ents who can afford the expense of college and graduate school. The percentage of minority applicants to graduate schools whose parents can afford to finance graduate education is far less than that of white applicants. While there are some loan programs, they are particularly unattractive to members of minority groups from impoverished backgrounds. The more attractive financial prospects of a career in medicine mean that if the same debts must be incurred in training for a career as a physician or as a biomedical researcher, the former will more often be the preferred choice.

Currently the major support for access to research careers by minority students in graduate school is being provided by private foundations. We find it hard to understand why the Department of Health, Education, and Welfare is doing so little to support graduate training of minority applicants when everyone agrees that there is a shortage of such individuals.

JOHN N. FAIN
R. H. POINTER
Division of Biological and Medical Sciences, Brown University, Providence, Rhode Island 02912

Labor-Intensive Production

In his editorial "Corporations and the less developed countries" (30 Nov. 1973, p. 873), Philip H. Abelson mentions an IBM typewriter plant in Bogotá, Columbia, that relies on labor-intensive production techniques as an example of increased corporate responsiveness to the desires of the host countries. I suggest that an additional, more telling, incentive is at play—corporate self-interest.

This conclusion comes from my observation of a similar project under development in Bombay, India—an industrial estate near the international airport that will deal exclusively with the assembly of electronic equipment. Sponsored by the semigovernmental Trade Development Authority, the estate will house predominantly non-Indian concerns. It is to be a free trade zone, devoted exclusively to export production. Components will be flown in and assembled at the estate, and the finished product will be flown out again. The production process is highly labor-intensive. The value added in India will amount to more than 50 percent of the product's final cost. The project will provide employment for some 45,000 people.

In this case, labor-intensive techniques are being developed, not because of any benevolent feelings on the part of employers, but because labor-intensive techniques are profitable. Because the wage rates in India are low, it is more profitable to have the electronic equipment assembled by hand in India than to use a capital-intensive (or labor-intensive) technology in a developed country. The companies get a cheaper product; India gets the employment.

Such mutual benefit is likely to provide a more reliable and significant binding cement between the multinational corporations and the less developed countries than is corporate benevolence.

DAVIDSON R. GWATKIN
55 Lodi Estate, New Delhi 11003, India

Virus Research

The provocative report "Microbiology: Hazardous profession faces new uncertainties" by Nicholas Wade (News and Comment, 9 Nov. 1973, p. 566) raises important questions, all of which deserve consideration and discussion. In general, a distinction should be made between the primary hazards to which the scientist is exposed and the potential of secondary hazards to the public at large. Most discussions of the latter tend to ignore the biologic constraints by which infectious disease patterns are stabilized by a kind of "environmental homeostasis." Sulkin and Pike's extensive reviews of laboratory-acquired infection (*1*) fail to document the secondary spread of agents initially alien to man (for example, louping ill and Newcastle disease viruses) or intrinsically pathogenic for man, but acquired by an unnatural route in the absence of the natural vector (for example, Venezuelan equine encephalitis virus).

Wade attributes to those in "virologic circles" a concern that "the ability to genetically manipulate flu viruses could lead to a new combination that might escape from the laboratory, by infecting an employee, say, and spread to the population at large." He then quotes Wallace Rowe of the National Institute of Allergy and Infectious Diseases as saying, "This could recreate the conditions for an influenza pandemic like that of 1918." Rowe voices

a concern that I have also heard expressed by others. While there is a growing acceptance of my suggestion (*2*) of the possible role of genetic recombination of influenza viruses in nature in the genesis of pandemic influenza, re-creation of "the conditions for an influenza pandemic like that of 1918," would require not only a new virus but a world war and the subtraction of a half century of progress in the health sciences, including the elimination of most antibacterial drugs in present use. (Most deaths in 1918 were the result of secondary bacterial pneumonia.) What about the possibility of a pandemic, not like that of 1918, but like those of 1957 or 1968? How can possibility be refuted in biologic science? Of course it is possible that hybrid viruses of laboratory origin may "escape" despite past evidence to the contrary. However, do we then cease our "genetic manipulations" of the viruses at a time when we are just beginning to appreciate their implications? Is it generally appreciated that contemporary influenza vaccines are made from recombinant or hybrid viruses deliberately designed in the laboratory for optimal production characteristics (a prime requirement for vaccines which have to be redesigned every few years to match mutations of the virus in nature)? More important, the segregation of influenza virus genes by genetic recombination (reassortment) has, in recent years, accelerated our understanding of viral replication, the biologic function of the viral proteins, and the nature of antigenic variation. Is the abandonment of this method of genetic analysis being implicitly proposed?

The question is not "Could it happen?" but rather "Is it likely to happen?" If it does, does the risk exceed the real and present danger that is constantly posed by mutation or recombination in nature of wild type viruses?

A pandemic of influenza apparently requires: (i) the appearance of an influenza A virus, the hemagglutinin antigen of which differs markedly from that of the preceding virus; (ii) a population that has no antibody to the new virus but high levels to the old one; and (iii) probably as a consequence of (ii), the disappearance of the old virus to provide an ecologic niche for the transmission and circulation of the new strain.

How is an antigenically novel virus produced? Certainly not by genetic recombination, which adds nothing new, but rather reassorts the old (that is, the antigens of the input parental viruses).

Certain of these "old" antigens now present in animal influenza viruses have not yet seen the human host; their recombination with human viruses could create hybrids endowed with genes necessary for their replication in man. Clearly, such viruses, one of which I am guilty of "creating" (*2*), should not be considered as candidates for live virus vaccines in man. It is not likely that they will "escape from the laboratory, by infecting an employee." Laboratory-acquired infection by an influenza virus is a rarity even with wild type strains recently isolated from man. Almost all strains of influenza virus, upon their isolation in alien laboratory hosts (principally the chick embryo). lose their virulence for man. Indeed, a problem with experimental live-virus vaccines is the maintenance of sufficient human virulence to allow infection to occur. Furthermore, secondary spread from vaccinated subjects seldom has been observed. To add to this, the indolent progress of a wild type virus in the early stages of a pandemic has been frequently observed and suggests the need for a concatenation of factors, including optimal population density, environment, and season for a successful pathogen to emerge. Finally, because influenza virus virulence is clearly polygenic, the crossing of domesticated viruses will usually lead to the production of progeny of intermediate virulence (that is, less virulent than the most virulent parent) as "virulence genes" are redistributed. I submit that all present viral vaccines have been obtained by "genetic manipulations"—largely empirical.

It is time to capitalize on the legacy of modern molecular biology in the deliberate design and choice of the viruses with which we shall live and which shall defend us. Certainly, as his power in the laboratory increases, the biological scientist must couple his enthusiasm with sobriety and caution. His colleagues and critics owe him similar restraint in our present climate of research pragmatism.

EDWIN D. KILBOURNE
Department of Microbiology,
Mount Sinai School of Medicine,
City University of New York,
New York 10029

References

1. S. E. Sulkin and R. M. Pike, *New Engl. J. Med.* **241**, 205 (1949); *Am. J. Public Health* **41**, 769 (1951); *Bacteriol. Rev.* **25**, 203 (1961).
2. E. D. Kilbourne, *Science* **160**, 74 (1968).