his head under water, and was brought to shore about ten minutes after the boat was overturned. He was cyanosed and pulseless. I carried out the Schäfer method, and continued it with the help of a coastguard. Hot-water bottles and blankets were quickly fetched, and the limbs of the man rubbed by willing helpers. The Schäfer method produced great venous congestion of the face and neck, due no doubt to the expression of the blood out of the abdominal organs, the liver in particular. On changing, after some period of useless effort, to the Sylvester method I found the congestion rapidly disappeared. On going back to the Schäfer method the congestion at once reappeared. The same happened again on repeating the change. I came to the conclusion from this very striking experience that the best thing to do in a similar case would be to change frequently from the Schäfer method to the Sylvester and back again. This procedure evidently must keep up an artificial circulation of the blood through the head. I would urge others to follow this course and not stick to the one method only.

In the case of the boy, aged 9, I used the mouth-to-mouth method of inflation, placing a handkerchief over the boy's mouth, and pressing a hand over his stomach to prevent inflation of that organ. I found this method very easy and effectual, and a lady after watching me carried on the method very well, while I was called away to attend another rescued child. The boy had been found after some delay under the sail of the overturned boat, and could not be resuscitated.

The mouth-to-mouth method is far the most effectual method for children, and on another occasion I should try it on an adult, and alternate this with the other methods. From my practical experience I am convinced it is not the best thing to stick to any one, but to frequently change the method. I am speaking, of course, of help given by a trained man.—I am, etc.,

LEONARD HILL.

Loughton, Dec. 26th, 1911. London Hospital Medical Co lege.

DISEASES OF THE PANCREAS.

SIR,—In your review of Professor Albu's monograph on the diagnosis of pancreatic diseases, published in the JOURNAL of December 30th, 1911, p. 1698, you say that "when a drop of a solution of adrenalin is instilled into the eye the pupil dilates under normal circumstances, but in diabetes and Graves's disease this fails to occur." May I point out that the first part of this statement is contrary to the fact, and that the second part is only to a certain extent true? Loewi (Vienna Medical Society, July 14th, 1907), who first made observations on the effect of adrenalin on the pupil in its relation to diseases of the pancreas, found that small doses do not act upon the normal eye, but that after removal of the pancreas its instillation causes speedy and pronounced mydriasis, so that this effect can be used as a means for diagnosing pancreatic insufficiency. In 18 cases of diabetes Loewi obtained a positive result with 10 (55 per cent.), but with patients suffering from a variety of non-pancreatic affections mydriasis was only observed in 2 out of 30. Similar results were obtained by Quadrio (*Il Policlinico*, xv, July 26th, p. 933) and others. I have not had the opportunity of reading Professor Albu's book as yet, so I cannot say whether the mistake is his or your reviewer's, but, at any rate, it is likely to mislead your readers.

Further on in the same review it is stated that my suggestion that the crystals obtained in the "pancreatic" reaction "are a pentosazone is impossible, as pentose urine does not give the reaction." Such a dogmatic statement must, I think, be due either to a disregard of the facts or to a failure to appreciate them correctly, possibly owing to the difficulties of struggling with a foreign language. Normal urines to which a pentose, such as xylose or arabinose, has been added yield an osazone resembling in many respects that found in some urines giving a positive "pancreatic" reaction, as I showed in a paper read to the Royal Society in 1909.—I am, etc.,

London, W., Dec. 30th, 1911. P. J. CAMMIDGE.

SWEETS IN CHILDHOOD.

SIR,—There will always, no doubt, be found some who question whether sweets can have any deleterious effect on the teeth notwithstanding any evidence which has been, or may be, brought forward. Mr. Hopewell-Smith and Dr. O. Hildesheim may continue to believe that "Our knowledge of the etiology of dental caries is based on misapprehension, developed on lack of research, and crowned by ignorance," but I am sure the great majority of the members of the dental profession who have studied the question would vigorously contend that our knowledge of the etiology of dental caries is based on the illustrious scientific work of the late Dr. W. D. Miller, developed on "the knowledge that has been laboriously accumulated by the dental profession during the last twenty years" and crowned by the success of a method of prevention based thereon, which has recently been described as "eminently practicable and almost wonderful, nay, miraculous to the uninitiated, in its results."—I am, etc.,

London, W., Dec. 30th, 1911. J. SIM WALLACE.

SIR,—The answer to many of Dr. Hildesheim's problems is already to hand. The incidence of dental caries coincides, both in man and beast, with the use and stagnation among the teeth of "soft, sticky, starchy, and sugary food." The late Mr. Mummery's investigations proved this. At one end of the scale were exclusively meat eaters (Gauchos of the Argentine Plains) free from caries; intermediate were races using coarsely ground cereal foods (Hill tribes of Northern India); and at the other end civilized peoples using soft, sticky, farinaceous foods.

The problems enunciated may be answered in this way:

1. *The especial incidence in childhood.* Dental caries is a disease of early life among civilized peoples, since from the moment of eruption teeth are exposed to conditions favourable to decay.
2. *The greater incidence in females after puberty.* Not so in my experience.
3. *The different incidence in different social classes.* Avoidance of dental caries depends on personal cleanliness.
4. *The incidence among different races, and the relation, if any, to their dietaries.* Incidence of dental caries varies directly with the stagnation of soft, sticky, starchy, and sugary food.
5. *The incidence in wild and domesticated animals.* Depends on the same factor. The horse is a case in point: the wild horse is free from caries; the domesticated horse, fed on crushed oats, suffers from caries. (Mr. J. F. Colyer has published information on this point.)
6. *The degree of vulnerability of various teeth and of various surfaces.* Runs exactly parallel to their chances of stagnation. Well illustrated by the popular idea that the wisdom teeth come through decayed.
7. *The bacteria that are causally related to caries.* Needs more investigation.
8. *The nature and function of Nasmyth's membrane.* Whatever be its nature or function, it fails to protect against caries.
9. *The difference between white caries and brown.* White is rapid, and found in young teeth; brown is slow, giving time for the causative or other germs to develop chromogenic functions.
10. *The determining factors in the alkalinity and acidity of saliva.* Needs more investigation, but either weak alkalinity or weak acidity (and the salivary reaction is always weak) is favourable to cleansing and incapable of harming the teeth.

As concerns the starting-point of this correspondence, "sweets in childhood," I personally am convinced of the evil effects of stagnant sugar on the teeth, and I should like to get historical data as to the period when the use of sugar became general, and data of the incidence of caries before, during, and after that period, to test my conviction. Unfortunately, dated skulls are hard to come by—can any of your readers tell of any?

By the way, may I ask those who talk of the child's physiological craving for sugar what happened to the children during the long ages before sugar became a cheap household commodity? Did they die for want of sugar, or was the craving satisfied by the modicum present in what vegetable food they could get? Or have we come on a new fact in evolution—that children rapidly evolve a liking for what is nice?—I am, etc.,

London, W., Jan. 1st. J. G. TURNER.

PARASITISM.

SIR,—In the BRITISH MEDICAL JOURNAL of December 23rd, 1911, p. 1679, Mr. Wheldon points out that my definition of a parasite does not allow of differentiation between symbiosis and parasitism, but this is rather