letters

Problem exposed by solar-neutrino work

The 1983 discovery of the intermediate vector bosons of the weak interaction at CERN was a moment of great satisfaction to physicists everywhere. It marked the end of an era that began 50 years earlier, and firmly established the unification of the weak and electromagnetic forces.

For many elementary-particle physicists in the US, that satisfaction was tinged with misgivings about the health of the American program in experimental particle physics. The CERN success, coming as it did after the ideas that led to it had been rejected in the US, and coupled in time with other disappointments in high-energy physics in the US, appeared to signal a serious decline of the American national effort in experimental particle physics.

To evaluate the situation, and to consider a new, very high-energy facility as a possible remedy, a subpanel of the High Energy Physics Advisory Panel of the Department of Energy was formed in 1983. While the creation of the subpanel was in part occasioned by European success, much of the testimony presented to the subpanel did not especially fault the US program. It was thought that the $p\bar{p}$ collider at CERN and the discoveries made with it were the result of the insight and determined action of a few gifted individuals—a not uncommon feature of discoveries in science—and were not to be construed as indicating a difference in vitality between the European and American programs.

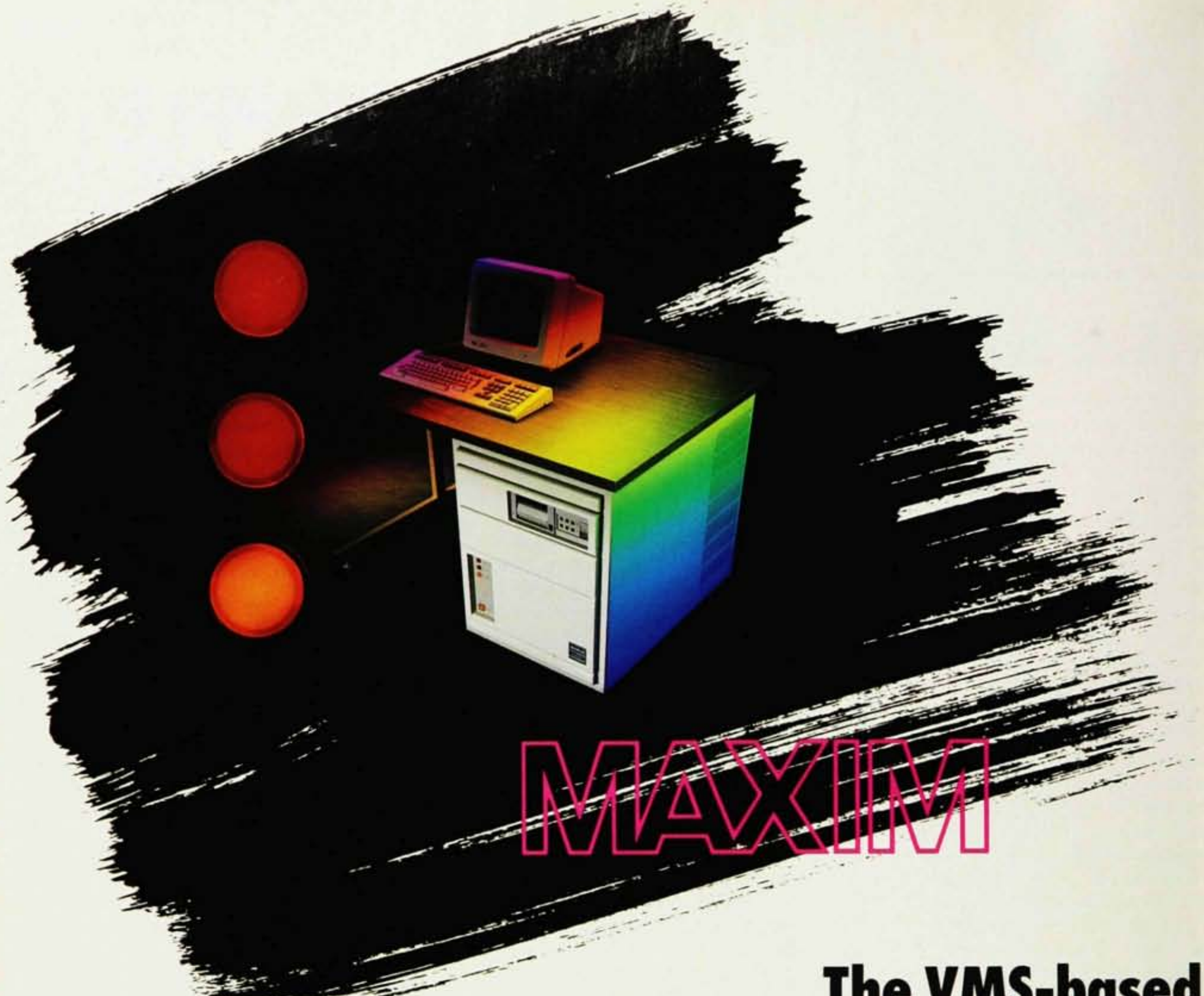
There has, however, emerged an event that again calls into question the vitality of the present American scientific enterprise. It has arisen in an area peripheral to, but not divorced from, high-energy accelerator physics. In 1981 a proposal to construct a multi-ton gallium detector to measure the flux of neutrinos from the primary fusion reaction in the Sun, $pp \rightarrow De\nu_e$, was reviewed by a panel formed by the DOE Office of Energy Research. The panel recommended in the strongest terms that the proposal be funded despite its need for many tons of gallium, expected to cost between \$15 and \$25 million. The stimulus for the experiment was a discrepancy of a factor of three between the result of the first and only solar-neutrino experiment and the prediction of the standard solar model. It was, of course, recognized that either the experimental result or the semi-empirical calculation based on the standard solar model might be in error. But it was also recognized that the discrepancy, if real, might require significant modification of either the solar model or the understanding of the intrinsic properties of neutrinos—the former possibility clearly of importance in astrophysics, and the latter clearly meaningful in particle physics. [Recent work has increased the interest in this problem; see the news story on page 17.]

The recommendation of the 1981 panel received public approbation from the astrophysics, nuclear-physics and particle-physics communities, but it was never acted upon and funds were never allocated for the experiment.

In early 1985 an essentially similar proposal, based on the experience gained from a prototype gallium detector in the US, was submitted to DOE by several American institutions. This proposal was reviewed by an *ad hoc* panel (mandated jointly by DOE and the National Science Foundation) of the NSF Nuclear Science Advisory Committee. The panel approved the proposal and recommended that it be funded.

In the meantime, slow but real progress has been made toward a 50-ton gallium detector, to be ready perhaps in 1988, at Baksan, the major facility for solar- and atmospheric-neutrino physics in the USSR. Of equal significance, a proposal to construct a multi-ton gallium detector, Gallex, was made to the underground physics laboratory at Gran Sasso, Italy, by a group of French, German and Italian scientists, some of whom had been involved in the US proposal reviewed in 1981. The Gallex proposal has been approved and the respective governments are providing funds to permit operation of the detector by about 1990.

At the moment DOE is still unwilling to commit funds for an American



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gallium experiment, and it appears that American participation in the experiments abroad, if it were to come about, would constitute the total extent of the US program to study the fundamental solar fusion reaction and the implications of the solar-neutrino problem.

It may of course be argued that science is international, and therefore it is not of great moment where a given experiment is done. Nevertheless it is difficult to view the present status of gallium experiments in the world without questioning the behavior of the US science establishment. The situation might be understood if there had been a division of opinion on the potential significance of the experiment or on its feasibility—or even on its cost. But the reports of the review panels do not reflect divided opinion: They have been without reservation in favor of the gallium experiment as proposed in the US, in part because the experiment would continue work in an area pioneered and developed by American scientists. How then does one explain the failure to act on the positive recommendations of those panels?

The answer to that question appears to consist of several parts, which have, not entirely coincidentally, reinforced one another. First, as the experimental facilities in all areas of physics and astronomy have become more expensive and more time consuming to build and maintain, the single-minded concentration by scientists and funding agencies needed to ensure the success of the projects for which they feel responsible has increased proportionally. In that atmosphere, an experiment that is tangential to the primary interest of those busy individuals is likely to be treated in a kindly but distracted way, as a child often is when one is deep in concentration. Second, it follows that in the absence of a large number of scientists who propose to work on a given experiment and know themselves to be necessary to its success, vocal support for such experiments by the community tends to be muted and a sense of urgency tends to be lacking in administrators. Third, the ratio of the cost of an experiment to the number of scientists employed by it is high for experiments such as the gallium experiment, and perhaps thought too high by administrators. Finally, the funding of an experiment that bridges different areas of science, but is apart from the momentary mainstreams of those areas, raises the fear of dilution of already scarce resources if it is funded, unless it is minimal in cost. It is natural for each area to give approval and at the same time exhibit reluctance to bear the cost.

What all this adds up to is that the gallium experiment is another example of the inability of American science, as it is now organized, to react flexibly and to prosecute aggressively an idea for an experiment of great potential interest, despite the manifest merit of the experiment.

The fate of the gallium experiment, which is important in its own right, raises a significant concern for the future: namely, that venturesome scientific ideas that lack a large supportive constituency, that cross disciplines and that are apart from the mission-oriented mainstreams of those disciplines will find it increasingly difficult to obtain funds, whatever their merit.

To forestall such a development American scientists must strengthen their resolve that ideas of the highest quality are to be realized no matter what their disciplines or constituencies. And US science administrators must be willing to provide less compartmentalized, more enlightened care of future proposals than they have given the ill-treated proposals for the gallium experiment.

ALFRED K. MANN
University of Pennsylvania
Philadelphia, Pennsylvania
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Microphysical reality

Quantum mechanics is already 60 years old, but still a lot of discussion is going on about the real meaning of the quantum formalism. This was evident during the conference on microphysical reality and the quantum formalism held in Urbino, Italy, last fall. The conference was organized to celebrate the 50th anniversary of the famous Einstein–Podolsky–Rosen paper.[1]

Recent experiments done to see whether Bell's inequalities are violated in reality answered[2] in the affirmative: The inequalities are experimentally violated (see PHYSICS TODAY, April 1985, page 38). Now the hard question arises: Is Einstein locality violated? Some physicists claim that because no signals can be transferred with the apparatus used in these experiments it is proper to talk not about "action faster than light," but rather about "influence faster than light." Others do not believe in the interpretation of the crucial experiments and claim that some of the additional hypotheses necessary to interpret the experiments are wrong. The question of locality was the hottest issue of the conference. Only one thing was clear from the discussions that took place: It is difficult to find agreement even on minor issues. Therefore we thought that it would be a good idea to organize a poll on quantum mechanics using questions that would be answered "yes" or "no" only. We ob-