

received a small quantity of the gas. I was, however, unable to pursue the intended experiments because of press of other duties, and submarine disasters suggested that delay in the development of this use of helium might result in loss of life which would otherwise be saved; realizing, furthermore, my own inability to carry on the large scale experiments that would be necessary, I decided to lay the matter before the Bureau of Mines. On January 29, 1924, I accordingly wrote to Dr. S. C. Lind as follows:

You have probably heard of caisson disease and the theory that it is caused by the release of dissolved gases in the blood as pressure is removed on coming out of the caisson, or, in the case of a diver, in coming up from a considerable depth of water. There is considerable physiological evidence in favor of this theory. This is the trouble which limits the depth at which diving operations are practicable.

Inasmuch as helium is only about one-half as soluble in water as is nitrogen, it has occurred to me that a diver breathing a mixture of oxygen and helium would be able to work at greater pressures or to come up from a greater depth more rapidly. I suppose that any oxygen dissolved as such in the blood could be readily disposed of by muscular effort and would not be a source of trouble. If helium is of service in this connection, it would obviously be of great economic importance in salvaging operations.

I had hoped to try a few experiments with mice, releasing them suddenly from a pressure of several atmospheres, using in one case air and in the other case a mixture in which helium replaced the nitrogen of the air. However, my time is so much taken up that there seems to be no very early prospect of trying such experiments. It has occurred to me, therefore, that this is a problem which the Bureau of Mines might like to investigate. The results, if successful, would doubtless be patentable, so that work should be done with due regard for protection. I am not desirous of making any money personally from such a venture, but I would hate to have any one else do so, and would want any profits used for scientific purposes. I will be glad to hear what you think of the prospect.

At this time I was altogether unaware that this use of helium had occurred to any one else, and certainly had no means of knowing what was going on in the mind of Mr. Thomson. The issuance of a patent to C. J. Cooke, on November 6, 1923, was unknown to me until a long time after, as I am not in the habit of perusing patent literature. While, therefore, my associates and I can not claim priority in publication, we are, I am sure, indebted to no one else for the idea.

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## DE KRUIF'S MICROBE HUNTERS

OUR attention has been drawn to a book called "Microbe Hunters," recently published by Harcourt, Brace and Company, New York, written by Paul de Kruif. The work evidently aims at being a kind of popular history or rather romance regarding medical discovery, and mentions us among others. We should like an opportunity to say, for the information of your readers, that the author's statements about ourselves and our researches are incorrect; that they are not supported by reference to the original literature; and that his knowledge of the subjects with which we have been concerned is obviously incomplete.

We have been legally advised that some of his assertions regarding ourselves are libellous according to British laws; but in America we have no means of protection except a public denial of the truth of his allegations, and we therefore trust that you will allow us to publish such a denial, as emphatically as we may, in your columns.

Dr. Cuthbert Christy's signature does not appear on this letter as he is in Africa; before sailing, however, he left us the following statement:

"With regard to Chapter IX of Paul de Kruif's book 'Microbe Hunters,' I beg to emphatically state that it contains statements which are totally erroneous and misleading. As an example I will quote paragraph 2, page 264, which reads: 'The third member (*viz.*, myself) became disgusted with the ignorance and failures of his two colleagues and went off prospecting for rubber. . . .' This paragraph is absolutely untrue. It suffices to say that I have always given credit to Castellani for his discovery of the trypanosome as the etiological agent of sleeping sickness—see, for instance, my letter to *The Morning Post*, August 22, 1923. As regards my abandoning my colleagues and going off prospecting for rubber, this is absolutely untrue. I never abandoned my colleagues, and, as a matter of fact, I did not get interested in rubber until 1906 which was three years after the labors of the First Sleeping Sickness Commission were completed."

ALDO CASTELLANI,  
GEORGE C. LOW,  
DAVID NABARRO,  
RONALD ROSS

## PUBLICATION BY PHOTOGRAPHY

THE recent discussion of publication by photographic reproduction of typewriting (*SCIENCE*, Dec. 31, 1926; Feb. 18, Feb. 25, 1927) may warrant the following additional notes. The subject is well