Big Physics and New Ideas

In Faye Flam's article "Big physics provokes a backlash" (News & Comment, 11 Sept., p. 1468), Melvin Schwartz is quoted as saying that large collaborations "suffocate new ideas and discourage initiatives." I strongly disagree. Such experiments often provide opportunity for new initiatives that would not otherwise be possible in the current federal funding environment.

Innovative ideas are encouraged in the early phase of large experiments. Approval of an experiment by a laboratory or a funding agency is obtained on the basis of a compelling physics problem or class of problems. R&D funding then allows a variety of technological solutions to detector issues to be pursued, with the final choices made on the basis of prototype tests. An excellent example is the recent decision of the Superconducting Super Collider's (SSC's) Solenoidal Detector Collaboration (SDC) to choose an innovative solution to the problem of reading data out of a large detector at high speed with low noise, high precision, and a large dynamic range. This is the work of a rather young physicist, as was the competing proposal, which also performed well. In the "old days," in general, such parallel R&D efforts could not have been carried out.

Large experiments have a long lifetime, and thus it is fair to ask whether such opportunities arise after a detector is built. The answer is a definite yes. Detectors have numerous upgrades in response to new accelerator conditions and new physics opportunities. In addition, new initiatives, for which a separate complete experiment would not be approved because of the cost, can be carried out within the context of an existing detector. An example from Fermilab's Collider Detector Facility (CDF) is the use of neural network electronics to select B meson decays in a few microseconds. Young physicists proposed this, built the hardware, and are now testing it. An existing detector that was approved for other physics goals thus provided a platform from which new ideas and initiatives were encouraged. Building an entirely new "small" experiment would simply have cost too much and would likely have been rejected as being too risky.

The same opportunity for initiative exists for addressing physics issues. The CDF detector was approved on the basis of its capabilities for studying high mass (top quark, W, Z, supersymmetry, and so on) and high transverse momentum phenomena. However, once the detector was operating, its data could be used by CDF collaborators for whatever problems interested them. For example, a few physicists were interested in searching for new heavy stable particles. With the CDF data sample, they were able to carry out and publish this analysis.

There is another example that has had a major impact on our field. Although the CDF detector was not designed to be a high resolution meson spectrometer, a group of young physicists believed that rare, exclusive, final states in B meson decay, constituting less than 0.1% of B decays, could in fact be reconstructed in CDF, and that this would allow important properties of the B meson to be studied. They were successful and published the results. This convinced a previously skeptical high energy physics community that important studies of the electroweak interaction could be carried out with B decays at hadron colliders.

As mentioned in Flam's article, Martin Perl's success in discovering the tau lepton within the context of a large collaboration is an example of the ability of individuals to pursue their own physics interest. However, Flam quotes Perl as saying that this was possible because the SPEAR experiment "was designed without specific goals . . ." and Burton Richter as saying, "We wanted to look for new phenomena." The implication is that the large experiments today are different, that they have, instead, single scientific goals. This is not so. The CDF and D0 experiments at Fermilab, the LEP experiments at CERN, as well as the SDC and GEM experiments at the SSC, were not approved for a single physics goal, but rather to search for new phenomena at the highest available energies in $\bar{p}p$ and e^+e^- collisions.

This is not to say that there are no problem with very large collaborations. There are certainly sociological problems, and great care must be taken so that the young physicists get the credit for their important contributions. But the view that there is no room for new ideas and initiative in large experiments and that the many talented scientists all move in lockstep toward a single goal is simply wrong.

Melvyn J. Shochet
Enrico Fermi Institute, University of Chicago, Chicago, IL 60637

Flam's article presents a rather one-sided picture of the dynamics of large collaborations. Perl's discovery of the tau particle came about exactly because he was embedded in a group which at the time was very large! There is no less room for creativity within the Collider Detector Facility at Fermilab than there was in Mark II. The data stream is enormous, and it has been demonstrated that small groups or even individuals can mine that stream with great creativity.

There is yet another aspect of these collaborations not mentioned by Flam. In the old days that Schwartz describes in the article, a student could work at a small machine with a professor and maybe one or two other students. His exposure would come from presenting a paper at an American Physical Society meeting, perhaps only once. Contrast this with a large collaboration where the work in progress is reported every 2 weeks. When a student or postdoc presents his work, it will be heard by members of maybe 30 of the top institutions in the world. In addition, there are talks, as before, at the high energy physics conferences. Sources of help and criticism are much broader, and the student is exposed to a much greater spectrum of co-workers. Indeed, the best of the students and postdocs from CDF are now moving into tenured spots at top universities, which indicates that their work is well recognized.

I do not agree with Schwartz's suggestion that we need a new breed of physicist called "detector builders." In the 1970s, after strong focusing was invented, we first saw a split of this type. There were the accelerator builders, the bubble chamber builders, and a vast army of graduate students who only knew how to run the TVGP and SQAW reconstruction programs! Fortunately, the modern collider has closed this gap by entwining the detector and machine so completely that physicists are, once again, working and talking with each other. Students get well-rounded experience and have contact with many different types of experts.

We are entering a new era of physics, and there are problems with the large groups. CDF, with 400 members, is at a size where the democratic process can still function. I think the SSC detector groups will have to evolve new ways of coping with the problem of individual creativity as opposed to the discipline required by a detector that is well integrated and must be maintained for a long period after the original subcomponent builders have moved to other projects or as upgrades. I do not believe it will be an impossible task. The idea that these detectors will be manned by groups of a 1000 physicists in "lockstep" with no exposure of their individual contributions is ridiculous! I wonder if Schwartz has ever been able to get even *two* physicists in lockstep!

Alvin V. Tollestrup
Fermi National Accelerator Laboratory, Post Office Box 500, Batavia, IL 60510

Corrections and Clarifications

In the Research News article "Pot, heroin unlock new areas for neuroscience" by Marcia Barinaga (18 Dec., p. 1882), the diagram of the molecule anandamide on page 1883 was incorrect. The correct structure appears on page 1948 of the same issue, in figure 1A of the report "Isolation and structure of a brain constituent that binds to the cannabinoid receptor" by W. A. Devane *et al.* (p. 1946).