

An 'SSC' for condensed matter

I agree with Herman Feshbach (June, page 7) that attacks on "big science" do physics no good. As a community we have both intellectual and practical reasons for enjoying successes in branches of physics other than our own. The problem is that when budgets are "at the threshold of pain," as the May PHYSICS TODAY (page 55) put it, it is just not good enough to say, as Feshbach does, that physics funding "is no zero-sum game."

We are all aware that particle physics costs a lot, but for exactly the reasons put forward by Feshbach, most physicists would support a budget that reflected these extra costs to a reasonable extent. However, it must be recognized that the condensed matter community plays the major role in training physicists and extending research opportunities both geographically and across the spectrum of institutions. Condensed matter physics produces twice the PhDs at about a third the total cost of particle physics. Arguing about the relative importance of different fields is futile, I agree. But the point needs to be made that most students choose condensed matter physics because they feel it gives them more interesting career opportunities. Of course none of these points (as Feshbach argues) justify attacks on the SSC. If all were well with condensed matter, we should indeed devote our time to supporting our colleagues in pleading for the SSC. However, all is not well, and there is an urgent need for us to develop proposals for our own condensed matter "SSC." In our case this is no one machine, for the essential backbone of condensed matter research is the small group—it leads to flexibility and diversity and provides a stimulating internal competition. This diversity is now threatened by the crisis in funding:

- ▶ Many excellent proposals are going unfunded because of budget shortfalls.
- ▶ The current difficult climate encourages a rather conservative approach by referees. The result is that controversial (often the most interesting) and

interdisciplinary proposals suffer unduly.

- ▶ Limited funding often results in support only of what is judged to be the "best" group in a given area, with consequent suppression of competition.

- ▶ There is a legitimate need for more flexible small grants, such as those given by the Research Corporation. That body does a very valuable job in getting young researchers established. However, there is also a need to keep well-established researchers going. The more senior investigator, capable of excellent research and graduate teaching, but not recognized as one of the major stars in his or her field, has nowhere to turn at present.

I believe the current state of condensed matter funding should be examined carefully by the APS division of condensed matter physics with a view to developing proposals for a national system of small grants in condensed matter research. Such a scheme would be our SSC.

STUART LINDSAY

Arizona State University
Tempe, Arizona

6/86

FESHBACH REPLIES: In reading Stuart Lindsay's letter one would imagine that I had written a Reference Frame column in defense of big science. Far from it—the column in question approved of both big science and small science in the sense that each area of endeavor should determine the facilities it needs according to the priorities attached to the scientific issues. Condensed matter physicists should evaluate their need for big science facilities such as the synchrotron light source, high-flux reactors, spallation neutron sources and so on, as well as for "one-room tabletop experiments," in terms of what important scientific questions they will address effectively.

My remark regarding the zero-sum game for funding is based on more than two decades of concern with budgets in which I have interacted with Presidential science advisers, the Office of Management and Budget, and Con-

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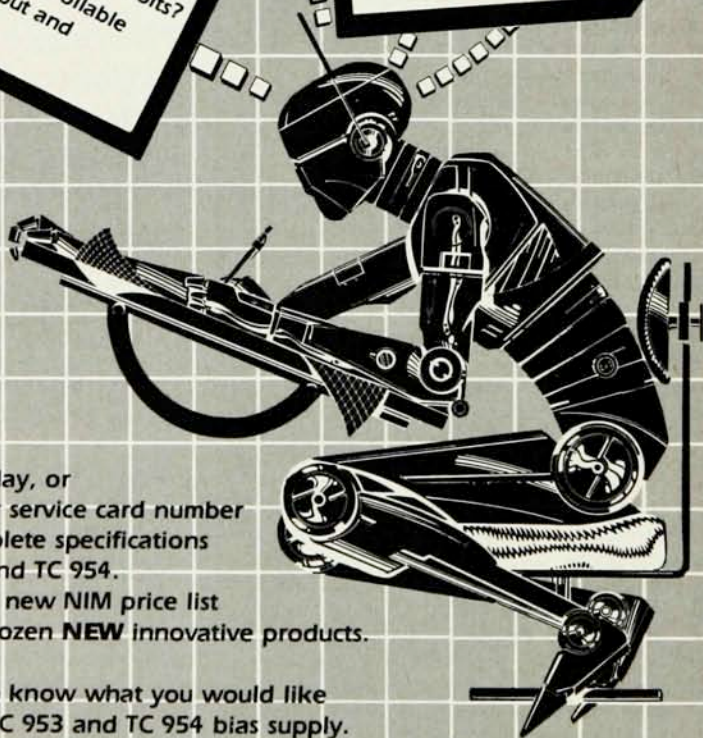
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letters

gress. I found nearly no evidence that funding in one field limits funding in another. The notion that reducing one field's funds will increase another's is empirically just wrong. The point is that there are many other parameters that enter into the determination of a final Federal budget, and that in my opinion have a much greater weight than the total funds assigned to basic research.

I am sorry to learn that "many excellent proposals" in condensed matter physics are going unfunded. But let me assure Lindsay that that is the case for each and every field of physics. Obviously it is much more productive if we work together than if we indulge in internecine squabbles.

HERMAN FESHBACH

Massachusetts Institute of Technology

1/87

Cambridge, Massachusetts

The SSCene Creed

The physics community may stand or kneel.

I believe in the Copenhagen interpretation of quantum mechanics, the second law of thermodynamics and the unitarity of the S matrix.

I acknowledge one vacuum for the basis of Hilbert space, the inattainability of absolute zero and the nonobservability of phase.

I am willing to concede second quantization of fields, the wave-particle duality and the path integral formulation of quantum mechanics.

I am reasonably comfortable with canonical quantization, the manipulation of divergent quantities as though they were infinitesimals, and the confinement of quarks.

I am willing to speculate on the possibility of supersymmetry above present-day collider energies, the collapse of the wave packet upon measurement, and the "true" number of dimensions of the universe.

In my less lucid moments I will even buy supergravity and the introduction of local SUSY transformations on a manifold.

After a few martinis I will slur, "Hell yes!" to the suggestion that the underlying structure of the universe is a two-dimensional conformally invariant field theory.

After a prefrontal lobotomy I will declare fervently that a unified field theory encompassing all known particles and interactions is inevitable before the end of this century.

In the name of quantum mechanics, the Dirac equation and the compactifica-

tion into itty-bitty circles of everything we don't observe, Amen.

SANFORD WILSON

ERNEST LEWIS

6/86

University of Texas at Austin

How to slice a research pie

Billions of dollars are targeted for enormous particle accelerators (and defense research) while many areas of physical science must subsist essentially unsupported even if they are of easily demonstrable technological or societal importance. Criminalistics, the scientific examination of articles of physical evidence recovered from crime scenes, is just one of scores of examples. The US spends considerable sums on crime prevention, rehabilitation, law enforcement organizational efficiency and the corrections system. That this country nonetheless continues to have far and away the worst crime rate of the industrial nations tells of the effectiveness of these measures. A key role in maintenance of a reasonable level of social order is therefore played by the ability to solve crimes, in particular by criminalistics as a scientific tool of criminal investigation. Nothing, however, is spent by the Federal granting agencies in support of physical science research applied to criminalistics. I suspect that the taxpayer would be less than delighted to find out that billions are dedicated to charm, color, strangeness, truth and beauty, while the jaded palates of research support strategists and program managers are not at all titillated by the facts that about 1 in 130 present US inhabitants will die by murder and that annually nearly a third of US households are victimized in some way by crime. Do the first instants of the Big Bang really merit that vastly greater support than does the safety of the country's citizens?

Unfortunately, many research programs that are innovative and have impact, but are unorthodox, fall victim to the passing of the buck because they do not neatly fall into a pet research support area. Worse, such programs apparently must be tainted with vulgar descriptors such as "useful" and "applied," if the intellectual snobbery that greets them is any guide at all. Curiously, such snobbery and the tendency to justify it by invoking "basic science" or "fundamental understanding" is all too often favored by those whose own work languishes in well-deserved obscurity, producing nothing but utterly inconsequential publications to clutter the literature.

No doubt basic research, even if expensive, has to be supported. How-

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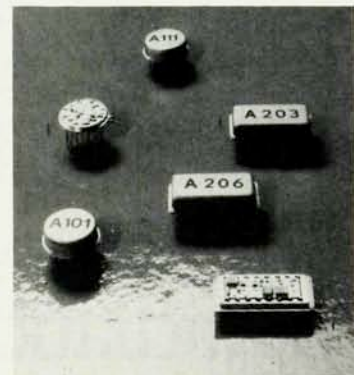
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