

dealing with an environmental process in which three factors, retained fermentable carbohydrates, acidogenic bacteria, and the enamel surface play the leading roles."

It is true that certain writers, notably Prof. Gottlieb, believe that proteolysis is of major importance in the initiation of dental caries, but it is my experience that these men find few sympathizers among the research workers in the U.S.A.

Dr. Pincus's interesting and valuable contribution in your same issue throws further light on the role of proteolysis in the progressive increase of a cavity when once formed. As one interested in the prevention of dental disease, the question as to whether the primary breach in the enamel of the tooth is due to proteolytic or acid-forming organisms is of far more than academic interest.—I am, etc.,

London, S.E.1.

GILBERT J. PARFITT.

Aluminium Pneumoconiosis

SIR,—In the annotation (March 13, p. 506) you have a comment on aluminium pneumoconiosis. This write-up is very interesting. I would like, however, to correct a rather doubtful statement made therein. The article states, "The Canadian investigators think that the disease may be a form of silicosis caused by vaporized silica." Just what your authority for this statement is, is difficult to see. We have always been conscious that, in the presence of so much silica, silica as a possible cause or contributing cause of the condition found in the abrasive workers could not be overlooked. We do not, however, feel that this is modified silicosis. We incline to the opinion that the disease is produced by the extremely heavy exposure to alumina. It is realized that as yet we have not complete evidence that this opinion is correct. Some experimental work is being done which may resolve the problem.

It would be interesting if you canvass the situation in regard to an aluminium plant in Sweden. Dr. Sven Forssman, Professor of Industrial Hygiene, State Institute of Public Health, Tomtebodavägen (near Stockholm), Sweden, showed me two lantern slides from films prepared on men who had been employed in the plant concerned. The lung changes exhibited by these slides were very similar to those found in our abrasive workers.—I am, etc.,

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A. R. RIDDELL.

Medicine and the Arts

SIR,—Prof. Geoffrey Jefferson is too sweeping. In the course of a reply (April 10, p. 705) to a scientific correspondent he makes a brief excursion into the arts, and in half-a-dozen lines disposes of Elizabethan prose and the writers of long poems. He writes, "The art of tight and lucid compression of thinking was unknown in Bacon's day." Surely Bacon, on the contrary, was the earliest English exponent of the aphorism, the essence of which is conciseness. In his *Advancement of Learning* he distinguishes two methods of writing, by formal discourse or by aphorism. One method or the other was to be adopted—they could hardly be combined—and therefore he practised both. The difficulty of reconciling comprehensiveness with brevity is an old one.

From prose to poetry, and the professor's faith in Baudelaire's dictum, "Long poems were written by those who had not the talent to compose short ones." This can hardly be dismissed with a smile as an exaggeration, because it is not the exaggeration of a truth. Rather is it a Gallic *bon mot*, not to be taken too literally; perhaps Baudelaire was a better poet than critic. Be that as it may, the English Muse refutes him wherever the pages are turned, even in Lancashire. One example will serve to counter Baudelaire's thrust and display our poets' mastery of both forms, the long and the short—that superb short poem "The Poppy" with its opening stanza,

"Summer set lip to Earth's bosom bare,
And left the flush'd print in a poppy there;
Like a yawn of fire from the grass it came,
And the fanning wind puff'd it to flapping flame."

The poet was Francis Thompson, one-time student of the Manchester Medical School (1877–83), who was capable, too, of sustained inspiration in his long poem, "The Hound of Heaven."—I am, etc.,

Ashton-under-Lyne.

F. G. RALPHS.

Bone Conduction in Otosclerosis

SIR,—It is interesting to note the contradictory opinions expressed by the commentators on my letter printed in the *Journal* of March 20 (p. 569). Mr. R. Scott Stevenson (March 27, p. 618), quoting the work of Tumarkin, refers to "the myth of increased bone conduction" in otosclerosis. Mr. R. R. Woods (April 17, p. 754), while referring to the genuine improvement of bone conduction which occurs in the operated ear (after the fenestration operation), goes on to say that he has never seen the hearing of the unoperated ear altered in any way, either for air or bone conduction; indeed, he says it is inconceivable that an operation on one ear could affect the working of the opposite one. Mr. E. R. Garnett Passe (April 17, p. 754) writes, "It has been known for some time now that improvement to hearing by both air and bone conduction occurs in the unoperated ear in a certain number of cases, but this improvement is never permanent."

The case I referred to illustrated the fact that increase in air and bone conduction can occur in the unoperated ear, and I made no claim whatsoever to a permanent result for this case. In February, 1947, Mr. Passe published in the *Lancet* the results of one hundred fenestration operations done by the Lempert technique since August, 1945. With an equal operative experience to this number over the last two years, and with an operative technique which the results substantiate, I hope I can give equally well what he and Mr. Scott Stevenson call a "careful, sober, and dispassionate estimation of end-results."—I am, etc.,

London, W.1.

W. H. B. MAGAURAN.

Transfusion Reactions

SIR,—Dr. John Wallace and Mr. R. D. Richards in their article (April 3, p. 640) on transfusion reactions describe cases in which venous spasm interfered with infusion of red cells. I have seen a reaction to infusion closely resembling the case described in detail in the article, where venous spasm also seemed to be the cause. Other evidence, however, suggests a different explanation. The recipient was a healthy nurse belonging to Group A. She was given 500 ml. of Group O blood in order to estimate survival of the donor's red cells in a normal person. Venipuncture was performed apparently satisfactorily, and the infusion ran quite freely. Some thirty minutes after the transfusion began the recipient complained of a little aching in the arm, and when nearly 400 ml. had run in the pain became more severe. It was now noticed that there was acute tenderness and swelling localized to the biceps. The infusion, however, was still running quite freely, but was stopped in view of the discomfort.

In estimating the survival of the Group O blood, using the method described by Ashby in 1919, as modified by Dacie and Mollison in 1943, it was found that no increase in the number of inagglutinable cells (Group O cells) had occurred after the transfusion was stopped. From this it was inferred that none of the infused cells had reached the general circulation. Swelling of the biceps was followed by discoloration of the skin of the lower arm and upper forearm. Full recovery of function in the arm and subsidence of the swelling took place in about ten days.

From these observations it seems likely that the blood was infused into the intramuscular spaces in the biceps due to the intravenous needle passing through the vein into the muscle, and that venous spasm was not the true explanation of the reaction.—I am, etc.,

Manchester.

W. K. STEVENSON MOORE.

A Case of White Asphyxia

SIR,—It is still no exaggeration to say that many theatres in the country are not equipped for this emergency and, what is more, there is much scepticism regarding the efficacy of cardiac massage. Various claims have been made as to the period of cardiac arrest from which recovery has taken place, but the average time limit is five minutes. The central nervous system can tolerate total arrest for three minutes ten seconds, and, even then there is some permanent alteration in psychic behaviour.