Sinox (in kerosene) killed only the younger leaves and flower spikes. Axillary buds developed and young flower shoots were evident at the time of frost.

Kerosene alone killed the younger leaves of ragweed and any flowers that were ready to open at the time of spraying, but the effect was temporary, and the plants recovered within a short time and continued to release pollen.

DISCUSSION

These results indicate that it is possible to stop pollen production in ragweed with chemical sprays. Greenhouse tests and some field observations, however, indicate that most of the materials used in this series of tests are more or less toxic to cultivated crops at the concentrations used in these experiments. Very few crops will tolerate kerosene, and thus the method of weed control described would be of little value in areas where crops are being grown.

Recent reports of work with growth-regulating substances[3, 4] have shown that ragweed, as well as many other annual weeds, can be killed by spraying with "2–4–D" and other similar materials. Limited greenhouse tests, conducted during the winter, have shown that low concentrations of these substances have a very pronounced effect on ragweed. When young ragweed plants, six inches tall, were sprayed, growth of terminal buds was stopped. No further elongation occurred during the observation period of two months, nor were there any flower spikes evident. Leaves present at the time when sprayed remained green, and the older parts of the stem appeared normal.

Confirmation of these results will be sought as soon as plants are available under natural conditions. Details of concentration of "2–4–D" and time of application remain to be explored, but the information already available indicates that it will be possible to develop a program of treatment that will prevent the production of pollen by common ragweed without having the undesirable features of complete destruction of vegetation.

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[3] C. L. Hamner and H. B. Tukey, *Bot. Gaz.*, 106: 232–245, 1944.

[4] P. C. Marth and J. W. Mitchell, *Bot. Gaz.*, 106: 224–232, 1944.

DISCUSSION

THE CONTROVERSY ON CHOLINESTERASES

FOR about two years a controversy, in the form of articles and counter articles in SCIENCE, has continued between Mendel and Rudney and Alles and Hawes concerning claims of priority for the discovery of two separate enzymes capable of hydrolyzing choline esters on the one hand, and the use of the term "pseudo-cholinesterase" on the other. The former point should be resolved simply by referring to the facts, and the latter should be considered without delay since, if the term is disadvantageous, it ought to be dropped from the scientific literature as soon as possible. The writer, as a completely disinterested individual, relative to sides in the matter, has undertaken this objective discussion in the hope that it might help to clarify the controversial issues.

It would appear that the significant facts are as follows:

(1) Alles and Hawes[1] were the first to point out, by experiments on human blood, that two apparently distinct enzymes exist that are capable of hydrolyzing acetylcholine. They arrived at this conclusion, which was reiterated by Hawes and Alles,[2] on the basis of differences in the enzymatic properties of the blood cells and serum as regards activity-pH, activity-sodium chloride and activity-substrate concentration relationships, as well as the differences effected by introducing methyl groups into the choline portion of the substrate esters. These investigators found that, whereas serum exhibits only a slight enzymatic hydrolysis of acetyl-β-methylcholine, the cells produce an enzymatic scission at a rate which is of the same order as that of acetylcholine.

(2) Mendel and Rudney[3] observed that purified preparations derived from serum and certain tissues exhibited enzymatic hydrolysis of acetylcholine, tributyrin and methyl butyrate, while those obtained from brain tissue and the red blood cells of some mammals possessed demonstrable activity only on the acetylcholine. These workers concluded that a non-specific enzyme, for which they proposed the name "pseudocholinesterase," was present in the former case and a specific cholinesterase in the latter.

(3) In a later communication Mendel, Mundell and Rudney[4] reported that acetyl-β-methylcholine was

[1] G. A. Alles and R. C. Hawes, *Jour. Biol. Chem.*, 133: 375, 1940.

[2] R. C. Hawes and G. A. Alles, *Jour. Lab. Clin. Med.*, 26: 845, 1941.

[3] B. Mendel and H. Rudney, *Biochem. Jour.*, 37: 59, 1943.

[4] B. Mendel and D. B. Mundell and H. Rudney, *Biochem. Jour.*, 37: 473, 1943.

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.

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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.