Those who support the concept of centrifugal flow in the cortex point to the great rarity of arteries in the outer third, which have never been visually demonstrated even by writers who have diligently sought them out,⁷⁻⁹ despite the apparent ease with which they could be filled, if they existed. Professor Trueta in his article has claimed to demonstrate them by electron microscopy. His photographs, which presumably are his best, are printed as negatives, contrary to accepted practice, and are therefore difficult to assess, especially without benefit of technical details, such as mode of fixation and precise site of origin of the ultra-thin sections. In the knowledge that these are prone to artifact formation even in the best of hands, and open to misinterpretation, the interested reader will come to his own opinion as to their evidential value.

Another technique used by Professor Trueta to show periosteal arteries in bone cortex, the tetracycline injection method and ultra-violet microscopy, gives, if anything, additional proof of the regular capillary nature of the vessels in compact bone, a fact in support of the centrifugal flow concept and anyway far better and more easily demonstrated by Indian ink perfusion. It is surprising that Professor Trueta should refrain from demonstrating periosteal arterial channels regularly passing into the outer cortex of normal bones, using the perfusion technique of which he is rightly an acknowledged master. This should not be so difficult; even a small capillary is 5 μ in diameter and can be seen easily by medium power light microscopy. Until arterial channels are demonstrated to be a regular feature of the outer compactum, the concept of centrifugal flow in the cortex of long bones, as originally enunciated in 1957,¹ may be said to reflect the facts of cortical circulation better than the older view supported by Professor Trueta.—I am, etc.,

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Gall-bladder Dyspepsia

SIR,—I hope you will forgive me if I disagree with you when you say in your leading article (July 20, p. 128) "a further diagnostic bubble is burst" on the strength of Dr. W. H. Price's article (July 20, p. 138) in the same number. He seems to rely on x-rays for the diagnosis,

and both he and Dr. A. H. Douthwaite (August 3, p. 318), following Moynihan, seem to stress gall-stones as the cause of the dyspepsia. Herein, I think, lies the fallacy; it is the disease of the gall-bladder, not the stones, which causes the dyspepsia, and x-rays only play a small part in the diagnosis. Dr. Douthwaite rightly lays stress on the symptoms.

John Ryle,¹ a very reliable and astute observer, divided the dyspepsias into five groups, according to causes, (1) habit, (2) nervous, (3) toxic and infective, (4) irritative (ulcer, infections of the gall-bladder and appendix), (5) mechanical (pyloric stenosis, cancer, etc.). Of 267 cases, unselected except that they had been ill enough to go to hospital or a consultant, he found that 52% fell into groups 4 and 5. My friend Arthur Hurst inspired me with a special interest in these groups where surgery and medicine overlap so much in treatment, and gave me much help and advice.

The diagnosis of chronic cholecystitis depends on the symptoms; distension after meals, goose skin or shivering, and referred pain and tenderness in areas, front and back, supplied by seventh, eighth, and ninth segments of the spinal cord. The symptoms may not be typical, and may indeed resemble those of duodenal ulcer. The gall-bladder is tender, and there may be guarding of the right rectus. It is only when the disease is so far advanced that the gall-bladder fails to concentrate dye, or contains stones, that a cholecystogram shows abnormality. Gall-stones, I believe, only cause symptoms when they cause obstruction, or when there is, as so often, infection present. Indeed, Hurst² thought that Moynihan's "inaugural symptoms of gall-stones" were the symptoms of cholecystitis, and he thought that if x-rays showed want of concentration in the gall-bladder the case was beyond medical treatment.

If medical treatment has failed, or is contraindicated, then operation is justified, and operation gives more satisfactory results. If there is no such entity as gall-bladder dyspepsia, it is difficult to explain the success of cholecystectomy in ending the dyspepsia in cases of cholecystitis.—I am, etc.,

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Different Patterns of Human Porphyria

SIR,—As a worker who for many years has been engaged in the study of porphyria in man, as well as cattle and pigs, I feel that a few additional remarks to the important contribution of Professor Jan Waldenström and Dr. Brigitta Haeger-Aronsen (August 3, p. 272) are needed. Firstly it is important to keep in mind that "porphyrias" is not a precisely defined term. Goldberg and Rimington¹

write that porphyrias are metabolic disorders, hereditary in origin, but nevertheless they also use the term to designate acquired conditions.² It is therefore not logical to define porphyrias as inborn errors of metabolism; if the nomenclature now in common use is accepted, both hereditary and non-hereditary porphyrias must be accepted. Secondly, Professor Waldenström and Dr. Haeger-Aronsen forgot to mention two forms of hereditary porphyria in man, idiopathic hereditary coproporphyria³ and infantile haemolytic anaemia accompanied by excretion of porphyrin precursors porphobilinogen (P.B.G.) and δ -aminolaevulinic acid (A.L.A.).⁴ These two hereditary metabolic diseases are especially interesting because the former is associated with an abnormal amino-acid excretion pattern and the latter forms a bridge between hereditary haemoglobinopathies and porphyrias. Recently I have pointed out⁵ that it may be more correct not to distinguish between porphyrias, haemoglobinopathies, and inherited anomalies of bile pigment metabolism and look on these inborn errors of metabolism—enzyme disorders, molecular diseases—as errors of pyrrole metabolism and not as three distinct groups. It is, however, to be remembered that our knowledge of these conditions is at present very limited. Therefore it is to be recommended that the porphyrin metabolism should be studied also in haemoglobinopathies and hereditary anomalies of bile pigment metabolism; in general, biochemical studies of as broad a nature as possible, together with genetical studies, should be performed in these conditions. In my opinion the nomenclature of the porphyrias is to-day very contradictory, with every investigator presenting his own variation. We have to admit that our knowledge is too limited to accept a rigid subdivision of the porphyrias, and various authors are very far from agreement. Personally, I agree with the interpretation of Professor Waldenström on the whole, but I want to point out that conditions are a bit more complicated than is expressed in the article of Professor Waldenström and Dr. Haeger-Aronsen. Thus I have seen two paralytic cases of acute intermittent porphyria where no P.B.G. could be demonstrated in the urine by ion-exchange chromatography, and I feel it is important to point out that there is no correlation between the excreted quantities of P.B.G., A.L.A., and porphyrin and the clinical symptoms in acute intermittent porphyria, a fact first pointed out by Ackner *et al.*⁶ Professor Waldenström and Dr. Haeger-Aronsen mention in their discussion that acute intermittent porphyria may be several genetically closely related but different molecular diseases due to various mutations of the same gene. In a study of the relatives of a series of cases of acute porphyria by means of ion-exchange chromatography of P.B.G. and A.L.A. I found several facts pointing in this direction. This material is to be presented in detail together with a thorough descrip-

tion of the analytical technique used.⁷ Interested readers are referred to this article and two recent reviews.^{8,9} Finally I want to stress the statement of Goldberg and Rimington¹⁰ that whenever an active case of acute intermittent porphyria is discovered it becomes a duty to test the urines of relatives for the presence of porphobilinogen; this test needs to be done with ion exchange chromatography and as far as possible determination of A.L.A. should be included (see ref. 7).—I am, etc.,

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Foreign Bodies in the Stomach

SIR,—I enclose a photograph of foreign bodies removed from the stomach of a male prisoner. There were no symptoms,

but the ingestion of foreign bodies had been suspected and an x-ray showed knives, etc., and two very strange objects which proved to be prisoners' tightly compressed sugar tins. The knife was in three pieces, which in the photograph have been laid end to end.

The biggest collection of foreign material that I have ever heard of was published in the *Sunday Pictorial* of September 21, 1958, when it was said that a surgeon in Sweden (Dr. Rossman) had at one operation removed: 1,500 pins and needles, 100 nails, 330 buttons, 167

coins, 12 curtain rings, 11 keys, and 176 other things, including fish hooks, badges, stones, a buckle, and a bicycle tyre valve!—I am, etc.,

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Part-time Work for Women Doctors

SIR,—I read with interest the correspondence earlier this year regretting the loss of married women to the profession. I have, since qualifying in 1948, worked in general practice almost non-stop, despite having had four children over that period. For the last seven years I had my own small list in London. This spring we moved to Suffolk. Extensive local inquiries indicated that there was no part-time work in any medical field whatsoever. Before starting a fresh N.H.S. practice (the area is intermediate) I realized that I would have to brush up my obstetrics considerably, and thus need to work in a labour ward plus its associated clinics for a few months, a house job being out of the question with a young family. I explained to the departments I approached that I would of course not expect any remuneration. It proved impossible to obtain these facilities. I was able to attend Mr. David B. Brown's excellent two weeks' course in Chelmsford (September 7, p. 597), but that of course was only a beginning. The only department that has so far offered me the facilities is one in the West Indies. Continental medical friends assure me that in various European countries such facilities are made available as a matter of course and courtesy among colleagues.

One appreciates that a busy department might thereby have a little additional responsibility placed on it. On the other hand, it would be sending out into its district a disciple, thereby facilitating the highest level of co-operation that is so desirable. If this is really asking too much, not only will married women doctors be lost to the profession, but the latter shows itself lacking in that flexibility without which nothing can either live or grow.—I am, etc.,

Little Cornard,
Suffolk.

ELEONORE ETTLINGER.

Periodic Health Examination

SIR,—Everybody will agree with your correspondent Dr. A. H. Levers (September 28, p. 810) that in the present state of the National Health Service it would be impossible to carry out all the tests he mentions. "Half a loaf is better than no bread." and from the point of view of malignant disease a yearly check up would be of much value in detecting "early stage cancers." A quick examination of the mouth, tongue, breasts in the case of women, uterus with smear (this latter need only be done every two or three years), and rectum for cancer of the rectum or prostate, would be of

very great value and save many lives. Such an examination should not take more than 20 minutes. To wait until disease is well advanced before diagnosis is a counsel of despair.—I am, etc.,

MALCOLM DONALDSON,
Hon. Secretary.

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Oxford.

Certification

SIR,—As far as the working conditions in general practice are concerned, there has been too much talk of money—and too little demand for positive improvements in the working system. The major difficulty is, and always will be, cost. The Government is loath to implement any scheme—no matter how praiseworthy—which will cost it more money. Any suggestion must then create not only improved conditions but also not cost the Government money to implement.

The suggested need for revision of insurance certification—little changed since its inception—put forward by Dr. Alex Crawford (September 28, p. 808) seems to be a field of possible hope. I agree with his remarks that, in town practice at least, the ritual of certification is very time-consuming.

The attendance of the patient at a surgery has often been necessitated more by the need for a certificate than by the degree of illness, and these attendances make up a fair bulk of patients in surgeries to-day. The G.P.'s time is increasingly taken up by administrative matters, and this insurance certificate field is one which could easily be removed from him and done just as well somewhere else—with a consequent lightening of his work load, and therefore improved care for those really needing his time. A system could be evolved based on a local insurance clinic where a patient could attend for all certificates needed. The clinic could be staffed by a part-time rota of G.P.s, etc. (seeing all patients but their own) and paid on a sessional basis. If treatment was required the patient could still attend his G.P. for this, but a vast number of patients would be able and willing to handle their own minor illnesses without wasting their time in surgery as they do now mainly to obtain a certificate. The deterrent effect of a clinic attendance could prevent the milder lead-swinging activities which must cost the country millions each year, and the scheme would show a profit in no time.

Short or prolonged certificates could be issued at the clinic's discretion and the G.P. notified accordingly. The duration would be based on clinical condition and not on the list-preserving relationship which tends to bias the G.P. The money saved on insurance payments would gratify the Treasury and the guilt complexes of patient and G.P. would be lessened.

Complications and difficulties are obvious—for domiciliary illnesses, etc.—but none are insoluble with a little