, being quite innocent of meteorology or of any other branch of science outside of his profession, and was therefore willing to take hold of the business seriously, instead of laughing at it, as all the scientists of the poor Secretary [of Agriculture] were suspected of doing. How good a man he was from this point of view is evinced by the fact that, he did not take a rain-gauge with him to measure it.

The expedition's head claimed success (*2*), although subsequent reports were in conflict with this (*3*). In any event, the rain of ridicule that followed the expedition seems to have dampened the voters' and Congress's enthusiasm for the project. Simon Newcomb suggested a cheaper variant of the experiment: "If [the most powerful bomb] can condense vapor a quarter of a mile away, then anybody can condense vapor in a room by slapping his hands. Let us therefore try slapping our hands, and see how long we must continue before a cloud begins to form" (*2*).

From Carter's account of the Colorado debate, it is clear that to the general public, today's weather modification techniques are as unreliable as were those of 1891, and there appears to be little scientific evidence to contradict this view.

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University-Industry Interaction

Rustum Roy (1 Dec. 1972, p. 955) considers several modes of university, government, and industrial interaction. His goal is the improvement of these interactions to fill an assumed national gap in the areas of "applied science" or "applied research." It is unfortunate that such an important subject is again treated in a superficial manner. One national problem may indeed be that of increased industrial productivity, and this "national" problem is certainly mirrored in specific industries where foreign competition, environmental considerations, and "consumerism" place such strong demands on productivity. However, the industrial sector is not as monolithic as agriculture, and cooperative schemes involving elite "troikas" of government (they have the money), universities (they have the brains), and industry (they have management and money) are just too simplistic to cover the diversity of U.S. industry. Consideration has to be given to the size of the company, the nature of the industry, its rate of change, and a variety of other economic, historical, technological, and sociological considerations.

To many of us in electronics, the problem is not that of a more effective use of the universities, but rather that of transferring technology, products, and insights to manufacturing divisions which are necessarily concerned with short-time scales. The problem is to understand both technological trends and business trends so that the necessarily longer-range view of research can be properly phased with the planning and performance of a company. It is not an easy task and requires intimate knowledge of both the company and its industry. That kind of insight does not arise from any of the models described by Roy. Indeed, I can only see Roy's models of use in two ways. The first is simply manufacturing "fire fighting," where a simple idea or experiment can often solve a serious problem. The specific industry then benefits from the intellectual capital of the university. Another way is to supply an "intellectual front" for industries that lack the understanding, conviction, and funds to adequately support their own industrial research. I fail to see either the educational or industrial returns from such roles for a university.

Finally, I am not aware of any "shutdown" at RCA that could be associated with its "withdrawing, from fundamental research, even from research applied to its own problems, the support it had been giving for two decades." At the end of 1972, RCA left the main frame computer business, and the whole corporation reacted to this large loss. The reduction of research (less than 10 percent) at RCA Laboratories can be completely related to a specific business problem and not to major changes in philosophy.

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We wish to protest vigorously the comment in Roy's article concerning the research activities of the Ford Motor Company. We cannot speak for the other industrial organizations cited, but we wish to note for the record that research at the Ford Motor Company has not been shut down; in fact it is increasing. While the level of nonoriented fundamental research support has been decreased somewhat since the mid-1960's, such research still occupies a respectable and legitimate fraction of our overall research program. Numerous business pressures, resulting in part from governmental regulations for environmental quality, safety, crashworthiness, and so forth, have required reorientation of some of our research activities. Within these problem-oriented areas our fundamental research has expanded and plays a very significant role.

We agree with Roy's thesis that university-industry interaction needs to be enhanced, consistent with the roles, missions, and objectives of academic and industrial research organizations. We ourselves are strongly committed to developing and implementing new modes and patterns of effective interactions between our own staff and our academic colleagues.

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I apologize to Cody and Compton if they felt that my term "shutdowns" was too sweeping in describing the changes at Ford and RCA. There seems, in any case, to be little disagreement that there has been a marked shift (reorientation?) in the distribution of research support within U.S. industry, away from the more fundamental end of the spectrum.

While Compton notes that Ford is committed to developing new modes of interaction with academe, Cody's rather hopeless view of coupling is more representative of the reaction I find among large, high-technology companies, who (regrettably, in my view) have so far influenced national science policies not only for themselves (where they may be applicable), but also for companies with low and middle technology and for small companies. The tragedy of it all is not that such coupling has failed, but that it has never been seriously tried by any industry (and I would sincerely appreciate hearing from any reader who can supply data on exceptions). It will be exceedingly valuable if Ford's new models of university-industry coupling were described in the literature, and if more industrial research managers would lay out their rationale for using, or avoiding, such coupling.

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