Letters

Experimental Phenylketonuria

Waisman and Harlow ("Experimental phenylketonuria in infant monkeys," 12 Feb., p. 685) are to be commended for having initiated experimental studies on mental retardation which are sorely needed. The production of "mental retardation" in animals also promises to be an important tool for investigators concerned with the relations of behavior, development, and biochemistry. It is difficult, however, to accept their statement that phenylketonuria "almost identical in all respects to the clinical disease" has been produced in monkeys. Even if the biochemical criteria employed (serum phenylalanine levels and excretion of phenylpyruvic acid) are assumed to be sufficient, the demonstration of an "intellectual deficit" is questionable. The authors report that "the most outstanding behavioral finding in the phenylketonuric animals is their low motivational level during testing," and acknowledge that "low motivation played some role" in the animals' impaired performance. They nevertheless prefer to emphasize the part presumably played by an "intellectual deficit" similar to that seen in PKU.

The authors report that the experimental monkeys often rejected the milk-phenylalanine diet. It is well known that animals reduce their food intake when there is an imbalance of amino acids in the diet. This reduction is in addition to that based upon taste, which the authors acknowledge. To overcome this reduced intake, the experimenters resorted to hand-feeding, even to the point where "vomiting during hand-feeding was common in some animals." Such experience with food (and with the experimenter) by a young, developing animal, coupled with the effects of the amino acid imbalance on food-seeking behavior, is certainly not conducive to good performance on tasks where food is the reward. Since the relations between motivation and intelligence are far from clear, it is difficult to determine if their experimental monkeys mirror the intellectual deficit seen in PKU.

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That we are in essential agreement with Karrer is evident in our cautious summary conclusion: "The observed slowness in adapting to testing procedures, or even failure to adapt, and the inadequate performance *suggest* an intellectual deficit" (italics added).

In the behavioral sections of the paper, including method, results, and discussion, we deliberately emphasized the motivational problems and the negative aspects of the feeding of these subjects because we concur that one cannot establish an intellectual deficiency under conditions of inadequate testing, and testing is necessarily inadequate if motivation is low. On the other hand, we quite legitimately raised the question that motivational deficits can also stem from intellectual inability to meet the demands of a testing situation. Thus, we faced the insoluble problem presented by the objective data: To what extent were the performances low because of poor motivation, and to what extent was motivation low because of inability to perform the tasks?

The psychologist must to some extent take a clinical approach in diagnosing mental deficiency in animals, just as he must in diagnosing mental deficiency in human subjects. Thus, the clinician looks to qualitative factors to distinguish the aphasic child or the schizophrenic child from the mental retardates, although they may all show the same overall score on an intelligence test. We also looked to qualitative factors in the behavior of the experimental monkeys in this study, especially in the latter months when their physical conditions seemed stabilized and forced feeding had ceased, and when they were accepting food rewards with more consistency. We noted their general failure to avoid physical obstacles, the immaturity of their play in exposures to each other in regular play sessions, and the greater tendency to show progressive deterioration in performance of complex tasks than of simple ones. In our comparisons with normal monkeys on tasks known to be correlated with increasing age in the first 2 years of life, we tended to use control data of animals younger than the experimental subjects, and, because of the augmented number of sessions required by the experimental animals, the age discrepancies (stated in terms of age at the start of the tests) increased as the tests progressed. Our overall evaluations led us to the conviction that these animals were severely retarded intellectually, but, because of the motivational questions, we took the conservative stand in this paper and understated our case, concluding only that it looked "probable that mental retardation did play a major role in the generally depressed performance of the phenylketonuric animals on the more complicated tasks. . . ." We are currently engaged in developing tests using motivational conditions other than food reward so that we may evaluate phenylketonuric animals under less ambiguous conditions.

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Marine Biology and a New Canal

In recent months the possibility of a new canal across the isthmus region of Central America has been discussed with increasing frequency. Although no definite site has been selected from the three or four possibilities, there does seem to be agreement that the canal would not have locks—in other words, it would be a sea-level canal.

In my opinion, this aspect is of particular interest to marine biologists and oceanographers. Although the new canal might be 70 to 150 miles long, depending upon location, it seems likely that there would be mixing of Atlantic and Pacific waters within it. Land run-off would, of course, affect the resultant salinity, but, except during times of extremely heavy rainfall, estuarine conditions probably would be

maintained. As a result, this salt-water connection between two oceans would provide a wonderful opportunity for observing the interaction of previously separated marine communities.

The chance to study such a dramatic ecological adjustment must not be lost. Current reports indicate that engineering surveys may start in about a year, with actual construction getting under way about four years later. Completion of the canal might take another 10 years. However, in order to take maximum advantage of the situation, distributional surveys, taxonomic studies, and related oceanographic work should be initiated as soon as possible after the site is selected.

A scientific effort of this magnitude certainly would provide an excellent opportunity for broadening scientific cooperation among the nations of the Western Hemisphere. This, too, cannot be permitted to pass by. I hope that preliminary planning will start soon.

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The Smithsonian

It seems to me that D. S. Greenberg (12 Mar., p. 1266) unfairly depreciates the original contributions to knowledge made by the Smithsonian in earlier times. When I was elected to the National Academy I believe there was no institution in the United States which had more members in the Academy, unless possibly Harvard University.

Greenberg makes no mention of Secretary Baird, who was decorated by nearly all maritime nations for his work for the fisheries, and who was no less distinguished in herpetology and ornithology besides.

Nor did he mention Secretary Walcott, who was president of the National Academy and probably more influential with Congress and administrations than any other scientist of his time. His advice led Andrew Carnegie to endow the Carnegie Institution. He himself conceived and founded the National Advisory Committee on Aeronautics. I heard a paleontologist remark that Walcott contributed more than all others to the knowledge of Cambrian paleontology, and 75 percent of his contribution was made by his expeditions in the field while secretary of the Smithsonian. He founded the Research Corporation and promoted R. H. Goddard.

It would be improper in me to allude to scientific work of the fifth secretary. But I may at least mention Stejneger, Fewkes, Holmes (who was a leader in three fields of science and art), Hrdlicka, Merrill, Roberts, Stirling. Indeed, space forbids that I should even name those who, during my 70 years at the Institution, have added to the distinction of its scientific researches, and whose discoveries are textbook familiarities.

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The article on the current increase in research activity at the Smithsonian Institution must have aroused considerable interest in all centers of taxonomic study. It rightly points out that there are not many centers with sufficiently large collections for extensive work in systematics, although some active academic centers may be surprised to learn that "universities have pretty well dropped out of systematic biology."

The article goes on to say that the Smithsonian does not "threaten any existing institution." The established centers which are doing significant taxonomic work, both within and outside the universities, while applauding the success of the Smithsonian's new leadership, may perhaps be pardoned for wondering what the effects of this success will be on their own institutions, with their far more limited resources. All administrators should be pleased at rising salary levels, if only they can find the money to equal them, and the stimulus of a stepped-up program at the Smithsonian should enliven the field of systematics everywhere, if only the limited supply of well-trained systematists can continue to be available to all institutions traditionally engaged in this work.

Many thoughtful persons, scientists as well as administrators, may raise the question: If the federal government is to increase its support for systematics at the Smithsonian, which has direct access to Congress, will it also (for example, through the National Science Foundation) extend truly comparable support to the other long-established centers of significant systematic study?

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