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LETTERS

B Factory Proposals

I write in response to the article by Faye Flam, "Cornell leads battle of the B factories" (News, 27 Aug., p. 1111).

There are five principal concerns about the proposal by Cornell University. First, synchrotron radiation heating of the vacuum chamber is far beyond anything that has been allowed before in any storage ring anywhere in the world. Second, the superconducting cavities proposed by Cornell are specified to operate at twice the accelerating gradient of any superconducting cavity that has ever been used in an accelerator. Third, the so-called "crab-crossing" technique, which was proposed by Brookhaven and the Stanford Linear Accelerator (SLAC) physicists for use in future linear colliders, has never been tried. Fourth, the manpower resources of Cornell are marginal for the task. Fifth, there is concern about the schedule.

Cornell has estimated that it can construct a B factory for significantly less than can SLAC. However, this estimate may not accurately reflect the true costs associated with the program. In comparing costs, government officials should take into account the total cost of each proposal, including the commissioning and ongoing operational costs associated with bringing the machine up to the performance standards necessary to conduct the scientific work for which it is designed. In determining the site for the B factory, officials should also take into account the long-term interests of the U.S. high energy program. SLAC represents a billion-dollar federal investment that plays, and can continue to play, a central role in development of high energy electron accelerators. It seems to me unwise to create a new national lab, financed by the Department of Energy, that would require duplicating facilities already in place at SLAC, while simultaneously phasing down the nation's premiere electron physics lab.

Finally, I disagree with the remark that SLAC "has been teetering near extinction since its last big project, the Stanford Linear Collider, proved a disappointment. . . ." The linear collider has surpassed all the performance goals set for it for this year; the data taken up to now on the linear collider will produce, among other things, a measurement of the Weinberg angle that can be surpassed only by combining 24 separate measurements from CERN; the fixed-target program in 2 months of operation has produced the best measurement of the neutron spin-structure function that exists in the world; and recent proposals for use of our facilities were sufficient in number to commit all of our available running time through 1999, if I had allowed the program committee to commit us so far in advance. This is hardly a program "teetering near extinction."

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Biology at Caltech

Robert Olby, in his review (18 June, p. 1825) of Lily E. Kay's book *The Molecular Vision of Life* (Oxford University Press, New York, 1993), writes

> In [the] context of the industrialization and eugenic "cleansing" of the western seaboard Caltech [the California Institute of Technology] became the spearhead of the movement in the West for progress by technology and science.

How fashionable. How politically correct. And what a skewed view of the locus of some of the major scientific advances of our century. Kay's book is a distortion, and Olby's review an echo thereof. According to Olby,

> Kay comes to the conclusion that the existence of these long-term goals [to further the "fundamental aim of social control"] in the Rockefeller Foundation's program did not amount to a Machiavellian subversion and co-optation of academia. Rather, cultural hegemony was achieved "through the explicit and tacit constitutive processes of consensus formation."

That is, co-optation was not necessary. They all shared the same goals. How neat.

The essential fallacy of the book and the review is purporting to divine what was in the minds and psyches of Caltech scientists and what motivations underlay their research and guided their choices at three to six decades' remove. This is social pseudoscience.

To illuminate this fallacy, let us apply the same technique to the minds of Kay and Olby in 1993. What motivates their choice of subject matter and perspective? Might we suppose that these authors live in a dark fear that the social and cultural processes they study minutely are in fact but marginal factors in the human drama—that the (so far) hidden internal processes, the (dare we say it?) genetic factors innate within each human being are much more determinative of their

intelligence, personality, even character than the external circumstances which are social psychology's raison d'être?

Their apprehension is perhaps akin to that of the astronomers who are now startled by the concept that the universe they study may be but a quite minor part of an unobserved whole.

It has long been clear that obvious human physical characteristics such as height or eye color are genetically determined. But as long as the evidence for the genetic basis of the more subtle and distinctive human traits has been limited to nebulous correlations and twin studies, it has never been definitive and convincing. But now with the spectacular progress in our understanding of genetic processes and, in particular, with the promise inherent in the Human Genome Project, of eventual possible elucidation of direct causal linkages between genetic variations and behavioral traits, the antireductionists foresee their universe fading in significance.

How better to slow down or even halt this development of biological understanding than to impugn the motives of those whose research launched the "molecular vision of life" and thus revive the fear of genetic control, of "Brave New World" scenarios?

Could this be? No, we don't really believe these are the motivations of Kay and Olby. But by the same token we do not accept their presumption to divine the motives of the great biologists and chemists of Caltech who, in their elegant pursuit of basic scientific knowledge, established the framework for the extraordinary developments in biology today.

Might we consider the concept that the officers of the Rockefeller Foundation could have favored the Caltech scientists simply because their proposals were the more insightful and ingenious, their approach the more solid, their vision of biology the more far-reaching? And how right they were! A banal explanation to be sure. But it is nice to know that sometimes philanthropy and insight succeed.

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Response: Sinsheimer and Horowitz describe my review as an "echo" of Kay's distorted view of "some of the major advances of our century." I do not accept that such is the case. The chief aim of my review was to express clearly and succintly the thesis developed in the book, which I consider sufficiently important to deserve being widely read and discussed. Its deficiencies, which I alluded to, concerned Kay's comparative institutional analysis and the absence of any discussion of the relevance of her interpretation to the international dimensions of molecular biology. The history, in its global extent, shows the important role that social and economic relevance has played in the development of molecular biology, from polymer science and plant virology to chronic lobar pneumonia and blood chemistry. That some of this research has been supported on the grounds of its promise to aid us to "control" or deal with human problems is no surprise. How close such aims lay to "eugenics" in the popular meaning of that term is more difficult to determine.

There are grounds for querying Kay's statements about the continuity of the eugenic goals of the Caltech program, but I believe these stem more from her use of words like "intervene," "social control," and of course "eugenics" in different contexts and with different shades of meaning. There is a world of difference between the old eugenics and "reform eugenics." Nevertheless, I accept her conclusion that the Rockefeller Foundation asked for projects that had relevance to social needs and that it was not prepared to go on supporting science simply for its own sake. The Foundation officers accepted this remit, and scientists were aware of it. I see nothing morally reprehensible in this (*1*).

Sinsheimer and Horowitz stress the power of the new molecular genetics to conquer aspects of human nature hitherto the preserve of the "antireductionists." Leaving aside their simplistic representation of genetic and environmental determinism, and granted this newfound power, I doubt that within the scientific community, geneticists included, there is a consensus as to what effect molecular genetics will have on our understanding of the determination of human personality traits. However, we may hope that the quality of research on human behavioral genetics will improve (*2*).

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References and Notes

1. For a criticism of Kay's book on this point, see an article by R. L. Sinsheimer in the Caltech magazine *Engineering Science*, **54**, 32, (1993); for a somewhat different interpretation of Rockefeller Foundation policy, see R. Kohler, *Partners in Science* (Univ. of Chicago Press, Chicago, IL, 1991).
2. J. Horgan, *Sci. Am.* **268**, 122 (June 1993).

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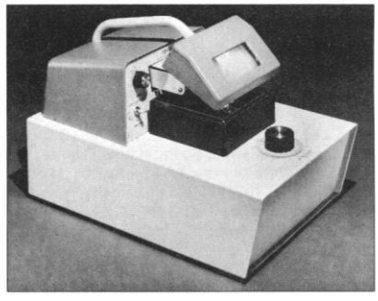

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