letters

Super Collider

The recent article by Sheldon L. Glashow and Leon M. Lederman (March 1985, page 28) is an interesting addition to what appears to be an all-out effort by particle physicists to sell their new dream, the Superconducting Super Collider, to other scientists and to the US government.

An interesting argument—supposedly directed at their fellow scientists—is that in particle physics and cosmology the rules of the game are only partly known, whereas in the other sciences they are completely known. Proponents of these other sciences should ask themselves whether they in fact work by a set of completely known rules. Is there such a difference between the sciences? If this allegation of the authors is true, then one cannot expect any fundamentally new theories (especially concerning the nature of matter) to emerge from these other sciences. If such is the case then these other sciences should defer to the particle physicists and SSC.

To assist the government in making up its mind I should like to point out that the amount sought by the particle physicists is, after all, only \$3–6 billion. Compared with the current US Federal budget deficit or the Latin American debt this is a paltry sum. There should be comparatively little difficulty in paying it. One note of caution, though, is that 10 or 20 years from now particle physicists will want to build a 1000-km accelerator costing maybe \$60 billion. After all, the 3000-year-old debate on the nature of matter must continue. Future governments may take heart in that the Earth is roughly only 40 000 km in circumference, thereby setting an outer limit on the size of any accelerator. Maybe I should retract this last statement, since I can imagine particle physicists 100 years hence exhorting space explorers to find suitably larger planets in order to continue their debate.

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10/85

SHELDON L. GLASHOW REPLIES: Important new theories *do* emerge in other sciences: Cluster chemistry, superconductivity and molecular biology are examples. How truly fundamental are they? Do they not result from a complex interplay among many atoms, about which Heisenberg and his friends taught us all we need to know long ago?

Nuclear physics, like atomic physics before it, was once a "fundamentally new theory concerning the structure of matter." Not any longer. The proton is a composite of quarks held together by the color force.

As we probe more deeply into the structure of matter, obstacles appear beyond those intrinsic to our science. Accelerators become dreadfully expensive, while the practical value of our hard-won knowledge seems nil. Strange particles are almost 40 years old and are still entirely useless.

We have a correct, complete and consistent theory of elementary-particle phenomena, but this is precisely what bothers us. Our theory has too many adjustable parameters to be the last word. Unlike other scientists, we yearn for experimental data that will knock our theory down, so that our next one can be better yet. Who knows what surprises SSC will reveal? If we did we wouldn't need the machine. We don't quite know what we are doing nor where it will lead. That's what I mean by fundamental, and it's really the only honest argument we've got going for us.

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1/86

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The high-energy-physics community must be commended for its courage. Proposing the construction of a \$6-billion piece of scientific equipment at a time of \$200-billion government deficits and budget slashing left and right by Congress and the President certainly takes guts. The chances that the Superconducting Super Collider will actually get funded are about the same as that I will win the New York State lottery, and I haven't even bought a ticket.

Perhaps another strategy is called for. In the past scientific equipment was funded by private contributions, and thanks to the Reagan tax cuts, the wealthy have more funds to contribute. If the desire to advance basic human

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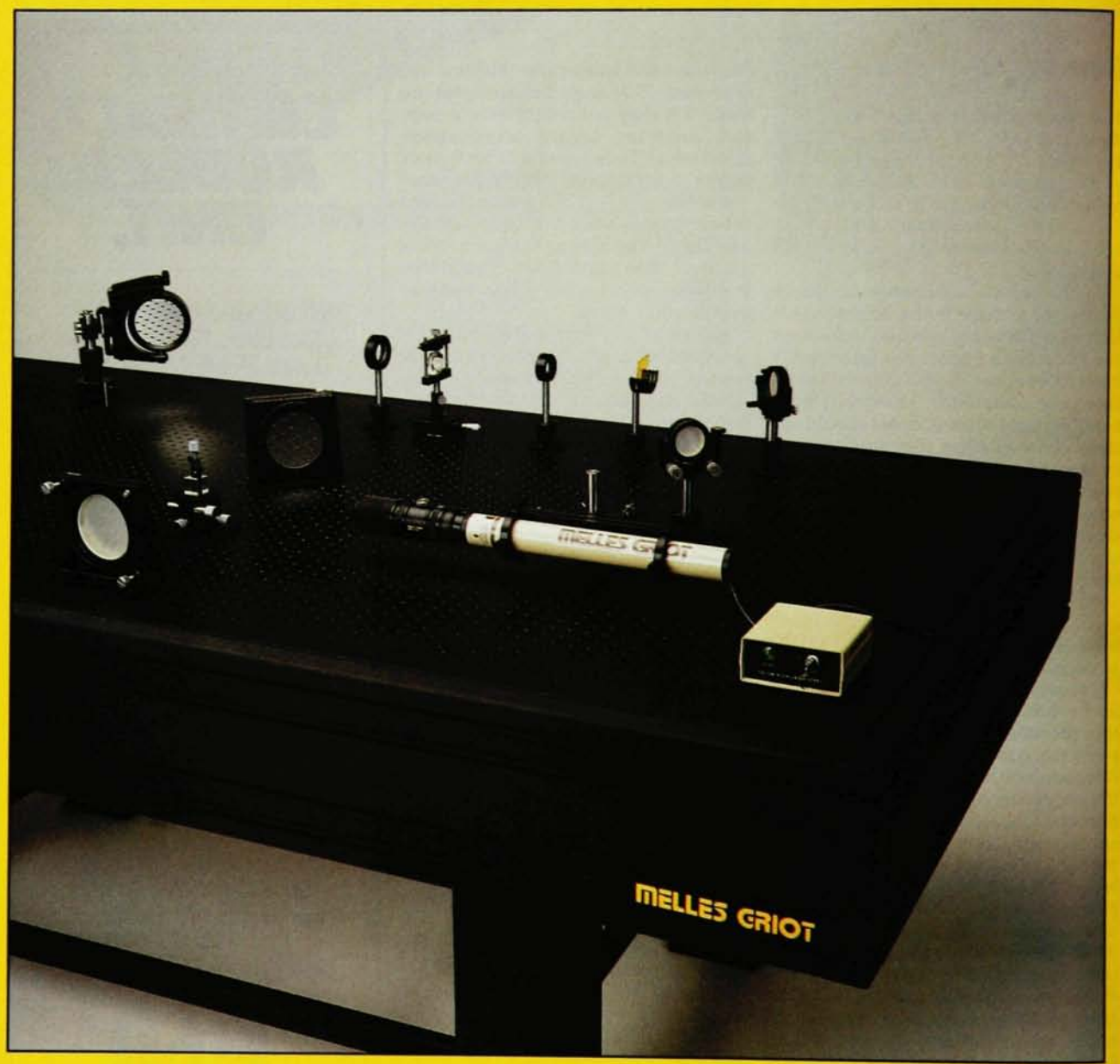

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knowledge is not enough, donors could be offered a touch of immortality in the form of brass plaques announcing, "this magnet contributed by Joe Jones."

If this fails, Federal funding could be obtained by another stratagem. Add another bending magnet directing the primary beam upward and claim that it could be used to destroy incoming enemy warheads. This idea would then be no more farfetched and no more expensive than some of the proposals currently under investigation for the "Star Wars" Strategic Defense Initiative.

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I read with interest Sheldon L. Glashow's and Leon M. Lederman's account of SSC and its significance, in the March issue of PHYSICS TODAY (page 28). For me, it is hard to understand how in modern times, in which all religious dogma is held to be but human invention that one is free to accept or reject, physicists can still believe that their equations are the ones that rule all causality in the cosmos. Must we still hold that our mind is at the center of the universe?

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Funding big science

I am writing to comment about the recent letter by Rustum Roy (September, page 9) titled "Funding big science." Even though some of his concerns have been addressed by others, I feel that a few additional comments are appropriate.

Roy argues that because big-science facilities have become so expensive, they are not worth their cost. He believes that all science should be "brought into a new balance" based on the criterion of eventual social utility. I would like first to take issue with the distinction between big and small science based on presumed funding-level differences. A close examination of science-funding needs demonstrates that further progress in all branches of science and technology depends on up-to-date instrumentation, which is becoming ever more expensive. In many respects we are all in the same boat.

The above point has been amply documented in the March 1985 issue of PHYSICS TODAY. The articles on major facilities show that not only high-energy physicists but also nuclear scientists, plasma physicists, biologists, chemists, materials scientists, medical scientists and many others rely in their research on ever larger and more expensive facilities. The article written by Daniel Kleppner (March, page 79) on behalf of "small-scale-research" practioners further supports this viewpoint. After enumerating some typical prices for instruments needed in such areas of research Kleppner concludes: "Setting up a typical small laboratory can cost anywhere from a few hundred thousand to over a million dollars." Regarding operating costs he writes, "an active university research group can require $200–$400 thousand per year; many groups need much more." Earlier he notes the critical shortage of instrumentation in university laboratories and quotes estimates of a "total need [of] over $1 billion." These are the sad facts of contemporary scientific research.

With regard to SSC, we note that while it is indeed a very expensive instrument, it is also the only major facility being requested by the high-energy community and its cost can be legitimately written off over a 10-year planning and construction period and 15–20 years of exploitation. Viewed in that context, it is not clear that there is a significantly disparity between the funds being requested by the high-energy community and those required by several other scientific disciplines.

But there is a more important issue, namely whether the support of a given science should be predicated on its ability to demonstrate contributions to "stable employment, ···prosperous new and old industries and··· a cleaner environment." I have several difficulties with such a criterion.

There would be serious consequences were we to abandon our quest for understanding the world around us independent of the immediate practical application that such knowledge might give us. For several millenia this intellectual curiosity and the ability to pass the acquired knowledge to future generations have distinguished mankind from other inhabitants of our planet. Are we going to abandon it now, at a time when we have made unprecedented progress in raising the level of well-being of all mankind? Are we now going to be interested solely in satisfying our material desires?

Even if we answer yes to those questions, a practical difficulty remains. What is the algorithm that will determine which research is most likely to further our technological know-how? What value do we put in the algorithm for the stimulation of young minds and inducing more students to go into science, mathematics or engineering? Ernest Rutherford, six years before the discovery of nuclear fission, declared, "Whoever says that nuclear energy cna be practically used speaks moonshine." And thus, I would argue,