

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 paper¹ 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.

is the process of senility in the animal can not be unrepresented in the plant world.

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THE METRIC SYSTEM

REFERRING to the article, "Progress in Metric Standardization," by Professor Bingham in your impression for March 3, it seems impossible to make the metric party understand that while, in some applications, the adoption of the metric system is easy, in others it is supremely difficult. It has been shown repeatedly that the easiest of all units to change are those of capacity and that the easiest of all places in which to adopt the system is the scientific laboratory, and the metric argument is that, these changes being easy, therefore all others are easy, when the fact is that others are so difficult that they have not been brought about in any country in the world, the net result being a dual or mixed system to which the arguments advanced for the metric system have no application.

Your readers should obtain the recent Report of the National Industrial Conference Board on this subject, which is the result of an investigation that consumed a year and is the most exhaustive that has ever been made and which confirms *all* of our contentions. Moreover, the Report is signed, without reservation, by two members of the Council of the American Metric Association who, the facts being established, signed it because they could not do otherwise.

Within a year a committee of the Conjoint Board of Scientific Societies of Great Britain, representing forty-nine scientific societies, representing, in turn, every conceivable phase of scientific activity, have made a unanimous report recommending that the metric system be *not* adopted in Great Britain.

Moreover, these are but examples. During the past century seven investigations worthy of that name have been made in this country and Great Britain, the result of every one being adverse to the claims made for the system. The plain fact is that the metric party always lose when both sides are heard, the most recent example being at the late convention of the

Chamber of Commerce of the United States of America.

The weakness of the metric party today lies in their refusal to read the anti metric case. Because of this, their representatives went before the Senate Committee on Manufactures during the past winter at Washington and repeated claims that were disproven twenty years ago. The case was thus made extremely easy for the opposition, as we had only to point out the facts in order to show not only that the metric party had no case, but also to discredit their witnesses as incompetent.

There is no better illustration of this failure to acquaint themselves with the facts than Professor Bingham's naive assumption that the opposition is composed of "a few gage manufacturers."

It is interesting to note that, after we have been assured for many years that the system is "universal" in Chemistry, you are now inaugurating a campaign to bring about its use by chemical manufacturers.

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THIS letter is suggested by Mr. Eugene C. Bingham's article on "Progress of Metric Standardization" in the March 3 number.

I am an ardent advocate of the metric system, and feel that one of the greatest difficulties in bringing it into general use has been our prolonged period of consideration, during which many of us have been really working with two systems, and have borne all the burdens which that condition imposes.

The Drug Trade and Pharmacy generally has probably gone as far in the change as any other commercial group, but if the system were made compulsory there would be a large number of changes, some requiring an act of Congress. For example, that requiring that certain medicines bear the content of certain drugs—Opium, in grains per fluid ounce.

For the transition period certain comparative tables will be necessary—a comparative table showing prices in dollars and cents per Avoirdupois pound or ounce equals dollars and cents per kilo or grams.