loosa, at a cost of $82,000. Construction work will be started immediately.

THE *Journal* of the American Medical Association states that the governor of Bengal laid the foundation stone of the new School of Tropical Medicine at Calcutta on February 14. The Indian government donated $195,000 for the site and will contribute toward the upkeep of the institution. Owing to the prevalence of tropical diseases in India, the work of the laboratory will be chiefly the investigation of causes of tropical diseases in an effort to discover more accurate methods of diagnosis and more advantageous process of treatment.

STEWART S. BRUCE, formerly professor of metallurgy and ore dressing at the Michigan College of Mines, is temporarily filling the chair of metallurgy at the University of Idaho, Professor R. B. Elder having a leave of absence on account of illness.

THE research chair of medical psychology in the University of Queensland, Brisbane, has been filled by the appointment of Dr. J. P. Lowson, university demonstrator in experimental psychology at the University of Cambridge.

## DISCUSSION AND CORRESPONDENCE

### OSBORN VERSUS BATESON ON EVOLUTION

PROFESSOR H. F. OSBORN'S challenge (this *Journal,* February 24, 1922) to Professor Bateson for his position on the evolution theory, ought to and probably will, make many a biologist gasp a little and wonder a good deal.

If one scans a bit thoughtfully the landscape of human life for the last few decades, he can hardly fail to see signs that the whole battle ground of evolution will have to be fought over again; this time not so much between scientists and theologians as among scientists themselves.

The purpose of this note is to put side-by-side two sentences, one from Bateson's Toronto address, the other from Osborn's challenge. Says Bateson: "Biological science has returned to its rightful place, investigation of the structure and properties of the concrete and visible world"; and Osborn: "If this opinion [Bateson's as to the failure of studies so far made to reveal the causes of the origin of species] is generally accepted as a fact or demonstrated truth, the way is open to search the causes of evolution along other lines of inquiry."

Of the many things that fairly beg to be said about both these sentences, this seems to me the most urgent: Why have biologists felt it so much more incumbent upon them to "search the causes" of the origin of the bodies which are subject matter of their science, than astronomers, and geographers and geologists have to search the causes of origin of the bodies they study?

Or, putting much the same question in another form: What would have been the effect on the sciences of astronomy, and geography, and geology, had their devotees given relatively as much time and energy to searching for causes as have evolutional biologists?

I doubt if any one acquainted however slightly with progress in the several domains mentioned would hesitate much for answers to these queries.

Undoubtedly those who investigate the heavenly bodies are interested, and deeply interested, in the causes which produced these bodies. And undoubtedly, too, all students of the earth want to discover the "causal factors" in earth production.

I venture here to be a trifle personal. Having been for years closely connected with investigations on the oceanography of the Pacific Ocean, I am greatly interested in oceanic causation. Indeed it would be a very great satisfaction could I contribute even indirectly and in the smallest way to discovering the causes of the Pacific Ocean.

But my oceanographic feeling has always been that "investigation of the structure and properties of the concrete and visible" greatest of oceans would be more fruitful than would search after the causes of it. Possibly I am wrong, but my guess is that the attitude of the great majority of modern astronomers, geographers, and geologists, toward their domains has resembled more my attitude toward

oceanic evolution than my attitude toward biological evolution when, twenty years ago I supposed, as Professor Osborn seems still to suppose, that search for causes of this latter evolution is the supreme goal of biological study.

But I am mindful that there is a reason why biologists have been goaded to strain themselves more in search of originating causes in their domain than have other scientists in search of such causes in their domains. That reason is the historic circumstance that these other scientists have long since been relieved of danger from the germ of supernatural causation in their domains, while this germ still lingers in the biological domain.

The way by which biology may escape limbo in this matter, Bateson, along with a considerable number of naturalistic biologists, is apparently beginning to see. "Meanwhile," he says, "our faith in evolution stands unshaken."

What is the lesson, practical and theoretical, implied in such a declaration? What it is for Bateson of course I do not know. For myself it is this: *Let us stop trying to convince ourselves and others that we have discovered, or in a few minutes will discover, the causes of evolution, and devote our efforts to perceiving for ourselves and convincing others of the naturalness, through-and-through, of evolution.* In other words, *let us bestow much more time and energy upon the grounds of our faith in evolution as one of nature's grandest processes, than upon searching after, and speculating about, the causes of evolution.*

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## FURTHER CONSIDERATION OF THE SIZE OF VEIN-ISLETS OF LEAVES AS AN AGE-DETERMINANT

IN a recent paper[1] regarding vein-islet measurements as a means of determining the age of the woody perennial upon which the leaves are borne, the use of fresh leaves under low magnification was criticised. The basis of the criticism was that different thicknesses of chlorophyll would affect the number of veinlets visible and thus affect the apparent size of the vein-islets. This of course, is true. The fact that it is true constitutes one of the important advantages of the method criticised and is an equally important objection to the *sole* use of the suggested method.

In my original paper it was pointed out that the palisade cells decreased in size with age as do all of the other kinds of cells in the leaf with the exception of the cells of the veinlets which increase somewhat in size. With increasing age both the lessening thickness of the chlorophyll-containing cells and the increasing size of the conducting cells will render the veinlets more conspicuous. The actual increase in the amount of conducting tissue in the leaf is emphasized by increased visibility. The use of fresh material under low magnification gives a morphological summation which the suggested method quite lacks and therefore I adopted it after a trial of both. In this case the method adapted to field use is the more precise, as an age-determiner.

As is well known, the venation of the leaf of any given species is affected by external agents. Different species respond to these factors in different ways. Since the size of the vein-islets is affected by these factors, a successful use of this method of age determination requires sufficient familiarity with the responses of the species used, to enable one to eliminate the differences not due to age. As soon as this is done the relation of the size of the vein-islets to age is clear.

Since the discovery that the "protoplasm" of plants was fundamentally the same as the "sarcode" of animals, the progress of physiology has been steadily toward a demonstration that in the essentials of composition and response the two are essentially alike. Any theory of senility which can not be applied to plant conditions is not a fundamental theory and can be disregarded. It is equally true that any characteristic so strongly marked as

[1] Ensign, M. R., Area of vein-islets in leaves of certain plants as an age determinant, *Jour. Bot.*, 8: 433, 1921.