LETTERS

Stanford's stand on Themis

In the interview with Chalmers W. Sherwin, which appeared in the September issue of PHYSICS TODAY, there are some serious misstatements. Possibly they are misquotations, but they should be corrected.

In particular, in a discussion of the Themis program, there is mention of "beautiful general-relativity-gyroscope-precession experiments conducted at Stanford." Themis does not support this work. This important experiment was first proposed by Leonard I. Schiff in 1960 in the course of his theoretical-physics research supported by the Air Force Office of Scientific Research. The experiment was then undertaken by William M. Fairbank, with support initially from AFOSR, and then from NASA and from the Air Force, Wright Field. This support is provided jointly with programs in the aeronautics and astronautics department. Thus pure physics and engineering are financed together, and in this respect only there is a resemblance to Themis.

However the Themis program is specifically designed so that universities like Stanford, which have already demonstrated excellence in research, are not eligible. It was originally intended that Themis would not interfere with project research chosen for its scientific or engineering importance. In fact, Themis is being expanded at a time when the overall research budgets are stationary or being reduced. Implementation of Themis in this way is bringing about a drastic reduction of university research programs and an almost complete stop to new directions in fundamental research.

Meanwhile, fortunately, some fundamental research projects such as the superconducting orbiting gyroscope are continuing. To accomplish the experiment on general relativity Stanford physicists must make, and are making, really major advances in technology. A precision of 0.01 sec of arc per year is expected. This sort of precision certainly would be useful in long space journeys or in satellites placed for long periods on station. Incidental technological achievements permit maintaining other cryogenic devices for long periods in space. Methods for producing and measuring extremely small magnetic fields are also greatly advanced. In turn, the possibility of zero-field regions prompted Stanford physicists to begin research on free-precession helium 3 gyros that may give a precise gyro usable on earth.

Who else needs such precision and is willing to go to so much trouble for it? Probably nobody else now. But what becomes possible will eventually be practical, and from a basic-research program will arise a whole new realm of technology. Themis will do well if it can match it.

ARTHUR L. SCHAWLOW
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Free or mission research?

The interview with Chalmers Sherwin contains statements that greatly disturb me. I realize that statements in such an interview cannot be elaborated, and that if they were so qualified with detailed explanations and examples they might convey an impression opposite to that which one first experiences. My comments on this interview pertain to my first impression and hence are subject to change through better understanding.

The following statements particularly disturbed me: (1) ". . . the real strategy for research should be one of coupling long-range scientific work to short-range practical results . . ." (2) "Hindsight showed a broad lack of coupling between basic and applied research during the previous 20 years, which the country will no longer permit." (3) "If you want research funds, couple your request to an understandable need and justify the relevant long-range work by short-range practical results." (4) "In the future we must do only those experiments that are crucial to theoretical issues. Some scientists seem to want to do research only if it is useless."

If the above criteria had been applied by each investigator to determine whether he should start or continue his own research project, our greatest advances in understanding and mastering nature would still be in the hands of fate. How could a man like Paul Herget, who in the mid-thirties had a passion for calculating the orbits of asteroids and minor planets, justify his work on any of the above grounds?

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Had he not simply been in love with the idea and allowed to pursue it, then our government would not have had an expert in the Sputnik crisis.

Does this approach deny the value of mission-oriented research? Of course not. Albert B. Sabin and his polio vaccine are a case in point. What is very dangerous is the word "only" in statement 4 above. The choice of the word "crucial" is also unfortunate: Pierre Duhem[1] proves a "crucial experiment" is impossible.

Universities throughout the world have the duty of supporting creative people in the job of pure thinking. The chairman of each department has the responsibility of finding creative people and giving them free reign and encouragement. Academic vice presidents and deans as well as government administrators are too far removed from the academic world to be entrusted with this duty. Our citizens, through the government, have an obligation to support universities at the financial level; the government should simply act as a fund-gathering and disbursing agency. This agency must trust the judgement of university presidents, and they in turn must trust the judgement of their department heads, who in turn must trust the ultimate pursuer of truth—the individual researcher. The former are servants of the latter. The government may set up its own mission-oriented research projects wherever it pleases as long as this does not interfere with the collection, transmission and augmentation of truth by universities.

Sometimes we have to look to nonscientists for the most impartial analysis of the role of science and government. I ask the reader to compare the following quotation from a noted statesman, philosopher and former president of the UN General Assembly with Sherwin's statements. The difference in philosophies may be a matter of national survival!

> Never before did the survival of whole civilizations depend upon science, theory, thought, trust in reason, the inventiveness of the human mind, the effective organization of whole armies of researchers and technicians, as it does today. The cultures and civilizations that seek science and knowledge for their own sake are alone likely to survive; and those who only seek to use nature without loving her, namely, without cultivating theory for its own sake, have simply no chance, whether in war or in peace. If you want to find out how much a nation or a culture has a chance of surviving, ascertain how much effort it is dedicating to pure theoretical research. This is the most authentic index of survival. And by "survival" is not meant the sheer continuance of bare existence, but an existence human, vigorous, independent, creative, universally recognized and respected, one that has something original to give to the rest of the world.
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> . . . If you want to be independent, then please pay the price of being Hellenized, scientificized and theorized. And when this price is specified, many a culture will balk at paying it, for it includes the virtual abandonment of most of its cherished ideas and dreams.[2]

Sherwin's interview concludes with the remark that we are so rich that ". . . we will be able to afford a whole lot of elegant, complicated and exciting intellectual activities that we never had thought we could afford before. And I think science will share in this realization." I only hope that he is using "science" in the same sense as the quotation above.

References

1. Pierre Duhem, *The Aim and Structure of Physical Theory*, Atheneum, New York (1962), p. 188.
2. Charles Malik in *Philosophy, Religion, and The Coming World Civilization*, Leroy S. Rouner, ed., Martinus Nijhoff, The Hague (1966).

JOHN B. HART
Florida State University

SHERWIN REPLIES: Arthur Schawlow is correct in stating that the relativity gyro experiment is not funded under the Themis program, as is implied in the interview. The phrase "conducted at Stanford University under the program" should read, "conducted at Stanford University under program-type support." The purpose of both this example and the following one from Lincoln Laboratory is to illustrate that with good local management and enlightened federal administration both important basic science and good applied science and engineering can be carried on together within a mission-oriented or mission-related program.

Schawlow also expresses his concern that the creation of the Themis program had the effect of reducing

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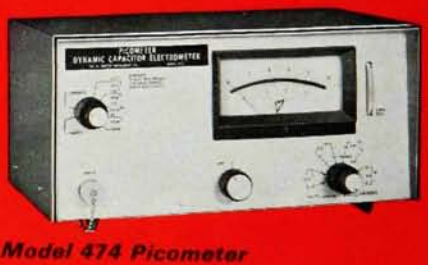

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existing university-research support. This may be true, but to what degree one cannot say unless one knew what the level of support would have been this year (that is, at the time of final congressional approval) *without* the existence of Themis. The program was, of course, planned as an "add-on." Its objective was to start building new centers of excellence while still maintaining the existing ones, and, under a happier international situation, it should have worked.

I agree with John Hart that there is a need for both free research and mission-oriented research. We disagree on the optimum strategy to obtain a suitable level of support for the free or undirected research. I suggest that as a practical matter as much long-range research as possible be justified by its relevance to mission research, and be funded and managed integrally with it. Hart states that the government has an obligation to provide the universities with funds to carry on a suitable level of free research without government direction or without regard for the government's mission objectives. I think it unrealistic to assume that the citizens, through the government, will provide under these conditions the level of support that the research scientists need. Thus I recommend that Hart's strategy be used primarily for areas of research—such as high-energy physics, for example—where relevance to current government missions is least apparent.

Malik's assertion that the amount of effort devoted by a culture to research is the most authentic index to survival, whether in war or peace, strikes me as being a half truth. On the 50-year time scale, an inadequate amount of pure research may well endanger the survival of a culture. On the 5-year time scale, however, an inadequate amount of applied research and development may lead to disaster. In short, the issue is one of determining the optimum balance between two essential, related functions that operate on two different time scales. The problem facing the scientists is to analyze quantitatively the two functions, seek a rational method of determining relative effort between them, and develop means of improving the efficiency of the overall process.

CHALMERS W. SHERWIN
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