Letters

The Poor Are Getting Richer

I should like to comment on the article by Eric Hutchinson, "Politics in higher education" (27 Nov. 1964, p. 1139), from the point of view of a member of one of those smaller institutions which are supposed to be rapidly degenerating. [Hutchinson wrote that as a result of government support of university research, through which "The rich get richer and the poor get poorer, . . . the poor institution gets poorer and poorer in the quality of its faculty."] I believe the effects of research grants on the small colleges and universities are not as bad as he implied. A survey of electrical engineering departments, particularly in the West and Midwest, will show that almost without exception the faculties are considerably stronger than at any time in their previous history. Certainly there has been an improvement in the diversity and quantity of educational background of the faculty members. In addition, a considerably larger faction of them are engaged in research than would be possible without government funds.

At the University of Colorado almost all the faculty members with appropriate qualifications have been able to obtain support for their research needs (that is not to say that we have been able to obtain all the funding we should like to have). Research grants have made it possible for a fair number of men to work at institutions where as little as 10 or 15 years ago research activity was practically impossible because of heavy teaching loads and lack of funds. There is certainly a danger that the rich get richer and the poor poorer; however, I think an examination of the current situation will show that although the rich have been getting richer, the poor have also been getting richer, and possibly at a greater rate. Again speaking from experience at the University of Colorado over the last 2 years, we are currently in a position to draw staff with better backgrounds and from a wider variety of institutions and industry than at any time in our previous history. This seems to be true at most of the universities in the Rocky Mountain and midwestern regions, and along with this there is a rapid increase in the graduate enrollment. Similarly, I believe you will find that the contributions of new and important ideas from these universities to the national scientific community are increasing.

The "emergence of a small number of super-universities of extraordinary prestige" which Hutchinson foresees need not be at the expense of the other schools. There are more first-class staff members than can be accommodated on a few campuses, and, if the granting agencies will continue to recognize talent where it is, the smaller institutions will continue to improve in quality. Additionally, members of the faculties at the most prestigious universities must be careful not to inflate their own importance in the same way that those of us in the less well-known schools are likely to exaggerate our contributions, for, as in the past, the best-known schools will continue to have a great deal to say about how our country's educational and scientific programs develop.

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Science Self-Generated

I wish to examine critically some remarks of V. R. Potter ("Society and science," 20 Nov. 1964, p. 1018) concerning the historical roots of the "upsurge in molecular biology." According to Potter, the freeing of funds from the polio program, owing to the development of a killed-virus vaccine, as well as a coincidental increase in the support of cancer research has "permitted and encouraged the expansion of . . . research with no particular disease in mind or research that is directed toward understanding the nature of life processes in general. . . . What has emerged is the new science of molecular biology. . . ." The tenor of these remarks is that the upsurge in molecular biology was a direct consequence of the fortuitous availability of research capacity and of funds that had been raised to support "a great humanitarian effort to increase the well-being of mankind." In the light of these remarks, molecular biology takes on the character of a somewhat prodigious child of medical research.

Such a pragmatic view is not unusual in this technological society and era but misleads with respect to both the actual history and the inherent developmental tendencies of the sciences, in this particular instance, of molecular biology. Proof that DNA is the physical carrier of heredity goes back to 1944 (O. T. Avery, C. M. MacLeod, M. M. McCarty, *J. Exptl. Med.* **79**, 137). The term molecular biology was introduced independently in 1952 by P. Weiss and W. T. Astbury (P. Weiss, personal communication). The DNA model with its genetic implications, according to Potter the "icon" of molecular biologists, was unveiled in 1953 (J. D. Watson and F. H. C. Crick, *Nature* **171**, 737, 964), and the existence of a genetic code was first proposed by G. Gamow in 1954 (*Nature* **173**, 318). None of these germinal accomplishments can be related to a sudden influx of funds from seemingly completed or intellectually unfocused medical-development programs. Not without humor, Potter has pointed out that "molecular biologists have a religion all of their own." I do not believe, however, that he would be prepared to accept the premise that the upsurge of religions results from the availability of funds for the erection of cathedrals or the remuneration of clergymen.

The central issue is raised by the mutually opposing views of scientific research as a promoted and utilitarian activity or as an autonomous development of cognition of the laws of nature. The contemporary scene is populated by promoters of research, a fact which may not be unrelated to the "unbalanced growth" of research expenditures as compared to the growth rate of the scientific community or the scientific literature [D. J. de Solla Price, *Little Science, Big Science* (Columbia Univ. Press, New York, 1963), pp. 92 ff.]. It is especially true that the medical Establishment in the U.S. is functioning as one of the large promoters of the life sciences; so did agriculture several decades ago, and so may astronautics do in the future. It appears, however, that "programs," whether for the development of a vaccine, an atomic bomb, or a space vehicle, produce less scientific "fallout" than is optimistically assumed. On the contrary, they are based upon preexisting basic scientific knowledge.

Technologies may seem to offer favorable nutrition and incubation for the scientific culture, but it is doubtful that they can alter the basic shape of the growth curve of this culture (Price, *ibid.*, p. 17). The autocatalytic growth of science as well as of discreet scientific disciplines is plausibly the result of a chain reaction of ideas, that is, it is self-perpetuating as well as inherent in and intrinsic to the active system under consideration. The "inoculum" of molecular biology was probably the demonstration in 1936 (W. M. Stanley, *Phytopathol.* **26**, 305) of the nature of tobacco mosaic virus as a macromolecular entity containing the instructions for its identical replication by cells. The "lag period" lasted to 1953, and the Watson-Crick-Wilkins model marks, perhaps, the beginning of the exponential phase of growth of the field. This growth has, at present, all the characteristics of a chain reaction of discoveries. One may hope that the body of knowledge and generalization so acquired will permit of medical applications, but such hope neither has generated the field nor is sustaining its growth.

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Information Exchange

The problem of timeliness in making research information available, discussed by Garvey and Griffith in their interesting article "Scientific information exchange in psychology" (25 Dec. 1964, p. 1655), is not peculiar to the field of psychology but exists throughout all of science. The authors might well have mentioned the role of the Science Information Exchange (SIE) of the Smithsonian Institution in providing psychologists with information concerning who is doing what research. Many have found this service useful in bridging the hiatus between the start of a research project and the eventual publication of its results. SIE answers almost 1000 questions a year in the field of psychology and has approximately 6300 grants or contracts registered for this field.

SIE answers questions, without charge, from any scientist at a recognized research institution, about who is currently working on any specific segment of scientific research [see M. E. Freeman and D. F. Hersey, *Science* **149**, 119 (1963)]. The more specific the question, the more carefully the information can be selected to meet the needs of the inquirer. Requests for information should be directed to the address given below.

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The Stumptail Macaque as a Laboratory Subject

Inasmuch as we have received several inquiries concerning the stumptail macaque as a laboratory subject since the publication of Kling and Orbach's reports [*Science* **139**, 45 (1963); *Animal Behavior* **12**, 343 (1964)], and because many of the people inquiring have had little or no previous experience with primates, we would like to call the attention of investigators considering use of the stumptail macaque to our experiences with these animals.

Whereas the young animals, in the age ranges used by Kling and Orbach, are generally docile and friendly, as they mature their potential for inflicting injury increases, and some may become resistant to handling and dangerous both to laboratory personnel and to subordinate cagemates. In maintaining a group of these animals, all of which had at least partially erupted canines and hence may be considered to have been 4 years of age or more, we found that certain individuals resisted handling by even our most experienced and successful monkey handlers. These individuals when captured would struggle and attempt to bite their captor and inflicted cuts even through heavy protective gloves. Furthermore, when the animals were housed together in a large cage there was persistent fighting, and attacks on subordinates resulted in multiple bite wounds on the extremities, producing large necrotic lesions requiring extensive treatment. When housed with monkeys of other taxa the stumptail macaques were ordinarily quite peaceable when with more dominant partners, but, when with subordinate monkeys, some persistently attacked and injured their cagemates.

It is our conclusion that, whereas the stumptail macaque (*Macaca speciosa*) is certainly more tractable than the rhesus (*Macaca mulatta*), it is not the most docile laboratory subject. We have individuals of the taxa *Cynopithecus niger, Ateles* spp., *Cebus albifrons, Macaca maurus,* and *Macaca nemestrina* which we can freely handle at some ages, even to the point of obtaining blood samples without restraining the animals. Some individuals of other taxa also permit free handling as juveniles, but some individuals of all taxa named may resist all efforts to win their confidence.

Investigators should make final selection of subjects on the basis of specific experimental requirements, and should always bear in mind that, aside from harboring many contagious diseases, at least the larger individuals of even the generally more tractable taxa are often difficult to handle and may require as many precautions as must be taken with most rhesus and other less tractable monkeys.

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Molecular Mimicry in Biological Adaptation

In the spirit of G. G. Simpson's incisive appeal for a synthesis of molecular and organismal viewpoints toward the greater understanding of biological adaptations (18 Dec. 1964, p. 1535), I should like to call attention to the phenomenon of molecular mimicry [*Am. Naturalist* **98**, 129 (1964)]. In this situation natural selection can lead to the mimicking of antigenic determinants of hosts by their parasites, with the possible subsequent development of antigenic polymorphisms in the parasitized host population as a defensive adaptation. Furthermore, it is possible that ecologically related but phylogenetically diverse parasites could develop similar "eclipsed antigens," which would truly be convergent evolution at the molecular (or submolecular) level.

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