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Institute for Retired Scientists

Scientists are essentially dedicated to the seeking of truth, their chief tool being their ability to think. With the onset of retirement, in most instances, the decline in mental ability does not parallel the more rapid diminution of physical capacity. Yet this fact seems to have been overlooked in compulsory retirement policies now in force at numerous institutions. Certainly these elder citizens who have pursued truth for so long could, if properly organized, constitute a source of knowledge and wisdom as yet untapped in our quest for a more efficient utilization of a potentially great "natural" resource.

Therefore, would it not be both profitable and beneficial to create an institute for retired scientists? The following thoughts occur to me in this regard: (i) that scientists monetarily subscribe early and throughout their lives towards defraying the cost of maintaining the "institute" and insuring themselves a place later on, much as they carry insurance; (ii) that laboratory, study, and, if necessary, living space be made available; and (iii) that the institute be established in a community having a mild, even climate throughout the year.

The benefits to be obtained would be numerous; not the least of them would be the creation of an atmosphere conducive to further utilization of the scientists' experience and knowledge and to reflective writing. It is also likely that younger scientists and students from all over the world might wish to work side by side with men they have heretofore only been able to read about. In addition, retired scientists at this institute might also be available to assume secretariat positions for the organization of various scientific meetings.

RALPH D. TANZ
Department of Physiology, School of Medicine, Western Reserve University, Cleveland, Ohio

Blood-Group Determinations of Ancient Tissue

In the article entitled "Blood groups of the ancient dead" [*Science* **131**, 699 (1960)], Madeleine Smith raises numerous questions. We wish to comment on the interference of environmental factors with blood-group determinations of ancient tissue. The author suggests that "analysis for rare sugar components of bacterial cell walls may show whether inhibition can be attributed to bacterial contaminants or not. . . . [This theory] would appear to offer at least a partial solution to the problem."

It is not evident from present information how rare sugars could serve as an indication of the origin of a given blood-group activity. Numerous bacteria possess polysaccharides immunologically and chemically similar to A and B substances and also to H(O) substances, as do many higher and lower animals and some higher plants. With the exception of blood-group-active substances in higher plants, there is at present no indication that blood-group activity is associated with any sugars other than those found in human blood-group mucoids. Conversely, contaminants from bacteria and higher plants containing rare sugars may be present in a tissue in large amounts without being blood-group-active.

As to the enzymatic action of microorganisms on blood-group substances, destruction of blood-group A, B, and H(O) specificity by these agents is much more common than transformation of A and B into H(O). Other blood-group substances, such as the M and N referred to by the author, are destroyed by proteolytic enzymes of animal and plant origin as well as by sialidase from bacteria and viruses.

Although the leaching process in soil is mentioned, alkalinity and acidity of the soil have a more direct effect on the stability of blood-group antigens. The serologically specific glycosidic linkages are extraordinarily acid-labile, while the hexosamines are sensitive to the action of even weak alkali.

GEORG F. SPRINGER
PETER WILLIAMSON
William Pepper Laboratory and Department of Medical Microbiology, University of Pennsylvania, Philadelphia

I am glad to know that my paper has been of interest to Springer and Williamson. Their comments are most helpful in drawing attention to some of the major problems in this work. I should like to make clear, however, that it was not suggested that rare sugars should "serve as an indication of the origin of a given blood-group activity." In the presence of a reaction which might be interpreted as evidence of the presence of blood-group antigens, analysis for the rare sugars of bacterial cell walls could show whether the presence of bacterial antigens must also be postulated. It is some added evidence that the reaction is due to the blood-group antigens if these sugars are absent, although it is not claimed that such evidence is conclusive or exhaustive. It merely constitutes an added aid in the assumption that any group-

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specific activity is blood-group activity. I hope that this will make clear that the use of this technique does not assume that blood-group activity should be associated with any sugars other than those found in human blood-group mucoids; as Springer points out, this assumption would be entirely unjustifiable.

The leaching process in soil is significant in that the blood-group substance remaining for analysis in buried bodies may only be the alcohol-soluble fraction. This elementary but important point is often ignored in the published techniques. While I am in complete agreement with the points made by Springer, I should like to draw attention to the absence of evidence on *rates* of decay of the blood-group substances in bodies buried under varying conditions and for varying periods.

MADELEINE SMITH
Serological Laboratory, British Museum (Natural History), London

The Makara in Ceylon

In your issue of 22 April [*Science* **131**, 1176 (1960)], Millard B. Rogers, in the article "An archeological pilgrimage to Santiago de Compostela," states that "the *makara* occurs in the earliest monumental sculptures of India, as early as the 3rd century B.C."

Makara scrolls are well known in the ancient ruined cities of Ceylon, chiefly Anuradhapura and Polonnaruwa, where these scrolls generally occur as the rails of the balustrades beside entrance steps to what little is left of the ancient buildings. The buildings, and hence the *makara* scrolls, can seldom be precisely dated, however. Anuradhapura was the capital of Ceylon from the 5th century B.C. to the 9th century A.D., but it is likely that its great period of sculpture began when Mahinda, son of the Indian emperor Asoka, brought the Buddhist doctrines to Ceylon about the middle of the 3rd century B.C. Polonnaruwa, which with considerable fresh Indian influence continued the Anuradhapura traditions of sculpture, was the medieval capital of Ceylon.

N. A. FORDE
1115 Stanwood Street, Philadelphia, Pennsylvania

N. A. Forde's letter to the editor concerning my article is a bit ambiguous, for it neither disproves my hypothesis nor supports it. For those concerned with Far Eastern archeology it simply repeats what we already know. There is one point, however, that should be clarified. Forde's implication that traditional dates concerning religious sites should be applied to artifacts found at the sites without supporting evidence is most unsound, and I feel that I should reply to this point.

It is true that the *makara* occurs in Ceylon and in all other countries where Buddhism and Hinduism were introduced. There is, so far as I know, no evidence that any existing sculptures in Ceylon can be assigned with certainty to Asokan times, and the earliest Sinhalese *makara* is one reproduced by D. T. Devendra [*Classical Sinhalese Sculpture* (London, 1958), Fig. 42]. There is a rich literature on Sinhalese archeology, of which the *Annual Reports of the Archaeological Survey of Ceylon* (1884 to date) form the core.

Since the tradition of the *makara* in literature predates the earliest known *makara* by several centuries, I would not be surprised if one day pre-Buddhistic *makara* were identified in India. It is less likely that any very early *makara* will ever be discovered in Ceylon.

MILLARD B. ROGERS
Seattle Art Museum, Seattle, Washington