Sir Charles S. Sherrington, who has recently retired from the presidency of the Royal Society, the highest scientific distinction in the world. He writes:

I find three instances in which I am by name and deed held up to public abhorrence. From each of the three statements made about me, the employment of anesthesia in my experiment is studiously omitted, although expressly mentioned in each of the published papers. . . . In two out of the three statements I am accredited with inflicting upon living animals and without the employment of anesthetics, a dissection and procedure that I pursued only upon animals already dead.

No further comment is necessary as to such misrepresentation.

II. MRS. WHITE

In the United States Mrs. Caroline Earle White played as prominent a rôle as did Miss Cobbe in England. She founded, and until her death was the president of the American Anti-Vivisection Society in Philadelphia and was editor of the *Journal of Zoophily,* now called *The Starry Cross.*

Nearly half a century ago Mrs. White called upon me in order to convert me to her views. We discussed vivisection in my office for about two hours. When we parted I said to her as nearly as I can recall:

I want my position to be clearly understood by you. I deem it a professional, a moral, and a Christian duty to thwart your efforts to prevent experimental research, because that is the most valuable and effective weapon in our warfare against disease in man and animals. The Anti-Vivisectionists are, in my opinion, the active enemies of animals as well as of human beings.

Nevertheless, we parted amicably and remained in friendly, though not intimate, personal relations until her death. I have always had a real appreciation of her fine personal character apart from this one blot, which was an obsession.

Some years ago I pointed out that she clearly advocated human vivisection. I quote now from my book on "Animal Experimentation and Medical Progress."[2]

On March 11, 1885, I gave the address at the Commencement of the Woman's Medical College of Pennsylvania. In this address I emphasized the terrible fact that about twenty thousand people were killed annually in India by snakes, especially the cobra. In reply to this address Mrs. White published an "Answer" and on page 10 of this "Answer" is found the following flat-footed advocacy of human vivisection.

"Dr. Keen mentions that in India alone twenty thousand human beings die annually from snake bites and as yet no antidote has been discovered. How can we search intelligently for an antidote, he says, until we know accurately the effects of the poison? I should reply that in order to find out the effects of the poison and to search also for an antidote, the best plan would be for the experimenters to go to India where they could find as large a field for investigation as they require *in the poor victims themselves. Here is an opportunity such as is not often offered for experimenting upon human beings,* since as they would invariably die from the snake bites, *there can be no objection to trying upon them every variety of antidote that can be discovered.* Nothing seems to me less defensible than these experiments on the poison of snake bites upon animals, since it is the one case in which they could be observed *with so much satisfaction and certainty upon man!*

"Such a proposal [I said] is as absurd as it is cruel. Even if the experimenter could afford sufficient time and money to go to India for months . . . how could he arrange to be present when such unexpected accidents occurred [for the cobra has no warning rattle]? How could he have at hand in the jungle, the ether, chemicals, assistants, tables, tents, food and drink, and the necessary yet intricate and delicate instruments? And even if he had all of these, how could he work with the calmness and the orderly deliberation of the laboratory when a fellow human being's life was ebbing away and every minute counted in such a swift poison?"

This is so swift that usually a patient dies in a few minutes. Even were a hospital and all appliances at hand the patient would ordinarily die before careful, painstaking investigation could possibly be made.

Of course this caused an angry protest from the anti-vivisectionists. But no one could more clearly and calmly advocate human vivisection than did Mrs. White in this quotation. Moreover after twenty-seven years of reflection Mrs. White said, "It does not seem to me that this is a cruel suggestion [*i.e.,* experimenting on human beings bitten by snakes] as my only object in it was to benefit the poor natives who die by the thousand every year."[3]

Again further comment is needless.

W. W. KEEN

PHILADELPHIA, PA.

HELIUM IN DEEP DIVING

IN view of the reported success obtained by the use of mixtures of helium and oxygen, substituted for air in deep diving and other high pressure work, whereby it is reported by the Bureau of Mines that not only is much greater speed of recovery from exposure to

[2] Houghton Mifflin Company, Boston, Mass., pp. 251–253.

[3] *Boston Medical and Surgical Journal,* July 25, 1912, p. 143.

the helium-oxygen atmosphere attained, with freedom from that painful and dangerous disability, "the bends," than where air is used, but that greater depths of diving are made possible without danger to the divers, it may be interesting to make note of the source of the original suggestion for this use of helium, since no other acknowledgment has been forthcoming.

I may premise by saying that very early in my career while teaching chemistry in the Central High School of Philadelphia, I carried on a series of experimental investigations concerning the inhalation of gases and wrote a paper under the title, "Inhalation of Nitrous Oxide, Nitrogen, Hydrogen and other Gases and Gaseous Mixtures." This was published in the *Medical Times* in Philadelphia, November 15, 1873, or fifty-three years ago. It was almost my first scientific paper and the results detailed in it were pioneer in their nature.

It was natural, therefore, that when in 1919 I found that it was reported or at least claimed that helium was being obtained in large quantities my mind should revert to the early work and especially to the results of breathing mixtures of hydrogen and oxygen, perfect for breathing, but highly dangerous as an explosive mixture. I realized at once that by substituting helium for hydrogen similar advantages as to rapid diffusion might be obtained and that the mixture would be innocuous and non-inflammable.

Interested as I was, I wrote to Dr. W. R. Whitney, of the Research Laboratory, General Electric Company, Schenectady, N. Y., the following letter, which, as will be seen, embodies the idea of using helium and outlines the advantages which would be expected from its use.

August 19, 1919.

Dear Dr. Whitney:

As you probably well know there is a limit to the pressure which men or animals can sustain in a caisson bridge-piers, salvaging sunken vessels, etc. This is usually put at about 65 lbs. to the square inch corresponding to something less than 200 ft. of water. It occurs to me that perhaps an extension of this limit might be obtained by producing an atmosphere containing helium instead of nitrogen, and rearranging as it were the proportion of oxygen with the helium to supply the needs of the animal body for oxygen. The idea is based upon the principle of the superior rapidity of diffusion of the low density gas and if the atmosphere under high pressure contains too much oxygen with the proportion of 1 to 4 as in ordinary air, air itself might possibly be diluted with helium giving somewhat the same result as an artificial atmosphere with helium and oxygen only. Inasmuch as our war development has called attention to the production of helium on a large scale, the idea does not seem to be a very far-fetched one as compared with its status when helium was a very rare gas, not to be obtained in any quantity.

I would like to know what you think of this general idea and whether you can pick a flaw in it. Its actual value would have to be based upon experimental tests on animals.

Very truly yours,
(Signed) ELIHU THOMSON

With all this in mind, I wrote to the Bureau of Mines, in the hope of getting some helium to make the necessary experiments, but did not obtain any.

In the *Chemical News* of December 19, 1919, was a note from Professor J. C. McLennan, of the physics department of the University of Toronto, stating that he was interested in finding uses for "helium" outside of balloon inflation. This seemed to indicate that after all there *was* a surplus of helium. I then wrote to Dr. McLennan as follows:

January 14, 1920.

Dear Sir:

I notice in the issue of *Chemical News*, December 19, 1919, under a note entitled "Helium" it is stated that you have been interested in finding uses for helium outside of balloon inflation. This prompts me to write you saying that I had communicated with our Bureau of Mines in relation to a use for helium based on the idea that after the war there would be a considerable stock of the gas on hand which could be obtained for distribution, if desired. I found that this was not, indeed, the fact; and practically no pure helium (or nearly pure helium) had been produced from the Oklahoma or Texas gas, as I had been given to understand had been done from the various publications. My idea in relation to the matter is on account of its very high diffusive power, as compared with nitrogen, it might be possible to make a mixture of helium and oxygen to be supplied to divers or others working under high pressure, the amount of oxygen being determined by experiments which, when mixed with helium, should allow respiration and much greater freedom in the removal of effete gases from the lungs, the gas interchanged and speeded up from two to three times. Only experiments could determine the value of this suggestion, if it has any; and, of course, it is based on being able to obtain helium fairly free from admixtures and in sufficient quantity to be supplied to a diver. Inasmuch as salvage and caisson work is limited at present to perhaps about 150 feet or a little more, it is thought that by substituting helium for nitrogen in the air breathed this depth might be say increased 50 per cent. or more.

Very truly yours,
(Signed) ELIHU THOMSON

Evidently there was foreshadowed in this letter an important application for helium, an application which if successful must lead to extension of depth in diving and minimizing of the painful results ac-

companying such work. Five years went by when publications of accounts of experiments eminently successful, as carried out by R. R. Sayres, W. P. Yant and J. H. Hildebrand, of the Bureau of Mines, were described.

I then wrote Dr. McLennan, under date of July 31, 1925, as follows:

Dear Professor McLennan:

You will probably recall that on January 14, 1920, I wrote you in response to a request for suggestions as to possible uses for helium, and outlined a possible set of advantages which it might have in working under high pressures, as in caissons or deep sea work. This was about five years ago, or more. I now find that under Serial No. 2670, the Bureau of Mines sends out a report dated February 20, 1925, in which experiments have been made in the line of my suggestion, by R. R. Sayres, W. P. Yant and J. H. Hildebrand. This is somewhat surprising to me in face of the fact that I tried to get from the Bureau of Mines a supply of helium for just such an investigation, but without any success. It surprises me because there is not the least reference to where the idea originally came from, and I would ask you if you know whether the suggestion, as sent to you, was forwarded to the Bureau of Mines in a letter or otherwise.

Awaiting your reply, I am

Very truly yours,

(Signed) ELIHU THOMSON

P. S. As my first scientific paper of fifty years ago or more dealt with the "Inhalation of Gases and Mixtures of Gases," in which field I carried on quite a number of experiments, and which paper was published in the Philadelphia *Medical Times* of March 15, 1873, it is natural that I should have a decided interest in this matter, and that my attention should turn to the possibilities of helium in this connection when such gas was available.

E. T.

and sent the following to the Bureau of Mines:

July 31, 1925.

Dear Sir:

In looking over the report Serial No. 2670 just received, I would like to call attention to the fact that on January 14, 1920, and in answer to a request for suggestions as to the new uses for helium, I wrote Professor McLennan suggesting the helium-air mixture for caisson work. I also, at the same time, made application to the Bureau of Mines for helium and intended to carry out experiments of this kind if a supply was available.

It seems to me that under the circumstances some acknowledgment is due as to the priority in the suggestion of this helium-oxygen mixture, on which I am glad to see experiments have been carried out with such signal success.

Very truly yours,

(Signed) ELIHU THOMSON

Encs.

P. S. As my first scientific paper of fifty years ago or more dealt with the "Inhalation of Gases and Mixtures of Gases," in which field I carried on quite a number of experiments, and which paper was published in the Philadelphia *Medical Times* of March 15, 1873, it is natural that I should have a decided interest in this matter, and that my attention should turn to the possibilities of helium in this connection when such gas was available.

E. T.

To the former I received a gracious acknowledgment and statement that he had called attention to my suggestion in *Nature* some years before.

To the letter addressed to the Bureau of Mines nothing definite as an answer has been received though about a year and a half has passed by. From recent reports, however, we learn that the helium-oxygen mixture has been used for more extended or deeper diving than before and that the sunken submarine, S–51, was successfully brought to the surface by divers supplied with the helium-oxygen atmosphere.

The moral to be drawn from all this is: If you have a good idea, publish it at once, or patent it, or both, in which case it is not so easy for the other fellow coming along years later to adopt it without giving credit where credit is due.

ELIHU THOMSON

SWAMPSCOTT, MASSACHUSETTS

ABOUT THE ACCUSATION OF PLAGIARISM OF THE LATE DIRECTOR OF THE PULKOVO OBSERVATORY, OTTO STRUVE

IN the December number of the *Journal of the Royal Astronomical Society of Canada* there appeared a biographical article by Mr. A. F. Miller, entitled "Camille Flammarion, his Life and his Work," in which the author accuses the late director of the Pulkovo Observatory, Otto Struve, of intentionally assigning to himself another's discovery, the accusation being based on a suspicion once expressed by Flammarion. Messrs. George and Otto Struve published in the March, 1926, number of the same journal, an answer, in which they completely refuted the accusation on the basis of some documents available and fixed the true state of matters.

As Mr. Miller nevertheless persists in holding to his accusation (July-August issue of the *Journal of the Royal Astronomical Society of Canada*), the astronomers of the Pulkovo Observatory feel obliged to protest against such a dishonoring of their late