Letters

Origin of Races

In the 15 February issue of *Science* was printed a brief report on the meeting of Section H, Anthropology, of the AAAS in Philadelphia, late in 1963. I was not there. In this report Th. Dobzhansky is quoted as having said that my theory of the origin of races is wrong and that I showed irresponsibility and naiveté in writing without concern for the impact of my "pronouncements."

If Dobzhansky will again read carefully Bernhard Rensch's book *Evolution Above the Species Level* (Columbia University Press, New York, 1960), the foreword to which he himself wrote, he will see that Rensch's book (unknown to me before 27 February 1963) had anticipated, in principle if not in detail, virtually everything that I said that Dobzhansky doesn't like.

On page 8 Rensch states that owing to "a single gene mutation various characters can be altered that control whole systems of physiological processes, e.g., mutational alterations in the quantity of active tissue of a hormone gland like the pituitary." (That is just the kind of mutation probably involved in crossing the *erectus-sapiens* threshold.)

On page 18: "Primitive and more advanced geographic races may live at the same time."

On pages 89 and 90: "The 'normal' geographic races usually differ in several genes as they have been subjected to natural selection for tens of thousands of generations." (In man 10,000 generations equals roughly 250,000 years.)

Dobzhansky's chief objection to my work, however, revolves around the fact that, in a 3½-page introduction, no specific mention was made of the possibility that the races of man had evolved from *Homo erectus* into *Homo sapiens* from a single mutation through peripheral gene flow, whereas I had discussed this possibility at length in the body of the book. However, if one grants the possibility of such a mutation as that exemplified by Rensch on page 8, happening once in some 100,000 individuals in a generation, as both Rensch and Neel [J. V. Neel, "Mutations in the human population," in *Methodology in Human Genetics*, W. J. Burdette, Ed. (Holden-Day, San Francisco, Calif., 1962), pp. 203–224] say is possible, then the chance that such a mutation might appear in more than one geographical race of man is not out of the question.

Much more serious to me personally, than Dobzhansky's discrimination in accepting statements from Rensch which he denies if I make them, is his charge, repeated many times since my book came out, that I wrote it irresponsibly, naively, or "mischievously." I wrote it without evasion or provocation and as truthfully as I was able, fully aware of the abuse that would follow, but not expecting an officious rebuke from a man of Dobzhansky's stature as a scientist.

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Restrictive Research Fund Policies

The reports referred to by Congressman Fountain [*Science* **139**, 578 (15 February 1963)] contain only one of the several types of evidence relevant to a decision to make more restrictive the regulations governing the use of funds granted for medical research by the National Institutes of Health. The Intergovernmental Relations Subcommittee has served a useful purpose in pointing out irregularities in expenditure of some of these funds, and clearly these must be corrected. The more general issue, however, requires estimates of:

1) The amount of funds that have been or are being misused. (There is no evidence that this is anything more than a very small proportion in relation to the total size of the program.)

2) The amount by which this loss can be reduced by more detailed and restrictive regulatory policies.

3) The cost of implementation of more restrictive policies.

These items would all appear to be susceptible to documentation and even quantitative estimation, and yet the issue is being joined on the basis of individual examples rather than on a logical comparison of items two and three. Provided only one side of a case is documented it is fairly easy to make a decision as to which way to go, although the decision may not be the correct one.

The financial cost of more restrictive policies does not end with the direct cost to the government of administration and enforcement programs. It includes an increased proportion of the limited scientific manpower available that will be absorbed into bookkeeping. It is my impression that the proportion of time spent by the highest paid and presumably most competent members of the scientific community in administrative and fiscal matters is already higher in this country than in any other major scientific country in the world. Every increase in this proportion occurs at the expense of the scientific effort of these individuals. Also to be reckoned are the instances in which inflexible regulatory procedures prejudice or delay research projects in such a way as to lead to increased cost—most scientists can quote examples of such situations having occurred even under the present "liberal" policies. No congressional committee has yet undertaken the documentation of the expenditures and waste involved in over-regulation of grants. It is to be hoped that somebody will undertake this before further moves are made.

The above considerations are quite independent of the more general question as to whether science flourishes better under liberal or restrictive administration. Although strong opinions are held on this subject, and it is a crucial question to be faced, the opinions are difficult to support empirically, and it does not seem to be a point that can be argued strongly.

The procedures so far instituted by the National Institutes of Health in response to the hearings of Congressman Fountain's subcommittee are irksome but not seriously incapacitating. However, the phrase "Congress is moving

in the direction of giving a closer scrutiny" implies that the goal is not yet reached. The point of diminishing returns, where the advantages of having funds available for medical research is outweighed by the time consumed in securing and administering them, may be close.

An aspect of the reports quoted by Congressman Fountain that has received inadequate recognition from scientists is the eloquent statements by the leaders of the National Institutes of Health in support of the liberal policies that they have been following. Clearly, NIH cannot support this point of view indefinitely against the desires of Congress, on whom they are, after all, dependent for funds. Scientists outside the government must also help in convincing Congress and the people that there are at least two sides to this question. The issue has come up initially with respect to support of the health sciences, but it may not stop there.

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Creativity and the Indigent Student

It is distressing to see . . . [you] give support to the archaic idea that a hungry student is a superior student [*Science* **139**, 79 (11 Jan. 1963)]. Some of the penetrating minds of the past and present may have been starved during their formative period, but to assign a cause and effect relationship is absurd. The same reasoning would suggest that we decrease by 50 percent the pay of all present scientists so that they will be twice as creative, thereby eliminating the need for a crash program.

Freed from financial pressure the "man of moderate endowment may show flashes of genius." Why dilute his academic struggles with monetary adversity?

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. . . Not all can be Enrico Fermi, but any reasonably competent Ph.D. can add to the sum of knowledge from which the Enrico Fermis draw. If recent Ph.D. theses are pedestrian, is it the fault of the Ph.D. candidate or of the professor and system under whom the work is done?

Furthermore, poverty at the graduate school level is not an automatic virtue. Probably lack of financial assistance has hindered more scholars, potential and actual, than reasonably adequate stipends could possibly do.

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. . . [the] report [of the President's Science Advisory Committee entitled "Meeting Manpower Needs in Science and Technology"] does not imply "that scientists, like nuts and bolts, are interchangeable and can be mass produced." It does imply that graduate schools will assert their traditional selectivity and accept only those students who are capable of quality academic performance; that science majors are not continuing their education because of financial difficulties; and that they can complete their programs earlier and do more creative work when devoting full time to educational pursuits than when working at odd jobs like cleaning pigeon cages.

The implementation of this document may not produce enough scientists—only because it doesn't start early enough! . . . To really increase the number of graduate students we must identify and encourage gifted youngsters in the secondary school—probably even more effectively in the elementary school. There are many studies to substantiate the fact that interest in science is "killed" or "kindled" early.

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. . . I have noticed that a relationship exists between the amount of expensive laboratory equipment and the ingenuity with which problems are solved and techniques developed. A laboratory in the early stages of growth, and short of money for equipment, develops a high proportion of new information through improvisation. As the physical plant takes on more elaborate equipment, experimental design more often is set up around the instrumentation than around the problem to be solved.

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. . . The increasing formalization of our educational processes stifles that type of creative mind that might be