At first blush this seems a reasonable enough statement, but when one inquires why it appears to be so it will be found that it is because it is suspected that physical characters are probably linked with functional ones, that there is a genetic linkage between the genes for the two different orders of phenonena. If such is the ground upon which this assumption is usually made then it ceases to be a reasonable one, for the good reason that it is based on no more than a suspicion or a "hunch" and not upon facts which are known to exist or have been demonstrated. I personally have a "hunch" that genes play an enormous part in the determination of behavior, but though I have much sympathy for Professor Strandskov's view, I see no ground for believing that there is any necessary connection between statistically aggregated physical characters and particular types of functional response.

Genetic linkage between particular physical traits and particular psychological traits is a phenomenon unknown outside folk belief.

The results of forty years of psychological testing and study of the major groups of mankind has pretty consistently followed the pattern of the results obtained by the first anthropological expedition ever to go into the field for the purpose of studying the psychological and physiological responses of such a group of mankind, the Cambridge Anthropological Expedition to the Torres Straits.[4] This expedition, under the leadership of Professor A. C. Haddon, and comprising, among others, the following members, Wm. McDougall, W. H. R. Rivers, C. S. Myers and C. G. Seligman, contrary to general expectation found no inherent psychological or physiological differences which would serve to distinguish the natives of Torres Straits from their investigators or from any other people with whom they were acquainted. Since then whenever the investigations have been conducted by unprejudiced workers (and fortunately they have been in the majority) the results have been uniformly the same. Summarizing these results for the psychological tests, Professor Otto Klineberg, after considering the evidence from every standpoint, writes, "We may state with some degree of assurance that in all probability the range of inherited capacities in two different ethnic groups is just about identical."[5]

As for the physiological differences which are said to be "inevitable" I am not aware that there is any ground for believing that these are either "many" or significant. Professor Strandskov speaks of "variations which have a physiological basis." This statement is unclear. Are these variations of a functional or are they of a structural or physical kind? Since Professor Strandskov writes that "many of these variations have been shown to be represented by different gene frequencies within different primary stocks and even within groups recognized as races" I can only take him to mean that these variations refer to structural or physical characters. If this is what Professor Strandskov means, then he is saying something very different from Professor Dice when the latter wrote that "it is recognized by anthropologists that many races also exhibit clearly marked peculiarities of physiology and psychology." As Professor Herskovits pointed out, practically all anthropologists not committed to the racist dogma hold exactly the opposite. Professor Dice's statement is therefore incorrect. He wishes upon anthropologists views which they do not hold.

Since the matter is one of the first importance it would be interesting to know what the "variations" which Professor Strandskov has in mind are. Professor Strandskov's *obiter dictum,* that if there do exist stock or group differences among mankind there is no reason for considering one group as inherently superior to the other, will strike a sympathetic chord. But to the present writer, at any rate, the evidence does not suggest, as it does to Professor Strandskov, that these differences "will in all probability be found to be numerous." My own view is the exact opposite of that.

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[3] *Ibid.,* n.s., 100: 146–147.

[4] A. C. Haddon (editor), "Reports of the Cambridge Anthropological Expedition to the Torres Straits," Cambridge, 6 vols., 1901–1935.

[5] O. Klineberg, "Mental Testing of Racial and National Groups," in "Scientific Aspects of the Race Problem," p. 284, Longmans, New York, 1942.

## THE CORRECT NAMES OF PARASITES IN HUMAN MALARIA

SABROSKY and Usinger[1] contributed an interesting article on the nomenclatorial status of human malaria parasites. After reviewing the present situation they propose, in order to regularize it, to bring the whole matter to the attention of the International Commission on Zoological Nomenclature, asking for a suspension of the rules and the official acceptation of the *de facto* names used by most parasitologists. The same action, although in slightly different grounds, was recently proposed by the writer.[2]

I almost entirely agree with the opinions held by Sabrosky and Usinger, but I don't completely support the names they propose as most adequate for the three common species of human malaria parasites; at least in the exact way they write them.

[1] W. C. Sabrosky and R. L. Usinger, SCIENCE, 100: 190–192, 1944.

[2] E. Beltrán, *Gaceta Médica de México,* 74: 61–74, 1944.

I think the name *Plasmodium vivax* (Grassi and Feletti, 1890) for the tertian parasite is perfectly right. But I don't suppose it is justifiable to call the quartan malaria parasite *Plasmodium malariae* (Feletti and Grassi, 1889, 1890). It should be a very dangerous and confusing procedure for the application of the Law of Priority to include references to more than a single dated work for a name; either their 1889 preliminary communication is the correct basis for the name, and reference to the 1890 paper must be dropped, or *vice versa*. Moreover, it seems to me that Feletti and Grassi did not clearly differentiate tertian from quartan parasites on any or both contributions. It is better to accept as the first clearly limited distinction of the quartan parasite that proposed in 1890 by Grassi and Feletti as *Haemamoeba malariae*, and call the organism accordingly *Plasmodium malariae* (Grassi and Feletti, 1890).

In regard to the parasite of malignant tertian malaria, the name proposed by Sabrosky and Usinger is *Plasmodium falciparum* Welch (1897). This form, the same employed by the Malaria Commission of the League of Nations,[3] is clearly incorrect, because Welch called this parasite *Hematozoon falciparum*, and being later transferred to the genus *Plasmodium*, the original author's name must be written in parenthesis.

Coatney and Young,[4] in a very illuminating discussion of the taxonomy of human malaria parasites, propounded the same designations here supported as the proper *de facto* names to be used.

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## THE GENERIC NAME OF THE SAND FLY

THE attention of the executive committee of the International Commission has been drawn to the communications regarding the generic name of the sand fly by Dr. William F. Rapp, Jr., which appeared in the issues of SCIENCE for April 28 and August 11, last, and by Dr. Charles T. Brues in the issue for May 26, last.

The proposed abandonment of the emended spelling *Phlebotomus* Agassiz, 1842, in favor of the original spelling *Flebotomus* used by Rondani when he first published this name in 1840, affects not only workers in systematic zoology but also—and perhaps especially—workers in the medical field in view of the enormous literature regarding the role played by this fly in the spread of disease. It is clearly of great importance that, in order to prevent confusion from arising, the correct spelling of this generic name should be settled as soon as possible. In view of the fact that the issue involved turns upon the interpretation of Article 19 of the International Code of Zoological Nomenclature, it appears to the executive committee that this is a matter which should be referred for decision to the International Commission on Zoological Nomenclature, as the authority officially charged with the duty of interpreting the application of the International Code in cases of difficulty. Communications in regard to this matter should be addressed to the International Commission on Zoological Nomenclature at their Publications Office, 41, Queen's Gate, London, S.W.7.

FRANCIS HEMMING,
*Secretary, International Commission Zoological Nomenclature*

# SCIENTIFIC BOOKS

## SPECTROSCOPY

*Experimental Spectroscopy.* By RALPH A. SAWYER. viii + 323 pp. 107 figs. 16 × 23½ cm. New York: Prentice-Hall, Inc. 1944. $3.75.

THE author states in the preface that "The purpose of this book is to discuss prism and grating spectrographs and the techniques of their use in research. It is designed for students of spectroscopy and for those in research laboratories who wish to make use of spectroscopic procedures. For this reason, extensive mathematical treatments have been avoided; a background of general physics and some physical optics should be sufficient for an understanding of the presentation."

Successive chapters and pages of the book deal with (1) "The History of Spectroscopy," 1–17; (2) "Light Sources," 18–27; (3) "Spectroscopic Apparatus—General Principles," 28–46; (4) "Prism Spectroscopes and Spectrographs: Theory and Construction," 47–83; (5) "Prism Spectroscopes and Spectrographs: Types and Use," 84–120; (6) "The Diffraction Grating: Theory and Production," 121–144; (7) "The Diffraction Grating: Mountings and Use," 145–182; (8) "The Photographic Process," 183–204; (9) "The Determination of Wavelength," 205–243; (10) "The Determination of Spectral Intensity," 244–276; (11) "Apparatus and Methods of Infrared Spectroscopy," 277–287; (12) "The Spectroscopy of the Vacuum Ultraviolet," 288–295; (13) "Spectrochemical Analysis," 296–310.

The need for a book of this kind has been growing for two decades during which extraordinary develop-

[3] Comité rapporteur de la Commission du paludisme, *Bull. Org. d'Hyg.*, 9: 139–262, 1940.
[4] G. R. Coatney and M. D. Young, *Publication No. 15 of the A. A. A. S.*, pp. 19–24, 1941.