

# letters

## Coverage of SURA

Your news item on the SURA accelerator project in the September 1984 issue of *PHYSICS TODAY* (page 55) raises important issues, but in what appears to me to be a rather distorted perspective. The scientific need for a cw electron accelerator has been felt by nuclear physicists for over a decade. When the first NSAC Long Range Plan was prepared in 1979 under the chairmanship of Herman Feshbach, such an accelerator was identified as the top priority major construction project for our science for the 1980s.

The physics to be addressed by a continuous beam electron accelerator lies at the heart of some of the most interesting current questions in nuclear physics. The electromagnetic probes have important advantages: the nucleus is relatively transparent to them and the basic processes are relatively well understood. The recent feasibility of accelerators with continuous (cw) beams means that coincidence measurements become possible and the range of questions that become accessible to experimental investigation increases manifold. Particularly exciting to nuclear physicists is the prospect for exploring the implications of QCD in nuclei with cw electron beams in the GeV regime.

The competition for a new facility in early 1983 was certainly strong, with three major contenders: Argonne (my own institution), MIT and SURA. An NSAC panel chaired by Allan Bromley recommended the SURA proposal and this recommendation was endorsed and transmitted by NSAC in early 1983. Inevitably some were disappointed by the choice, but once Secretary Model accepted the recommendation, most of us felt that we should get on with the business of constructing the accelerator and with the physics.

Later in 1983, at the request of the funding agencies, the second Long Range Plan was prepared by NSAC under my chairmanship and we were asked to look at the future of our field beyond the electron accelerator. Nevertheless, much of the physics discussion in that report concerns itself with

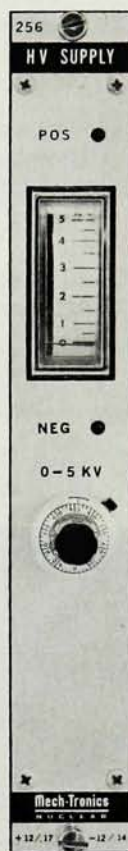
the experimental questions that are to be addressed by the new machine. To be sure, we discussed the next major facility that we would like to see: a relativistic heavy ion collider that holds considerable promise to explore a new regime of many-body hadronic matter. But nobody suggested in the discussion of the Long Range Plan that this collider should replace the electron accelerator. *PHYSICS TODAY* presents an apparent conflict between the two accelerators that simply was not there. The need and the priority for an electron accelerator was taken for granted by all the participants in the discussions that led to the preparation of that report. The author of the *PHYSICS TODAY* article remarks that the Long Range Plan "often gives the curious impression that the committee places more emphasis on the relativistic heavy ion collider," and proceeds to analyze the use of italics and bold print in that report. I suppose that the writer of that article is entitled to such observations (even though I had assured him several times that his interpretation and search for hidden meanings was incorrect).

Unfortunately there have been delays in getting started with the SURA project, most recently the first appropriation for construction was postponed in the Congress. An NSAC subcommittee was appointed to reexamine the scientific justification for a 4-GeV cw electron accelerator, and at a recent meeting of NSAC they submitted their report, which reaffirms the high priority for such an accelerator.

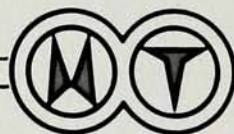
Politics unquestionably does get involved in the siting and funding of such a major research facility, but this should not allow us to lose sight of its justification in terms of new scientific knowledge and understanding. What concerns me is the relish with which *PHYSICS TODAY* dishes out the juicy gossip about the difficulties of a new undertaking that represents the scientific hopes of many of us. There is a self-fulfilling aspect to such reporting that should be taken into account in editorial policy, especially for a journal that represents the physics profession. The writer is not a detached observer, and by the nature of his reporting can

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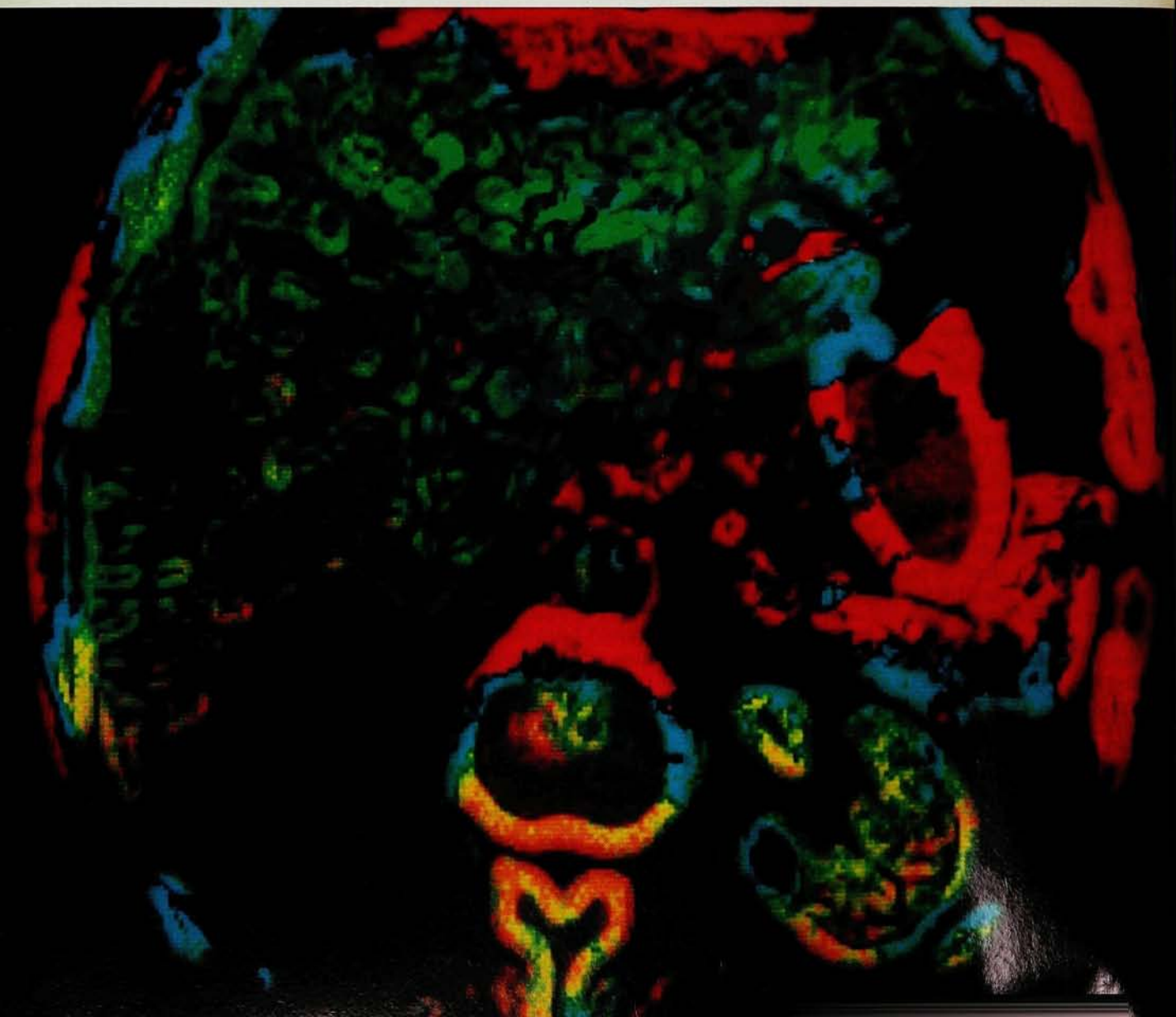
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## letters

influence the course of events. While the average reader may be amused—I am afraid that science will suffer. News should be reported fully and problems not be swept under the rug, but there is a line between reporting facts and inserting editorial opinion; in this case I feel that the line has been crossed.

JOHN P. SCHIFFER

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## Estimating SURA's cost

The article on the SURA accelerator in September (page 55) states that the project "lacks a definite design and detailed cost estimate." As our company prepared the estimate for the accelerator, I would like to respond to this criticism.

We have prepared estimates for the first SLAC and Fermilab accelerators as well as the LAMPF linear accelerator on which approvals of these projects were based. In my opinion, the SURA design was at least as complete and more certain to operate essentially as proposed than any of the abovementioned projects.

Although the design output requires improved klystrons, such expectations are typical of accelerator project proposals. If the required improvements cannot be made, a larger number of lower-power tubes could be used or a lower but still satisfactory current output accepted.

The overall cost of large accelerator laboratories is typically two to three times the cost of the accelerator. These additional costs depend, to a considerable extent, on the amenities provided and the scale of the administrative organization to be accommodated. They also depend on the cost of the equipment required to conduct the experiments and how much of that cost is included in the cost of the project. These items may require further definition, but I see no reason for re-estimating the cost of the accelerator itself as long as the existing design is not significantly changed.

WILLIAM M. BROBECK  
Brobeck Corporation

10/84

## Molecular rotation spectra

The "Search and discovery" section of PHYSICS TODAY should be the place to find carefully chosen, scrupulously edited, and appropriately referenced accounts of the most important recent developments in physics. It is read by nonexperts in the field under report, and it is presumed to be objective.

"High-spin molecular rotation spectra are surprisingly simple" (July, page 17), which purports to be an account of the enormous theoretical progress that has been made toward the understanding of the vibration-rotation spectra of polyatomic molecules in the past few years, fails on all these counts.

Over the past 20 years, important advances have been made in the understanding of the vibration-rotation spectra of polyatomic molecules. Among the most important general advances has been work on molecular symmetry by Jon T. Hougen and H. C. Longuet-Higgins, and the simplification of the Hamiltonian and the discovery of the correct way to treat centrifugal distortions when analyzing spectra, both by J. K. G. Watson. None of this has been reported in "Search and discovery." The clustering of the rotational energy levels of spherical top molecules at high angular momenta (the sole topic of the July 1984 article) is an interesting, albeit rather specialized, development for which William Harter and Chris Patterson can take some credit. However, all the credit for its discovery and classical interpretation must go to A. J. Dorney and Watson.<sup>1</sup> This reference is not given in the article and only a passing mention (with incorrect initials for Watson) is made to it. Also, much is made of ortho-para interactions ("the violation of a hitherto sacrosanct selection rule") as if this were a new theoretical development; this is untrue and has been well understood for a great many years. It was first observed<sup>2</sup> by Irving Ozier in 1971.

It is only natural to be enthusiastic about one's research, and it can easily happen that one has an inflated view of its importance. Harter and Patterson can certainly be forgiven for presenting their research in an overly enthusiastic manner bordering on Madison Avenue, but the editors of PHYSICS TODAY must take a more balanced view.

## References

1. A. J. Dorney, J. K. G. Watson, *J. Mol. Spectrosc.* **42**, 135 (1972).
2. I. Ozier, *Phys. Rev. Lett.* **27**, 1329 (1971).

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WILLIAM G. HARTER COMMENTS: Bertram Schwarzschild, the author of the July "Search and discovery" article, did solicit and use comments from a number of experts in the field of molecular spectroscopy, including Hougen. The resulting article was judged to be a fairly balanced account of retical developments that help in visualizing complex dynamics and spectra that arise from solving various molecular Hamiltonians. It was not intended to be an exhaustive review of the

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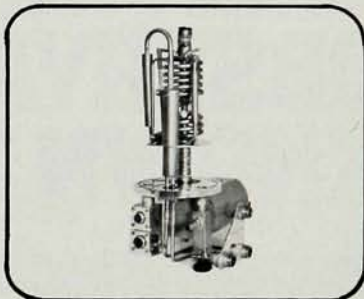
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