This was returned: "The swab contained a green mould. Culture: heavy growth of haemolytic *Bact. coli*."

It is my practice not to switch on the soda-lime absorber for the first few minutes of an induction, so I next took the following swabs: (1) from the expiratory valve before absorption and ether: sparse growth *Bact. coli*; (2) from the inspiratory valve before absorption and ether: one mycelial colony only; (3) from the expiratory valve after a half-hour's absorption and ether: sterile; (4) from the inspiratory valve after a half-hour's absorption and ether: sterile. This seemed reassuring, so I used the apparatus for a whole morning's work—the last two cases being short gas-and-oxygen anaesthetics in which I used the Mushin apparatus without absorption, and again I took swabs from the valves and absorber ends of the corrugated tubing. These results were: (1) expiratory valve: a few discrete colonies of pneumococci; (2) inspiratory valve: sterile after 24 hours' culture. I may add that the face-pieces are immersed in biniodide solution between cases, but the corrugated tubing is merely hung up when not in use.

It seems safe, therefore, to deduce that there is no danger of cross-infection from bacteria in a modern anaesthetic apparatus using soda-lime absorption, and it even seems safe without the use of soda lime. There is still the possibility of virus cross-infection, and this is a matter which is perhaps worthy of investigation.—I am, etc.,

Worthing.

F. R. GUSTERSON.

REFERENCES

- ¹ *British Medical Journal* (1873), 1, 353: quoted in *Development of Inhalation Anaesthesia*, by B. Duncan: Oxford University Press.
- ² Magath, T. B. (1938). *Curr. Res. Anaesth.*, 17, 215.

Psycho-analysis

SIR,—In your excellent account in your obituary column (March 8, p. 316) of Pierre Janet's record, you state that Freud's psycho-analysis produced better results than those of Janet. Is there absolute proof of this assumption? Janet's work, as you so aptly described, was on the lines of hypnotic suggestion, whereas Freud by his psycho-analysis reduced the complex regressions apparently by his reduction of abreaction. In the last generation of such giants in hypnotic treatment alone, where no psycho-analysis was used but only hypnosis, how do you account for the remarkable results attained at the Nancy School by Liebault and Henri Bernheim, also Wetterstrand of Stockholm, who used hypnosis alone? Does it not prove that exalted suggestibility alone may reach the deep centres? Bleedings were produced, rapid pulses were slowed down, and in Wetterstrand's work in a case of paroxysmal tachycardia with the pulse running at nearly 200 beats per minute, sphygmographic tracings showed the beats, under hypnotic suggestion alone, slowing down to 80 per minute. Bernheim produced blistering under hypnotic suggestion, and Liebault practised among the poor at Nancy for 30 years using hypnotic suggestion alone, and his results lasted and were excellent. This is all a true and strange paradox, and it would be interesting to have your comments on this where no Freudian methods were used.—I am, etc.,

Southall, Middlesex.

JOHN R. O'BRIEN.

World Food Council

SIR,—With reference to the opening statement in the annotation on the above (Feb. 8, p. 228) it has to be emphasized that questions arise at once where the food ought to be grown and whether it is necessary for poor nations to increase their purchasing power in order to buy their food from abroad. Advocated by high-powered emotional appeal regarding "starving" nations, the "World Food Board" plan aimed at stabilizing prices by means of buffer-stocks and at disposing, profitably for the producers, of unmarketable surpluses.

To deal with the last point first: in the plan the "starving" nations were in reality to be used merely as dumps for excess food after it had been paid for by a third party, obviously to the great benefit of privileged countries and persons—the professional producers-for-sale of food, a very small part indeed of the world's population. The aid of "starving" nations, with a population mainly of non-industrialized peasants and abundant cultivable land, could, however, be enlisted in their

own interest to solve their underfeeding problem for good and all by their being educated and even constrained to grow their own food (more likely to be of a suitable nature) on the spot—a different proposal of course from that of using large-scale loans as a means of developing backward countries—backed up to the hilt, moreover, by the results obtained by amateur plot holders during the war.

For the professional food-producing countries the solution of their long-term problem lies of course in adapting supply to demand and in diversification of industries. Buffer-stock measures, that textbook commonplace and the present case cure-all, are a purely palliative makeshift contrivance, applicable only internationally for the temporary alleviation of the effects of depressions. Their use internationally is impossible because consumers' interests are diametrically opposed to those of producers. They are, moreover, an example—a hidden one—of that universally condemned expedient, the export subsidy, the international variety of which would add insult to injury by requiring foreign industrial consumers to provide, at considerable expense and loss of bargaining power, the major part of the funds for the subsidy. Buffer-stock schemes, moreover, are liable to other disabilities. Enormous expenditure would be involved in carrying them out. Where a commodity is produced at home and abroad the existence of tariffs to protect domestic production would prevent the establishment of a "world food price," the essential basis for their formation; stocks would deteriorate on storage; and, finally and worst of all, because of guaranteed prices to the producer there would be overproduction of food with unmarketable surpluses, a situation incapable of being met by production-control measures in view of the inelasticity of supply and demand. We are not dealing here with philanthropy, and on a purely economic basis the building up of reserves to meet possible famine situations ought to be the obligation solely of those nations subject to such hazards.

The development by Sir John Boyd Orr of the "World Food Board" plan from the suggestion by Mr. S. M. Bruce in 1935 of a "marriage between nutrition and agriculture" and the composition of the Preparatory Commission to inquire into its soundness could be discussed in some detail with considerable advantage. Further remarks, however, will be confined to observations on a recent occurrence which in many respects supplies an up-to-date summary of the situation. In the House of Commons on Feb. 6 Mr. H. Wilson, M.P., reported on and supported the findings of the Preparatory Commission. Mr. Richard Law, M.P., suggested that both Mr. Wilson's speech and the report buried Orr's proposal of a "World Food Board"; whereupon Mr. Wilson interrupted to say that Sir John Boyd Orr was "highly delighted" with the report. The obvious comment is that somebody has queer ideas of relevancy and refutation in a reply, and that somebody else appears to be remarkably easy to please and to be liable to self-persuasion, as the quotation from the *Observer* (see *B.M.J.*, Feb. 8, p. 228) also demonstrates.—I am, etc.,

Aberdeen.

J. P. MCGOWAN.

Use of Penicillin Pastilles

SIR,—Dr. T. H. Manners Kerfoot states (Feb. 1, p. 197) that "stomatitis after oral penicillin may be due either to the penicillin or the sucrose base." He argues that, as the sucrose base is used in other pastilles not containing penicillin without causing stomatitis, the penicillin must be the cause.

There is an alternative explanation, that both penicillin and sucrose base are required to produce stomatitis. I believe this to be the true one for the following reasons: (1) As Dr. Alexander MacGregor (p. 197) points out, in the original work on penicillin pastilles we treated over a thousand patients without causing stomatitis, the base in this case being soft and viscous. (2) If sufficient penicillin is given by the systemic route it is secreted in the saliva, producing the same changes in flora of the mouth as are obtained by local treatment. These patients do not develop stomatitis. It would appear, therefore, that penicillin alone or penicillin in a soft base does not cause stomatitis. If, however, patients having systemic penicillin at the rate of approximately one million units a day are given boiled sweets to suck they develop the condition.

The clinical evidence supports this suggestion, penicillin-stomatitis starting in the areas receiving the most trauma, namely the tip and sides of the tongue, even in the later stages the more protected areas tending to escape. Though the pastilles and the trochisci both contain sucrose, the trochisci contain the greater quantity. It may be that continuous sucking of such a base alone would produce stomatitis, the importance of the penicillin being merely that more intensive treatment is prescribed when penicillin is an ingredient than when it is not. While Dr. Kerfoot's observations suggest that this is improbable with the lozenges that he prepares it is conceivable that the continuous abrasive action of sucrose crystals on the buccal mucosa, supplemented perhaps by the osmotic effect of a hypertonic solution, may be sufficient to cause stomatitis in the absence of penicillin. Observations on the point are desirable.

Quite apart from the relationship between the trochiscus and stomatitis there are other grounds for criticism. Dr. MacGregor stressed the importance of a viscous base if the maximum effect is to be obtained on the tonsils, quoting the references for his opinion. This being so I will merely state that I am in complete agreement with him. The trochiscus is unsuitable for use in plastic surgery of the mouth because it is so hard that the patient will not tolerate it, and even if he can be persuaded to maintain it in a less sensitive part of his mouth the base accumulates in intra-oral wounds and has to be removed manually.

In conclusion I would stress that I consider the trochiscus penicillini highly unsatisfactory for the reasons stated, and hope that the manufacturing companies will soon devise a more suitable base. Until then, I believe the original pastille devised by Dr. MacGregor and myself to be the most satisfactory, in spite of its limited keeping properties.—I am, etc.,

London. W.C.1.

DAVID A. LONG.

Delayed Reaction to Penicillin

SIR,—The recent article by Dr. A. I. Suchecki (Dec. 21, 1946, p. 938) on allergic reactions to penicillin prompts me to record a case where the reaction was unusually delayed.

CASE REPORT

At Forest Isolation Hospital, Mansfield, a girl of five years, suffering from bronchopneumonia following whooping-cough, received 400,000 units of penicillin in four-hourly doses from Dec. 15-19. The penicillin used was an ordinary commercial brand in aqueous solution, injected intramuscularly. Rapid improvement was followed by a slight relapse ten days later, and sulphathiazole 8 g. was given orally from Dec. 30 to Jan. 4.

On Jan. 27—i.e., 39 days after cessation of penicillin and 23 days after cessation of sulphathiazole—she developed a raised, pink, blotchy rash, generalized in distribution but most marked over the sites of penicillin injections; oedema, especially of the hands, lips and eyelids; pruritus of moderate degree; pyrexia of 101°-103° F. (38.3°-39.4° C.); and considerable bronchospasm, which was temporarily relieved by adrenaline. There was no arthralgia, no hydrarthrosis, no lymphadenopathy, and no jaundice, and the urine was normal in all respects. The features which persisted longest were the oedema (which subsided on Feb. 1) and the rash (which disappeared on Jan. 31). On the neck the fading of the urticarial eruption coincided with the appearance of a number of petechiae.

The most delayed penicillin reaction previously known to me was Dr. J. N. Turnbull's case (Jan. 18, p. 111), where the reaction occurred 35 days after cessation of parenteral treatment. A delay of 39 days before the appearance of an allergic reaction is certainly most unusual, yet the features were unlike those of a sulphonamide drug rash and were completely in line with those of penicillin reactions as described by Suchecki and others.—I am, etc.,

Mansfield.

I. A. G. MACQUEEN.

Endocrines in Gynaecology

SIR,—The fact that the action of hormones depends not only on the hormones themselves but also on the mode of reaction to them of the target cells is well known in endocrine biology, and is bound up with the general conception of "biological field"—a conception which we owe to the brilliant work of J. Needham and C. Waddington. Albright coined the

characteristic expression "Seabright bantam syndrome." The male Seabright has female feathering, which is attributed not to any predominance of female hormone in the bird but to the special reaction of its feathers to male hormone. This conception enables us to understand much of the physiopathology of endocrine diseases.

Mr. Aleck Bourne (Jan. 18, p. 79) has rightly stressed the importance of this biological principle in practice. Endocrine diseases are not diseases of one of the six classical glands of internal secretion but disturbances in the balance of a whole system of constitutional or metabolic regulation composed of the genes, endocrines, neurovegetative system, psycho-associational cortex, certain environmental factors, and the reactivity of the cells—the "competence" of the biologists. Thus endocrinotherapy is not the sole method of treatment of "endocrine diseases." On the contrary, its one-sided application may do harm by upsetting the balance of the system of constitutional regulation, as shown by the bad effects of an exclusively endocrine treatment in certain eunuchoidisms in the young and by the disasters of attempts at rejuvenation with testicular organotherapy.

At the Annual Meeting of the B.M.A. at Oxford in 1936 I opened a discussion on physical methods in endocrinotherapy, in which I stressed the importance of physical medicine in modifying the balance of the system of constitutional regulation and thus its superiority, in certain endocrine diseases, over organotherapy. In endocrine as in all other diseases we deal with the complete psycho-physical personality. It is curious that psychiatry and endocrinology, the two branches of medicine which have contributed to our modern concept of total personality in disease, seem to revert to the comfortable one-sidedness which they have been fighting; psychiatrists stress unduly the psychological factors at the expense of the organic factors, while endocrinologists seem to dwell exclusively on the hormones with which we have been flooded.—I am, etc.,

London. W.1.

A. P. CAWADIAS.

Endocrine Receptors in Relation to Cancer

SIR,—No doubt the idea that an endocrine stimulus or impulse must be received as well as transmitted has occurred to many, though I agree with Mr. Aleck Bourne (Jan. 18, p. 79) that it has not been given the prominence which Mr. Bourne rightly, I think, considers it should receive. The idea, however, was the main point of a paper, entitled "A Theory of Cancer" published in the *Lancet*, 1920, 2, 1,298. It did not attract any particular attention as far as I know, and I only remember it because I wrote it and because I have never ceased to think of endocrine glands in general as structures which not only transmit a particular kind of energy but whose effectiveness depends on the proper reception of the message transmitted. I suggested, in fact, that sensitivity to hormone messages was a necessary function of every normal cell of every tissue, and I endeavoured to trace the sequence of events that must occur in a group of cells in which this function was being imperfectly carried out as the result of a failure to cope with an excessive demand on the regenerative capacity of those cells. I was not thinking of special hormones but of what I called the "co-ordinating hormone," a hypothetical hormone whose existence it seemed to me to be a logical necessity to assume, as in no other way could the harmony of growth and function of all normal tissues be explained.

I venture to recall these points now as a comment on Mr. Bourne's paper because I thought that the idea of receptors (or, as I called it, cell sensitivity) could be applied to Dr. A. Paine's then recently published paper (*Lancet*, 1920, 2, 692) on the origin of cancer. Dr. Paine's views may be epitomized in the following quotation from his paper: "We have thus traced cancer to its source; it springs from a degenerate cell, a cell that has in some manner been damaged or injured, and from such a cell all cancer cells are descended." In my paper I attempted to show that cells so degenerated gradually failed in all their functions, both special and general; I tried to make the point that this failure must include the failure to respond to the message of the co-ordinating hormone and that failure in this function must inevitably result in cancer.—I am, etc.,

Leicester.

T. C. CLARE.