Letters

Grant Financing: PI Salaries

Desperate times require desperate measures, or so we are told. If ever a piece of time-honored advice were revealed as empty and dangerous, it is in Martin Frank's letter of 26 January (p. 393).

Frank suggests that if the National Institutes of Health (NIH) ceased including the salaries of principal investigators (PIs) in grants, enough money would be liberated to fund 5000 new grants. As to what would happen to the scientists whose universities would no longer be able to pay their salaries, he tells us not to worry: they could go to smaller colleges and universities that have "hard" money with which to pay them. Moreover, it would have the further advantage of attracting more undergraduates to the research profession because they would be exposed to active scientists.

This idea is inconsistent with an understanding of institutional finances and of the history of American higher education. With respect to the former, it would be recognized that the terms "hard" and "soft" money are only metaphors. They do not really describe different kinds of money, but money that comes from different sources. For a college or university needing to pay its faculty, those sources are limited in number and known. They consist of student tuitions, governmental appropriations, gifts and income from endowment, and salary offsets from research grants and contracts. The idea that small colleges and universities have a surplus of something called "hard money" that they can use to pay the salaries of scientists who leave the faculties of Harvard, Stanford, and the University of Michigan is preposterous. The thought of PIs from the Harvard Medical School faculty being snapped up by Williams, Amherst, and Wesleyan has a Woody Allen–like quality about it.

An equally serious flaw in the idea is that, if it were implemented, American higher education would be turned on its head. Our universities have been built on the premise that research and graduate education go together because each enriches the other. Therefore, they are best done in the same place by the same people. This system works, as the splendid accomplishments of our universities demonstrate. To destroy so successful and valuable a system in order to squeeze a few more grants out of NIH (in the unlikely event that the money saved would actually remain in the NIH budget) would be an instance of terminal expediency.

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Carrel's Cultures

I was surprised to see that Barbara Culliton, in her recent article "Rockefeller braces for Baltimore" (News & Comment, 12 Jan., p. 148) perpetuated the myth that Alexis Carrell "kept a chicken heart 'alive' for an incredible 34 years." Several errors are compounded in this one sentence (*1*). First, it was not a chicken heart that Carrel kept alive for 34 years; it was a culture of fibroblasts derived from embryonic chicken heart. As a surgeon, Carrel was interested in wound repair, and he hoped that the newly developed technique of tissue culture could be applied to studying wound healing. His assistant Montrose Burrows went to Yale University, where Ross Granville Harrison was observing directly the outgrowth of nerves from fragments of embryonic frog spinal cord maintained in clots of lymph. On Burrows' return to the Rockefeller Institute, he and Carrel began growing cells from a variety of tissues.

Carrel did not use organ perfusion in this work, although his organ perfusion studies began at about the time that he took up tissue culture. The perfusion experiments achieved considerable publicity in the 1930s after the collaboration between Carrel and Charles Lindbergh that culminated in the development of the so-called "glass heart" (*2*).

Carrel maintained the "immortal" heart cell cultures for only 6 months of their long life (*1*). The cultures were probably established in January 1912 and became the responsibility of Albert Ebeling in June 1912. Ebeling took them with him when he moved to the Lederle Laboratories of American Cyanamid in 1939, where the cultures were eventually discarded in 1946.

In the light of many subsequent studies, it seems unlikely that Carrel's cells were immortal. Like normal human diploid cells, chicken cells are very stable in culture, and as far as I am aware no authenticated cases of spontaneous transformation of chick cells have been reported. The longevity of Carrel's cultures has been explained by inadvertent or deliberate contamination of the cultures by cells present in the chick embryo extract used to feed the cultures (*1*). However, as B. L. Strehler remarked, the ultimate effects of the aging process made it impossible for Carrel to respond in his own defense to the questions that were being raised already in the 1940s (*3*).

Although Carrel's work on these cells was literally "incredible," Culliton is not the first (and probably not the last) to be impressed. In 1921, a journalist for *The World* wrote that if all the cells had been kept, they would have formed a "rooster big enough today to cross the Atlantic in a stride; it would also be so monstrous that when perched on this mundane sphere, the World, it would look like a weathercock" (*4*).

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REFERENCES

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2. A. Carrel and C. Lindbergh, *The Culture of Organs* (Hoeber, New York, 1938).
3. B. L. Strehler, *Time, Cells and Aging* (Academic Press, New York, 1977).
4. *The World* (New York), 12 June 1921.

Asteroid Paradox

Despite the Hollywood maxim that there is no such thing as bad publicity, the quotes from my lengthy telephone conversation reported in Richard A. Kerr's article "The great asteroid roast" (Research News, 2 Feb., p. 527) tempt me to swear off talking to reporters. There are two reasons for my being disturbed. The first is simply that I don't want people to think I am as intolerably arrogant as these quotations would suggest. The second is that for many years I have been advancing the view that the eagerness of reporters, historians, and many scientists to consider all serious scientific puzzles in terms of personal controversies is detrimental to the progress of science and that the apparent historical importance of controversies stems primarily from their sensational nature. I propose that it would be more illuminating if reporters were to use