

GLASHOW REPLIES: Enough of semantics! In essence, Philip Anderson and I are in complete agreement. Big sciences like the mapping of the human genome or the SSC project, however important in themselves, must not squeeze out equally important smaller sciences, whether "fundamental," "emergent" or whatnot. The destruction of once triumphant American space and planetary exploration by the manned shuttle program shows that this can happen. I am a staunch advocate of the SSC because it will answer basic questions that cannot otherwise be addressed, and its successes will inspire our youth to choose scientific careers. I am even more an advocate of increased and stable funding for the entire American scientific establishment, without which this nation cannot long endure.

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6/87

QCD and gluon observation

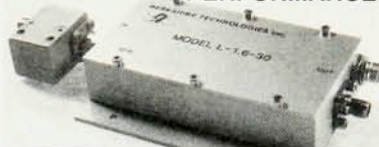
Every new accelerator always adds some new information from "bread and butter"-type data. However, as the cost of construction of accelerators escalates, patrons who finance such projects tend to require much more sensational discoveries. Such pressure for sensational discovery becomes more troublesome when coupled with a (little noticed?) technical aspect of big-accelerator-based physics: As the power of big accelerators grows, the detectors used also need to become bigger and more sophisticated. Not only is it impossible to construct such big and sophisticated detectors to be nearly 100% efficient, it is also (almost?) impossible to correct the data acquired from such detectors to the degree that they can be considered to come from an ideal detector. But predictions of models and theories are always geared for ideal detectors. The way this dilemma is solved is that research participants put vast arrays of data on the inefficiencies and defects of the detector into computer programs, use Monte Carlo simulation methods to simulate events according to the models and theories being considered (still for an ideal detector), push those computer-simulated events through the above-mentioned computer programs to make the predictions of the models and theories as deficient as the real detector, and then compare the results with the data.

In other words, the published experimental data and their comparisons with models and theories are for the specific inefficient detector, not for an ideal detector in general. If an outsider

wants to compare the data with his own models or theories, he simply cannot because his results are good only for an ideal detector. This aspect of big-accelerator-based physics increases the monopolistic power of the participants in big-accelerator experiments in interpreting the data and reduces the ability of outsiders to question critically their findings. Given this reality and the pressure to make sensational discoveries, there may exist both the motivation and the ability for some participants to exaggerate their findings.

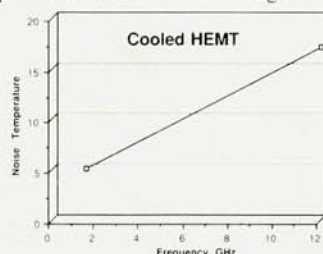
An explicit experience of mine illustrates that the above-mentioned aspect of big-accelerator-based physics is not just a fear on paper. Many readers probably remember the sensational claim of the discovery of "gluons" and of the confirmation of quantum chromodynamics many years ago. To understand what the claim really was, a word about the true status of quantum chromodynamics is in order: Quantum chromodynamics is a theory of nonobservable and strongly interacting quarks and gluons; the theory does not have predictive power since it cannot be solved exactly or approximately. What the groups who claimed the discovery of "gluons" did was to implement some suggestions of quantum chromodynamics into a scheme with many additional adjustable assumptions and parameters and to fit the data of electron-positron annihilation. Data fitting does play a role in advancing our understanding, especially when a theory lacks predictive power, but we cannot claim the objective confirmation of a theory nor the discovery of nonobservable particles such as "gluons" from data fitting, precisely because of the existence of those additional adjustable assumptions and parameters not derivable from the theory. At that time I suspected that the data from electron-positron annihilation actually were very similar to the ordinary soft hadron scattering data and did not warrant the claim of the discovery of "gluons." My paper expressing this view received two kinds of response from the referees and the associate editor of *Physical Review D*. The first kind was that quantum chromodynamics was so firmly confirmed and the discovery of gluons so firmly established that unless I could provide firm evidence to show the invalidity of quantum chromodynamics (no one can disprove a theory that does not have predictive power!), the paper should not be published. The second kind of response was that because my discussion was for an ideal detector and could not be compared with the published data, which were not corrected and were good only for the nonideal detector, the

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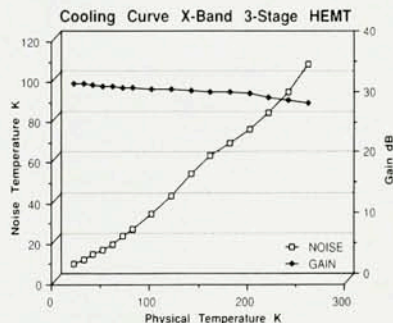
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Robert R. Borchers, the editor, is the Associate Director for Computation at Lawrence Livermore National Laboratory at the University of California.

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letters

paper should not be published.

After three years of dispute the paper was eventually not published, whereas in the meantime leading journals of high-energy physics rushed to publish many papers claiming further confirmations of quantum chromodynamics based on more data fittings or outright misunderstandings. Even the general physics magazines were pulled in to join the chorus to inform their readers that "gluons" had been discovered and quantum chromodynamics had been confirmed beyond a reasonable doubt as the theory of strong-interaction dynamics. From this personal experience, it is now my firm belief that unless the community of a "big science" can develop an effective system to separate sensational public relations affairs from objective discoveries, it may well degenerate, collapse under its own weight and be transformed into a "big bureaucratic meta-science."

CHIH KWAN CHEN

Lombard, Illinois

2/87

When physicists toss coins

A number of recent letters to PHYSICS TODAY have addressed funding priorities for research, and I wish to bring to the attention of the physics community a significant threat to the rational allocation of research money. I am greatly concerned that if high-energy physicists become interested in investigating coin tosses it will lead to the expenditure of tens of millions of dollars according to the following scenario.

The first step is dangerous because it appears quite innocuous: A theoretician arguing from symmetry (pardon the redundancy) will predict equal numbers of heads and tails. Of course there is always the danger that undergraduates will waste time on interesting questions when they need to be practicing solving square-well potentials, so the theoretician will not use terms like "fifty-fifty" or "half-and-half" but rather will define an abstract mixing angle θ_{ht} and argue that it equals $\pi/4$ radians, thereby insuring that his paper will only be understood by the cognoscenti. Experimental high-energy physicists will then rush to test this argument, and preliminary results will suggest strong confirmation. Later, though, a patient researcher will publish results, based on a stupefying number of coin tosses, that indicate a very slight deviation of θ_{ht} from $\pi/4$. These results, he will claim, cannot be explained as a statistical

fluctuation. Theoreticians will attack the experiment, resulting in bitter, divisive debates full of unpleasant invective; prominent theoreticians will begin to remove equally prominent experimenters from their Christmas card lists. The arguments will become even more vitriolic when the Russians announce that they obtained the same results with kopeks at Serpukhov, and then claim that their experiment has priority.

Out of the blue, a brilliant young researcher (in his tenth postdoc) will announce the results of an experiment that clearly shows a slight mass difference between the head side and the tail side of a coin; the small difference in mass will be calculated to be within 5% of the exact mass needed to cause the deviation of the heads-tails angle from $\pi/4$. This researcher will be awarded the Nobel Prize. Theoreticians will then rediscover an obscure paper published in an even more obscure journal in 1975 predicting the mass difference; the physicist responsible for that paper will not win the Nobel Prize. When the mass difference becomes generally accepted, the threat mentioned at the beginning of this letter will become reality: Tens of millions of dollars will be spent on detectors to look for the particle associated with the symmetry-breaking field.

Even more frightening is the possibility that high-energy physicists will realize that it is also possible for a coin to land on its edge, but will observe that this state is strongly suppressed. In analogy with the GIM mechanism proposed by Sheldon Glashow, John Iliopoulos and Luciano Maiani to explain the suppression of the two-muon decay mode of the K meson, the fact that a coin landing on its edge is almost never observed could be taken as evidence for another generation of quarks. No upper bound can be placed on the expenditures that would result from this suggestion. I therefore urge plasma and condensed-matter physicists to try to steer their high-energy colleagues away from investigating coin tosses.

EMORY KIMBROUGH

Montgomery, Alabama

9/86

How now, 'What's New?'

In the 3 October 1986 edition of *What's New*, the very informative bulletin issued every Friday by The American Physical Society over the signature of Robert L. Park, I read, "The concern of Berkeley scientists for the niceties of peer review may seem to be a recent development to those who recall the tremors produced by the creation of the Center for Advanced Materials at