hydrolyzed by "true," but not by "pseudo," cholinesterase, and benzoylcholine was split by the "pseudo," but not by the "true," enzyme. These specificities were proposed as a basis for the separate estimation of the two enzymes. No reference was made in this publication to the previous work of Alles and Hawes[1] on the differences in the enzymatic effect of blood cells and serum on acetyl-β-methylcholine.

(4) The controversy in SCIENCE began with the restatement by Mendel and Rudney,[5] on the basis of their findings alone and without reference to the work of Alles and Hawes, that separate "true" and "pseudo" cholinesterases exist.

(5) A claim of priority for Alles and Hawes as the discoverers of two distinct enzymes capable of effecting the hydrolysis of acetylcholine was submitted by de Laubenfels,[6] who also referred to the term "pseudo-cholinesterase" as an unfortunate designation, since most of the work already in the literature on the enzymatic scission of choline esters dealt with the activity of the "pseudo" enzyme but had always been called simply cholinesterase.

(6) Mendel and Rudney[7] countered with the statement that Alles and Hawes were not aware of the existence of a specific and a non-specific enzyme, and emphasized that the serum, that Alles and Hawes found possesses enzyme properties different from those of the blood cells, actually contains both types of cholinesterase. Mendel and Rudney then defended their term "pseudo-cholinesterase" on the ground that the "pseudo" emphasizes non-specificity; they referred to their previous suggestion that the term be provisional until the physiological function of the enzyme is established.

(7) Alles and Hawes[8] supported de Laubenfels in regard to the use of "pseudo-cholinesterase," and reaffirmed their priority for the discovery of the two enzymes.

(8) A final review of the situation was given by Mendel and Rudney,[9] in which they pointed out that the view of Alles and Hawes, that the two types of cholinesterase exist separately in human serum and blood cells, must be modified in the light of later findings showing that the serum contains a small proportion of the "cell enzyme," and furthermore biological localization of these enzymes varies from one species to the next. Mendel and Rudney then claimed that it was only their own work on specificity with purified preparations that furnished the proof of the existence of two enzymes. Finally they again repeated the reason for their innovation in nomenclature.

5 B. Mendel and H. Rudney, SCIENCE, 98: 201, 1943.
6 M. W. de Laubenfels, SCIENCE, 98: 450, 1943.
7 B. Mendel and H. Rudney, SCIENCE, 99: 37, 1944.
8 G. A. Alles and R. C. Hawes, SCIENCE, 100: 75, 1944.
9 B. Mendel and H. Rudney, SCIENCE, 100: 499, 1944.

From the foregoing recapitulation it is clear that, if one accepts the evidence reported thus far as proof for the existence of two separate cholinesterases, Alles and Hawes deserve the priority for the initial discovery which Mendel and Rudney confirmed and considerably extended. The possibility should be kept in mind that the specificities observed for cholinesterases may still be found to result not from the enzyme itself, but rather from other factors or concomitant substances associated with the enzyme. But in regard to the specificity as it stands to-day, it was Alles and Hawes who first demonstrated the different actions of two enzyme preparations on acetyl-β-methylcholine. The fact that one of the preparations, human serum, was shown subsequently to contain a small proportion of the other enzyme factor in no way detracts from their use of this substrate in contributing toward the enzyme differentiation. It was only natural for Alles and Hawes in 1939, when the first information was coming to light, to refer to the two factors as blood cell enzyme and serum enzyme as a matter of convenience, but they did not advocate that the names "'cell-cholinesterase" and "serum-cholinesterase" be adopted as official designations, and they used differences in properties, rather than locale, as their criteria.

If any one encountering the term "pseudo-cholinesterase" were to understand that "pseudo-" was meant to indicate non-specific, there would be no difficulty. However by definition "pseudo-" means false, and many might logically puzzle themselves with the question, "Just what is a pseudo-enzyme?" In truth, the writer has yet to speak to a single enzyme chemist who favors the term "pseudo-cholinesterase." However, though the undesirability of the term is apparent and it should be dropped from the literature, it is difficult to find one entirely adequate. With full knowledge of their shortcomings, the terms, specific and non-specific cholinesterase, might suffice until more knowledge is available; at least their connotation is less undesirable. In fact, these terms have been actually employed at times by Mendel and Rudney.

DAVID GLICK

RESEARCH LABORATORIES,
RUSSELL-MILLER MILLING CO.,
MINNEAPOLIS, MINN.

THE GENETIC DESIGNATION OF "STRAIN" IN BACTERIOLOGY

THE recent article in SCIENCE on "The Concept of a 'Strain' in Bacteriology," by George H. Chapman,[1] leaves much to be desired. One may well question the statement "Because of the strong dissociative tendency among many bacteria which tends to produce distinctly different daughter races from apparently

1 SCIENCE, 101: 429–430, 1945.

homogeneous parent cultures, transplants of such colonies are frequently considered as separate 'strains,'" because its major premise is that the parent cultures are apparently homogeneous. Most bacteriologists use the term "strain" for any independent culture, although various of these cultures or strains might prove to be apparently identical and belong in one "type" or "variety."

When dissociation occurs it may be of two kinds, "phenotypic" or temporary and "genotypic" or permanent. Still another occurrence is that of loss of virulence by pathogenic species in which the cultural and physiological characteristics may remain essentially unchanged. This poses the question: Is there a reliable method for accurately determining when a bacterial culture becomes genetically unrelated to its parent or sister cultures to enable one to designate the progeny as cultures or strains? The reservation of the designation of "strain" for the "offspring of a single 'pure' culture or better still, of a single cell" is a restricted form of definition because it leaves out of account the fact that all cultures are the progeny of single colonies or cells, pure or mixed, even though they are not designated and known as such. Several years ago the writer[2] discussed the pure culture concept in relation to microorganisms, pointing out the range in its interpretation by different investigators. The suggestion that strains "should only be considered as such when it is known that they are genetically unrelated" is an order quite out of reach and keeping with present methods and knowledge.

It is probable that most bacteriologists would be confused by the genetic appellation being considered as basic to the use of the term "strain" in bacteriology. After all is said and done, the terminology all scientists should be striving for is one that describes but does not confuse the scientist or layman of this or some related science. Just as the social sciences are jargon-ridden to their serious detriment, so also are some of the biological sciences cultivating confusion rather than understanding as fads come and go or grow.

E. M. HILDEBRAND

DUNEDIN, FLA.

EXPERIMENTAL TUMORS IN AN INSECT

AMONG the conditions which bring about the development of tumors such factors as hormones, nutrition, carcinogenic substances and others are currently studied by many workers in vertebrates, particularly in mammals. This preliminary note concerns an experimental animal not commonly used in tumor research, namely an insect (*Leucophaea maderae*, Orthoptera), and a factor, not usually considered as playing a role in tumorous growth, *i.e.*, innervation.

[2] E. M. Hildebrand, *Bot. Rev.*, 4: 627–664, 1938.

Innervation as a factor in the origin of tumors was studied by cutting the recurrent nerve at various levels. As in other insects this nerve, together with several sympathetic ganglia, represents the stomatogastric nervous system. The branches of the recurrent, which innervate the anterior portion of the alimentary canal as well as the salivary glands and their reservoir, were demonstrated in methylene blue preparations. When the recurrent nerve was cut tumors developed within ten days to several months after the operation in organs innervated by the recurrent nerve, *i.e.*, in the salivary glands, the salivary reservoir and the anterior gut. To date about 250 specimens with experimental tumors were obtained in this way. The tumors which may attain considerable sizes were verified by dissection of the animals, and many of them were cut for histological study.

Histologically the tumors consist of layers of cells which show various degrees of abnormality. In advanced stages the cells near the lumen of the organ, for instance, of the mid-gut, frequently break down into a brownish debris. The anterior portion of the mid-gut is a common site of these tumors. They are also frequently found in the wall of the salivary reservoir where they are particularly conspicuous because normally the wall is a very thin and transparent membrane. In the fore-gut and in the salivary glands well-developed tumors are relatively rare.

Several hundred animals were operated upon in various other ways (allatectomy, castration, etc.), care being taken not to disturb the recurrent nerve. These control operations did not cause the development of tumors. A more detailed report, to be published elsewhere, is in preparation.

BERTA SCHARRER

SCHOOL OF MEDICINE,
WESTERN RESERVE UNIVERSITY

THE SHORTAGE OF SCIENTIFIC PERSONNEL

I HAVE read with great interest the series of discussions and articles in SCIENCE relating to the shortage of trained scientists in this country. As a professional scientist (zoology, general physiology) the matter is of personal concern to me.

However, I have noticed that all the writers, who bewail the future results of the shortage, fail to consider one factor: the large number of highly trained scientists (Ph.D.'s) who are temporarily in the Armed Forces. The vast majority of these are anxious to return to a normal civilian position as soon as possible. They should be carefully considered whenever one discusses the dearth of scientists.

As a first-hand example, may I take my present occupation in aviation physiology with the Army Air Forces? There are well over a hundred aviation