Actinomycosis of Rectum and Anus

SIR,—In connexion with an investigation which I am making into the incidence and symptomatology of actinomycosis involving the rectum and anus, I should be greatly obliged if any of your readers would let me have details of any such cases which may have come under their observation.—I am, etc.,

London, N.W.3.

V. ZACHARY COPE.

Rutin in Capillary Fragility

SIR,—The annotation entitled "Rutin in Capillary Fragility" (May 31, p. 771) is in our view too committal on several points as yet *sub judice* and is incorrect on a number of others.

The name "citrin" has been, and still is, applied by many writers indiscriminately to concentrates, of various origin, that may or may not have been made precisely according to the method described by Szent-Györgyi in 1936. All these concentrates, however, seem, in so far as their physical properties have been described, to be water-soluble and relatively stable in aqueous solution. How "citrin" can possibly contain appreciable quantities of hesperidin, a glycoside of hesperetin (and therefore a derivative of flavanone—not flavone) and virtually insoluble in water, was never explained by Szent-Györgyi when he suggested that his citrin might be a mixture of hesperidin and its desmethyl compound, called by him "eriodictin" (*not* eriodictyol, an aglycone, already well known to occur in yerba santa and other plant materials). Unfortunately in the same paper Szent-Györgyi referred both to eriodictin and to eriodictyd, admitting subsequently (private communication) that the latter word was due to a printer's error! This did not help to lessen the confusion.

Two other workers have reported the isolation from "citrin" of eriodictyol glycosides and the preparation from them of eriodictyol by hydrolysis. It has been stated that these materials, though water-soluble, have no effect on capillary fragility. If either of these glycosides is Szent-Györgyi's hypothetical eriodictin, its water-solubility is difficult to understand; it would mean that replacement in the fairly complex hesperidin molecule of one methoxy group by a hydroxy group causes a virtually insoluble substance to become extremely soluble in water. We also find it difficult to credit the claim that insoluble hesperidin has been isolated from the soluble "citrin." As to the alleged inability of hesperidin to raise capillary resistance, we can only call attention to our published work, which seems unknown to the writer of your annotation: separately we have reported on many occasions that hesperidin, after repeated recrystallization from boiling methanol and probably rendered thereby more nearly pure than any other specimens examined, exhibits marked vitamin-P activity both on experimentally depleted guinea-pigs (A. L. B.) and on patients (H. S.) with low capillary resistance not raised by pure ascorbic acid alone.

Part of the discrepancy in experimental findings is no doubt due to the different origins and treatments of the materials tested and to the absence of any standard preparation or generally accepted technique for the biological test. It has also been suggested that the chalcone of hesperidin (with the oxygen ring opened) may be the active form and, owing to its relative instability, present in small quantities in apparently pure hesperidin, of which it is a tautomer. Californian investigators have claimed vitamin-P activity for the methyl ether of this chalcone, and a much increased stability, but the only material so described and examined by us did not appear to be of the claimed chemical composition. On the other hand, the work of Parrot and his colleagues in Paris with mixtures of isomeric epicatechins suggests that one of these isomers may be a highly potent form of the vitamin, perhaps responsible in traces for the activity of other less potent and apparently pure compounds.

However that may be, our own tests do not substantiate the claim that rutin—a derivative of flavonol, not flavone—has marked activity in the treatment of nutritional vitamin-P deficiency. Preliminary experiments on guinea-pigs and human subjects indicate that its activity is no greater than, and may indeed be only about half that of, hesperidin. Moreover, neither of the two samples received by us from the U.S.A. appears to be a single pure compound: both are pale greyish-green powders, from which pale yellowish crystalline material

can be separated by recrystallization. Of these two specimens, one was found to have a melting-point of 191° to 191.5° C., the other of 186° to 188° C. Even if the former is pure (which we do not believe), the latter cannot be.

Admittedly the whole of this confused position could not be clarified in a short annotation, still less in a letter, but we cannot accept your claim that "probably the active substance is rutin" without careful scrutiny of the evidence in favour of hesperidin chalcone and its methyl derivative or, more recently and more convincingly, of certain epicatechin isomers by Parrot and his colleagues. On chemical and biochemical grounds, therefore, we maintain that your conclusion as to the activity of rutin and the nature of vitamin P cannot be substantiated. Equally potent criticisms may be levelled at your assessment of its clinical uses.

Although we have confirmed the observation that a certain proportion of cases of hypertension have a significantly low capillary resistance, and also that the proportion of patients with retinal haemorrhage is significantly greater among those with low capillary resistance, we have no evidence to suggest that these phenomena are aetiologically related. As to the action of rutin in increasing the capillary resistance of hypertensive patients, our experience is that either an enormous quantity or very prolonged administration is necessary before any demonstrable effect is obtained. We have, in fact, not yet succeeded in restoring to normal levels the low capillary resistance in any hypertensive patient by means of rutin. Until much more information is available we feel that your statement that the most important purpose for which rutin may be given is "to prevent haemorrhage in patients with hypertension" is liable to give a false impression.—We are, etc.,

A. L. BACHARACH.

H. SCARBOROUGH.

Spinal Pumping

SIR,—Before the iron curtain clanged down A. D. Speransky was successful in publishing an account of the researches of his group in English. *A Basis for the Theory of Medicine* was published in New York in 1935. Here the reasons for his belief that the nervous system plays an important part in the genesis of acute and chronic illnesses are expounded. I have summarized his hypotheses in two papers published in this country—"Recent Researches in the U.S.S.R.," *Bristol med.-chir. J.*; "Constitution and Chronic Disease," *Brit. homoeop. J.*, 1939, 29, 224. Moreover, many of the experiments which formed the basis of his theories have been published in French and German journals, and a long bibliography at the end of his book indicates some of the sources.

Briefly put, Russian studies of the circulation of the blood and C.S.F. in the medulla, subarachnoid spaces, and spinal nerve sheaths gave rise to a number of new working hypotheses for investigating the role of the nervous system in pathological processes. The passage of antibodies through the blood-stream barrier was studied in experimental rabies by Ponomareff, A., and Tchechkoff, A. (*C. r. Soc. Biol.*, Paris, 1927, 97, 376), and Jowelew, B. M. (*Z. ges. exper. Med.*, 1930, 74, 217) and it was in this research that the technique of pumping or "pompage" to break down the blood-stream barrier and increase the permeability of the vessels of that barrier was devised. It was claimed that the antitoxins of diphtheria, tetanus, and dysentery could be made to pass through the hitherto impermeable barrier by this method.

The technique was first applied to human beings in cases of meningitis, where in six cases extraction and reintroduction of 10 ml. of C.S.F., repeated five to eight times, had dramatic results. A seventh case ended in a death, and the technique was abandoned temporarily as the "pompage" could not be altogether absolved. It was introduced again in the treatment of acute rheumatic fever with the idea that the salicylates could be brought into closer contact with the nervous system, and Gorschkow, M. A., and Babkova, A. A., published their results (*Z. ges. exper. Med.*, 1929, 67, 278). After the exhibition of salicylates 10 ml. of C.S.F. was withdrawn and reintroduced eight to forty times. 70% cures were claimed in first attacks and over 70% cures in cases where the attack was the second or third one. Later, "pompage" was used without any salicylates, and though the results were not so striking good effects were obtained.

Speransky also noticed that after "pompae" there was sometimes a dissociation of the rheumatic fever syndrome: sometimes the temperature fell but the joints remained swollen for some days; or alternatively the pyrexia was maintained for some days but the swelling and pain in the joints rapidly disappeared. He argued from his study of trophic disorders that the rheumatic process was a specific nervous dystrophy, and that it was not necessarily the increased accessibility of the salicylates provided by the pumping that was remedial, but that the pumping itself was a nervous "stimulus" that provoked a new set of reactions in the nervous system which could abolish the pattern set up by the rheumatic process. Pumping, therefore, is a non-specific method of interference, and Speransky's team claimed good results in typhus and malaria. But Speransky gives the warning that pumping is a trauma of the nervous system, and if it is not followed by a reaction specifically useful it will inevitably make the position worse.

The Gillmans are to be congratulated on their enterprise in clinically confirming the results claimed by the Russian workers.—I am, etc.,

Bristol.

FRANK BODMAN.

Iodine and Failing Lactation

SIR,—With reference to the article by Dr. Margaret Robinson on "Iodine and Failing Lactation" (July 26, p. 126), you may be interested to hear that my late respected kinsman William Bennett, of Otley, was 70 years ago prescribing iodine for the last three months of pregnancy and the first three months of the puerperium as a routine measure. His instructions were: "Take two drops of Lugol's iodine (freshly made) upon a lump of sugar dissolved in milk each morning throughout the 7th month; four drops in the 8th month; and six drops in the 9th month; repeat the six drops for the next three months." He insisted that the increase of dosage should be made when the moon was at full. For the pernicious vomiting of pregnancy he gave tinct. iodi mitis, 2 min. (0.12 ml.), in aired water, fasting.

When busily engaged in practice he attended a maximum of 280 confinements per annum. He was in active practice over 32 years, and in the extreme of age I have frequently heard him state that where his instructions had been followed no mother was without milk and no vomiting was uncontrolled. I personally have followed his treatment with similar results for the last 25 years. It is interesting to have specialist if not consultant approval, albeit belatedly, for this treatment. Has any work been done upon the connexion between pernicious vomiting, failure of lactation, and "pinning areas for cattle and sheep"?—I am, etc.,

Leeds.

JAMES E. OUTHWAITE.

Standard of Nursing in the U.K.

SIR,—The letter from the Registrar of the General Nursing Council for England and Wales (July 26, p. 149) deals with a matter which is always of importance but perhaps especially so at the present time, when the shortage of nurses is being so severely felt by all classes of hospitals and allied institutions. The Registrar refers to the fact that the G.N.C. is being pressed from certain quarters to a lowering of its standards of training, and points to the fact that nursing education standards here would already appear to be falling below those in the Dominions.

Is it permissible to suggest that the pressure on the G.N.C. comes from experienced and responsible sources, and that those responsible for British nursing standards would be well advised to pay heed to it rather than to raise further the standards of theoretical nursing education in this country?—I am, etc.,

Inverness.

T. C. MACKENZIE.

Report of B.M.A. Film Committee

SIR,—It is distressing that the Report of the B.M.A. Film Committee (Appendix II, Supplementary Report of Council, *Supplement*, June 21, p. 140) should have evoked neither appreciative nor critical comment in your columns. I must confess that, not being an avid reader of the *Supplement*, the Report at first escaped my attention, and with this in view one must earnestly hope that its relegation to the last few pages of the *Journal* did not impede its potential circulation. Further, its

length is of such proportion as might engender timidity on the part of a non-technical reader. Patient reading, however, dispels this notion, and student and teacher alike will find it of interest if not of value also. Surely the committee is to be commended on the publication of this document, for though many fragments of it have appeared before and separately, the collation of such material at a time when medical schools are still busy endeavouring to formulate their own ideas on the subject must be beset with difficulties.

The inclusion of a short section on the film strip (paras. 26-33) is indeed interesting and encouraging, though by virtue of loose definitions and important omissions this part of the Report lacks the necessary conviction. For instance, no mention is made of the fact that such strips are customarily prepared on non-inflammable film of 35-mm. gauge and that (at present) two frame sizes are available. Reference to one of the more important features of film strip has only been made in the most nebulous manner: it should be understood that the scripting and construction of a first-class strip is not at all unlike that of a motion picture. Hence, in the many cases where the demonstration of movement is non-essential, the former medium may be used with even greater efficiency than the latter and at a fraction of the production cost. Points concerning the production of recapitulatory film strips from selected frames of motion pictures need not be laboured here, but a section of film strip should not be complete without some allusion to the miniature lantern slide, without which there is a danger of imposing an unnecessary limitation on the versatility of the medium as a whole.

No doubt the above points would have been covered by a more complete survey, but as, in the words of the Report (para. 31), "Films and film strips are probably the most closely related visual aids . . ." it seems important that a clear conception of the inter-relationships involved should be presented. Together with the Scientific Film Association's Film List this Report must form a valuable guide to present trends, and should stimulate those of us who are even remotely connected with the preparation and use of visual aids.—I am, etc.,

Department of Medical Photography,
Westminster Hospital.

PETER HANSELL.

POINTS FROM LETTERS

Request from Finland

Dr. F. CHOWN (Penzance, Cornwall) writes: I have received a letter from a Finnish doctor—Dr. Reino Lagus, Pettosalmi—to say that he is likely to attend the International Congress for Mental Health, to be held in London in August, 1948, and in order to solve the difficulty of getting foreign currency he is willing to receive an English doctor, or member of his family, as a guest in his house for a month either this summer or at the beginning of next summer, if he in exchange will help Dr. Lagus in his stay in England. Dr. Lagus speaks English. The address is: Dr. R. Lagus, Arhadia, 31, A 23, Helsinki, L., Finland.

Warmth and Colour

Prof. W. BURRIDGE (Oxford) writes: . . . I have to suggest . . . that no explanation of colour vision can be held satisfactory if it neglects the artists. They are the scientists of colour sensing, and we should grant them the credit of being as correct in their judgments as we are in our own. That is to say, a satisfactory colour vision theory must make automatic provision for a division of colours into the warm, cold, and neutral as well as for yellow to be perceived as warmer than red. Up to the present, however, the physiologists and psychologists have assumed that the artists have made an awful mistake in sensing yellow as warmer than red. They have consequently framed theories to accord with the assumption that the artists are wrong. But if the artists were right there must be something fundamentally wrong with the assumption. It is also agreed among writers of textbooks on vision that no satisfactory explanation was known to them of the production of colours by Benham's top. This question was taken up in India by Dr. Naidu, of Annamalai University. He found a very simple explanation of the production of colours by Benham's top. In fact, by accepting a different basis for the fundamental action of light on the eye, colour production by Benham's top became a predictable phenomenon. Colour production by Benham's top accords with the artists' division of colours as well as with the sensing of greater warmth in yellow than in red.