THE OCCURRENCE OF OPISTOMUM PALLIDIUM O. SCHM. IN THE UNITED STATES

WARD and Whipple (18), in their "Fresh-water Biology," indicated that the genus *Opistomum* of the family *Dalyellidae* was not found in the United States. Von Graff (11) records new species of American *Dalyellia* but has not recorded *Opistomum*. Higley (18) did not find *Opistomum* among the rhabdocoeles of the Mississippi Valley. I have not been able to find any later reference to the *Dalyellidae* of the United States. It seems worth while, therefore, to record that *Opistomum* has been found in the vicinity of the University of Virginia. I have found four or five specimens in each of three collections made early in the spring of 1927. About the last of May the specimens were abundant in all collections made.

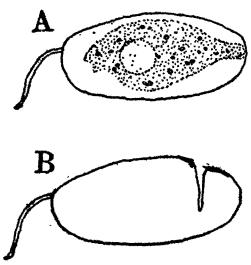

FIG. 1. *A*, camera lucida sketch of freshly laid egg. The egg was fixed to substratum by stalk shown at left end of shell; *B*, empty egg shell showing cleft through which young specimen had emerged. ×100.

All these specimens closely corresponded with Von Graff's (1882) description of the tribe *Opistomini*, a translation of which follows:

Dalyellidae with mouth and sexual opening in last third of body. The pharynx, arising from ventral surface of the intestine, is a long cylindrical tube with point directed backward. Two main branches of the excretory organ. A single median renal pore opening between mouth and genital pore. Vitellaria are not branched. Without bursa copulatrix. Independent uterus present. Rhabdites lacking. Chitinous structures of male copulatory organ furnished with numerous small needles of ductus ejaculatorum. One genus with character of tribe *Opistomum* (O. Schmidt). One certain species four and one half millimeters long from Europe; one uncertain species one and one half millimeters long from Australia.

The Australian species has eye-spots. My specimen lacked eye-spots. Moreover, it corresponded in all details with Von Graff's description of the species *Opistomum pallidium* (O. Schmidt). The specimens found in this locality represent this species.

In addition to recording this genus for the American fauna I am able to record that the specimens laid eggs in the laboratory aquarium during the latter part of April, 1927. The specimens would also lay eggs in depression slides whether isolated or in groups as great as five individuals to a depression. The egg was in each case deposited in the shallow marginal water of the depression slide. Each egg was fixed to the substratum by means of a slender curved stalk. Further, the eggs, thus deposited in depression slides, would develop. Data were kept for one of these eggs. April 23, 1927, two eggs were laid. Developmental changes were observed daily until, on April 28, a small *Opistomum* had left the egg through a subterminal cleft (Fig. 1-B). No attempt was made to rear the young individuals.

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THE WORD CARIBOU

STUDIES relating to the historical origin of words may be quite as interesting and oftentimes more profitable than monographic reviews dealing in wearisome detail with the phyletic development of some organism concerning which the most of us know little and care less. Very recently in reading the journal of that intrepid explorer, Sir Alexander Mackenzie, describing a canoe voyage from Lake Athabasca to the Arctic Ocean down the great river now bearing his name[1] I was impressed by a paragraph and the possible significance it might have in throwing light upon the origin of the word "caribou." This is perfunctorily recorded in various dictionaries as being either of French Canadian[2] or of Indian[3] origin without further comment, although some editions of Webster state that it is derived from the Indian word meaning "pawer" without noting the specific word.

Mackenzie, while making the trip in 1787, writes in his journal as follows (p. 209): "We saw some rein-deer on one of the islands (Great Slave Lake) and our hunters killed five and two smaller ones . . . This island was accordingly named Isle de Carreboeuf." While it is not clear from the context as to the size a reindeer must attain before possessing a numerical value, the implication that "caribou" is derived from *carre* signifying four, and *boeuf* indicating bovine or ox, is fully apparent and seems to have escaped the attention of lexicographers. In the reindeer horns are possessed by both sexes and the number four may well refer to the fact that each horn has a large horizontal as well as perpendicular prong, so that there are four principal prongs. Some editions of Webster give "carribou" as a variant spelling.

[1] "Voyages from Montreal through the Countries of North America," London, 1801, Allerton edition.

[2] Webster's Dictionary—some editions; Standard Dictionary; Lettré, Dictionnaire Langue Français.

[3] Murray, New English Dictionary; Webster's Dictionary; Century Dictionary; Hatzfeld, Dictionnaire Langue Français.

It is of course not impossible that we are dealing with a word originating in a tribe of northern Indians and that Mackenzie made an error in suggesting its French origin. The Algonquin Indians as well as their relatives of the Great Slave and Athabasca regions, however, use the word "Atick," while the Esquimaux use the word "tuktu" for reindeer. Even should the word "caribou" appear in some of the Indian dialects, one would still be inclined to suspect its French paternity through contact with early traders.

Among the historical papers of the seventeenth or the latter part of the eighteenth centuries, particularly those relating to the fur trade, references should be found which will remove any existing doubts. In the meantime we may assume the following derivation: Caribou < carribou < Fr. *carre* < *quarre* < Lat. *quadri* = *quattour,* four + Fr. *boeuf* < Lat. *bos* < (*bov*) < Gk. *boûs,* ox, the four-horned ox.

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"HABIT FORMATION"

THE note by Professor Wakeham[1] on this subject has such an important bearing on some current doctrines of psychologists that it may not be out of place to call attention to some implications involved. Professor Wakeham tells us that he "practised this passage, slowly and carefully, ten times daily, purposely putting in the wrong notes, for two weeks." The experiment very clearly demonstrates the fact that our organisms readily acquire certain habits of action which are followed without any thought on our part and that these habits dominate our conduct in very complicated cases. Most of us will be ready to admit that a very large proportion of our ordinary conduct is dominated in this manner and that this conduct is connected with some physical property of the organism.

But how does it happen that, in spite of these habits, Professor Wakeham was able to "put in the wrong notes"? The older psychologists would have said without hesitation that it is an illustration of conscious purpose. But "consciousness" and "purpose" are anathema to many modern psychologists. What words can be used in place of these to designate this phenomenon, which is so vitally important in our conduct—the ability we certainly have to do something contrary to our usual habit? It certainly seems difficult to account for it on the physical basis which we use to account for habits, for it is not habit at all.

Many modern psychologists deny the validity of introspection as a method of studying our mental conduct. How otherwise than by introspection does Professor Wakeham know that he tried the experiment he describes? Shall we deny the validity of the experiment because introspection was a part of it? In rejecting introspection are not psychologists rejecting one of their most important tools?

When Professor Wakeham tells us that he "put in wrong notes," the question immediately arises, why were they "wrong"? Were they wrong because they departed from the notes written by the author of the musical composition he was following or wrong because they were in violation of fundamental principles of musical harmony? To put it differently, were they wrong because of their departure from authority or because of a departure from the custom of musicians, or still again, because they were inherently objectionable to an untrained musical ear? But this would lead us far afield into questions of right and wrong in relation to conventions of society and in relation to the effect of conduct on the individual and on society, independently of conventions, if such a thing is ever possible.

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[1] G. Wakeham, SCIENCE, Aug. 10, p. 135.

QUOTATIONS

MUSEUMS AND GALLERIES

THE publication of an interim report by the Royal Commission on National Museums and Galleries will come upon most people with a little thrill of surprise. After sitting and taking evidence for twelve months, the commission has something so urgent to say that it can not wait till its final report is ready. The discontent which led to the appointment of the commission had long been brewing; but there is no need to go farther back than January of last year to find sufficient cause. In that month two important letters appeared in these columns. Lord Northbourne, writing as chairman of the Sudeley Committee, pointed out how greatly the British Museum and the National Gallery were hampered in their work for general education by lack of means, and Dr. Stanley Gardiner, writing for zoologists in general, complained that, for want of space, the Natural History Museum was losing golden opportunities of service to the health of man and the fertility of the earth. Dr. Gardiner's letter drew a sharp retort from Lord Cushendun, then Mr. Ronald McNeill, and financial secretary to the treasury, and others whose duty or hobby was national economy were not slow to protest against any suggestion of increased expenditure upon museums and galleries. The issue was joined. In July the commission was appointed, and among its terms of reference was this: