

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

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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

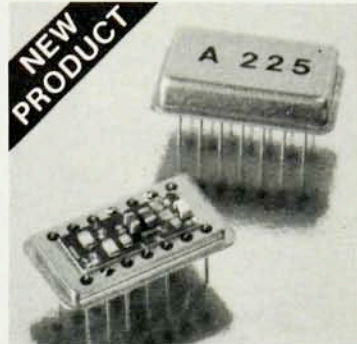
Billions of dollars are targeted for enormous particle accelerators (and defense research) while many areas of physical science must subsist essentially unsupported even if they are of easily demonstrable technological or societal importance. Criminalistics, the scientific examination of articles of 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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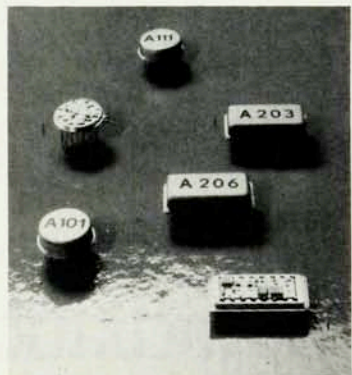
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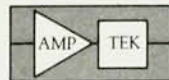
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HARSHAW/FILTROL

The Specialists

letters

ever, the crass funding imbalance that characterizes the present research support strategy cannot help but aggravate the already precarious technological stature of the US. Applied research, particularly when it targets nontechnocrat end users, has been flagrantly neglected. In time, perhaps such research may share in a crumb of the national research pie.

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5/86

Superstrings: Another view

In responding to Paul Ginsparg and Sheldon Glashow's column "Desperately seeking superstrings?" (May, page 7) let us start with the things one must agree with: Standard physics is not incorporated in superstring theory. The theory does not offer a solution to any known puzzle in particle physics, such as the generation puzzle, the CP problem or the axion problem, nor does it give any clue to the various parameters of the standard model. It is hoped in superstring theory that if we know the correct six-dimensional compact manifold with all its warts and holes, the theory will determine all the masses of the quarks and leptons, and all the coupling constants. This is a tall order. The discovery of the correct manifold may require more sophisticated mathematics than most physicists know. The goal of superstring theory almost amounts to saying that if you put in all of mathematics, all of physics will come out.

So let us grant that superstrings have not done anything for particle physics (at least not so far). Hence the real motivation for superstrings at present must lie elsewhere. In fact it lies in quantum gravity.

Gravity must be incorporated into the rest of physics. It is intolerable to have one world where gravity is ignored and quantum mechanics reigns supreme and another world where gravity cannot be ignored and use of quantum mechanics to describe it leads to meaningless divergent results. To search for a consistent physical theory describing all known physical phenomena is surely a scientific requirement and not mere theology!

Even superstrings may not lead to a unique, ultimate theory. All this semi-theological talk about the "unique and ultimate theory" is nonsense. There may be many superstring theories, out of which only experiment and empirical knowledge may allow us to choose

one as the most promising. Already at least five consistent string theories are claimed to be available in the market, and there may well be more, even an infinite number of consistent theories.

Further, search for consistent theories of even more complicated objects than strings, for instance, membranes and lumps, must continue. Any reported "no go" theorem in this context need not be regarded as a permanent barrier. Remember, without supersymmetry and higher dimensions, even string theories would not work. So other things will be discovered that will make the theories of membranes, lumps and even objects extending to higher dimensions "go."

It is illogical to claim that extrapolation of known physics up to 10^{15} GeV (Grand Unified Theories) is science, but further extrapolation up to 10^{19} GeV is theology. Actually, coming up from 100 GeV, 10^{15} GeV is already so near the Planck mass of 10^{19} GeV that the only logical possibility is to work on physics, including gravity, up to 10^{19} GeV.

It should be admitted that such a preoccupation with superhigh energies in the range 10^{15} – 10^{19} GeV is bound to strain experimental physics very much. The preeminence of experiments in physics must be reestablished. So it is imperative that physicists and technologists put their minds together to solve this crucial problem of the energy barrier. After all, no law of nature forbids the attainment of such energies in the laboratory. Human ingenuity knows no bounds and a method will be found to reach the superhigh energies so that controlled laboratory experiments can be done to test superstring theories, or even theories beyond them.

At present, nobody has the wisdom to claim that superstrings are the only correct theory. With equal emphasis one may say that nobody can brand a theory as theology just because it does not offer any immediate experimental test. Nobody yet knows the golden path to truth and hence all avenues of scientific inquiry must be kept open.

Echoing Paul A. M. Dirac, one might declare that theorists must be free to invent consistent theories without bothering too much about their immediate experimental confrontation. (If Dirac had bothered too much about the possible experimental discovery or non-discovery of the positively charged particle predicted by his relativistic equation, we would not have got the Dirac equation.) Sooner or later, consistent theories do find their experimental application.

Superstring theory is welcome even
continued on page 108

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