Animal Research

Jeffrey L. Fox closes his article on changes in animal care policy (News and Comment, 27 Apr., p. 364) with the comment that scientists should not use dramatic testimony from patients who have benefited from animal research because it "runs the risk of widening rather than narrowing the gap between the animal welfare and research communities." I disagree. The nuclear power industry is in trouble in part because they chose to disregard those who opposed the infant technology. Now those in opposition have such power that it is nearly impossible to contradict them.

Similarly, the research community must take the strongest possible action now while the animal rightists are still relatively vulnerable. I have watched them in action and attended rallies. They are not often open to reason. If we as scientists are to continue to be able to work on research with animal systems, we must use all of the tools at our disposal. In this case, it means using those extremely powerful emotional testimonials, showing up at animal rights rallies with literature supporting animal use in research, and generally trying to reach the public with the importance and necessity of animal research. I encourage local scientific groups to get together and fight this destructive movement to ban animal research before it is too late.

SHARON LYNN CAMPBELL
39-65 52nd Street,
Woodside, New York 11377

Yellow Fever Research

Colin Norman, in his article "The unsung hero of yellow fever" (News and Comment, 30 Mar., p. 1370), does an excellent job of defining the controversy over who really solved the riddle of yellow fever. But, as coauthors of *Guinea Pig Doctors*, we feel obligated to comment on several critical points.

One of those points involves the nature of the key discovery itself. The decisive piece of information was not the 12-day incubation period, as stated in the article. The key was the fact that yellow fever was only contagious during the early stages of the disease, when it was difficult to distinguish from typhoid, malaria, and influenza. Earlier experiments with mosquitoes failed because researchers, not wanting to clutter their data with misdiagnosed patients, always used late-state cases. By that time the patients definitely had yellow fever but, unknown to the scientists, could no longer pass it on.

Ask yourself, as we asked ourselves, which of the players was in a position to make this discovery? Was it Walter Reed and James Carroll, who were convinced almost until the very end that yellow fever was caused by bacteria? Or was it Jesse Lazear, who had spent years studying mosquito-borne disease?

The Reed scholars are saying, in essence, that Jesse Lazear, the mosquito expert, was not capable of understanding his own notes on mosquitoes but that the bacteriologists, finding the notebook after Lazear's death, understood instantly what it all meant. Our readings tell us that, while Lazear may have been self-effacing to a fault—while he may have been too sincere, too honest, and too naïve for the world he found himself in—the one thing he was not was stupid.

In any event, William Bean suggests that Reed should get the credit for brilliant confirming experiments, and that after all there was plenty of credit to go around. By that argument we could quit making so much of mere discovery and start awarding Nobel Prizes to the folks who do the follow-up.

Finally, we are bemused by the author's slightly condescending attitude toward the popular style in which we presented our work in *Guinea Pig Doctors*. What we are objecting to is a question of tone and example that was probably quite unintentional. Nevertheless, it opens up the issue of popularization, its purposes, and its place in the literature of science.

Norman complains that our book is not scholarly enough, and he implies that our research relied heavily on a single paper published in the alumni magazine of the Columbia University College of Physicians and Surgeons, Lazear's alma mater. It is true that such a paper exists, that we found it, and that it bolsters our conclusions. But it is hardly the cornerstone of our case against Reed.

A far stronger piece of evidence, for example, comes from the writings of Albert E. Truby (*1*), the Army doctor who cataloged Lazear's personal effects after his death. Truby found what he termed "the precious notebook" and gave it to Reed.

Reed, said Truby, became very excited about the discovery. "Hurrying back to his quarters, he immediately began to investigate the papers obtained from Lazear's quarters. In the notebook he found the data he wanted."