drugs. However, although it is not possible to prove this the responsibility of fluothane for the jaundice seems convincing. While many drugs have been indicated as causes of liver damage, this patient received no other agent even suspected of hepato-toxicity. Intra-abdominal operation near the liver and antibiotics have also been blamed; however, this lady was in hospital for immediate and delayed suture of her ulnar nerve, and antibiotics were not administered later than seven weeks before the onset of jaundice.

I would like to thank Dr. P. B. Newcomb for his kind permission to report this case.—I am, etc.,

Hackney Hospital,
London E.9.

J. ROBINSON.

Chlorphentermine

SIR,—I must protest at the reference made to chlorphentermine ("lucofen") (October 5, p. 853) in regard to the inference that chlorphentermine, while having an appetite suppressant action, also produces central nervous stimulation and is not satisfactory. We too have noticed amphetamines in this respect.

Chlorphentermine has been widely prescribed in this country and abroad and there have been no reports of nervous stimulation. This is corroborated by a number of publications, of which the following examples are listed.¹⁻³—I am, etc.,

D. D. H. CRAIG.

William R. Warner & Co. Ltd.,
Eastleigh, Hants.

REFERENCES

- 1 Levin, J., Trafford, J. A. P., Newland, P. M., and Bishop, P. M. F., *Practitioner*, 1963, 191, 65.
- 2 Mune, O., and Pallesen, A. E., *Ugeskr. Laeg.*, 1960, 122, 605.
- 3 General Practitioner Research Group Report, No. 12, *Practitioner*, 1961, 187, 216.

* * We have shown this letter to our expert contributor, who replies as follows: "Although absence of stimulating effects was noted in the reports on chlorphentermine ("lucofen") quoted above, claims for any superiority over amphetamine or dexamphetamine must await adequate comparative trials, as similar claims for other anorectic drugs have often subsequently been shown to depend on the use of less effective dosage. It must also be noted that evidence of habituation or addiction may only appear after several years of general experience with such drugs."—ED., *B.M.J.*

Amiphenazole and Bronchitis

SIR,—The persistent bludgeonings which we suffer at the hands of advertisers have mercifully blunted my sensibilities, so that I usually find it a painless matter to consign automatically to the waste-paper basket all but the small informative fraction of the advertising matter for pharmaceutical products which

comes through my letter-box. But I am moved to protest by an advertisement in the form of a news-sheet which caught my eye, with banner headlines on the first page, "Major Breakthrough in Chronic Bronchitis." Wondering whether I had missed something important in my own field of interest, I read on, to find that the drug to which it referred was none other than amiphenazole, under the trade name "daptazole." We have been familiar with the actions of this drug in chronic bronchitis for several years; as far as I am concerned, since my friend Dr. Tom Simpson drew my attention to it about five years ago. Publications by him and by Dr. G. M. Little were quoted in the advertisement. The observations of Dr. Simpson and Dr. W. Hawkins were recorded briefly in a letter in your columns¹; they concerned patients with ventilatory insufficiency and carbon-dioxide retention, in whom large doses of amiphenazole intravenously at hourly intervals were found to "stimulate the respiratory centre even when depressed by oxygen." It was also noted that effective doses made the patient cough, and caused nausea which helped to clear the sputum. Dr. Little's² work also concerned patients who were gravely ill with exacerbations of ventilatory insufficiency, and his moderate conclusion was a suggestion that amiphenazole might have a place in the treatment of carbon-dioxide retention. I hope that all who perused this document did so with an alert critical faculty and so perceived the extremely tenuous connexion between these quoted observations and the sweeping claims that "by prescribing this new (*sic*) drug the doctor can not only ease the burden of the chronic bronchitic, but postpone or prevent his eventual hospitalization."

So far as I know, there is no evidence that amiphenazole is of the slightest value in the management of the ambulant chronic bronchitic.—I am, etc.,

J. G. SCADDING.

Institute of Diseases of the Chest,
London S.W.3.

REFERENCES

- 1 Simpson, T., and Hawkins, W., *Brit. med. J.*, 1959, 1, 508.
- 2 Little, G. M., *ibid.*, 1962, 1, 223.

Cyclophosphamide

SIR,—Your special correspondent reporting on the symposium on cyclophosphamide held at the Royal College of Surgeons on October 4, 1963 (November 9, p. 1189), quotes me as follows:

"Dr. G. H. Fairley (St. Bartholomew's Hospital) felt it was too early to draw any conclusions on 62 cases of Hodgkin's disease that had been treated with cyclophosphamide over the past three years because in only eight of these had it been the first form of treatment."

This is incorrect and misleading. Since early cases of Hodgkin's disease are usually treated with radiotherapy it is

not surprising that cyclophosphamide was the first form of treatment in only eight of our 62 cases. What I in fact presented was an analysis of the results of treatment when cyclophosphamide was used for the first time regardless of previous radiotherapy or chemotherapy; in most cases cyclophosphamide was the first form of chemotherapy used. To say that I felt it was too early to draw any conclusions is quite untrue, as the results showed that cyclophosphamide, like other nitrogen mustards, is a most valuable drug in the treatment of patients with Hodgkin's disease and is likely to prove effective the first time it is used.—I am, etc.,

G. HAMILTON FAIRLEY.

St. Bartholomew's Hospital,
London E.C.1.

Apparatus for Oxygen Administration

SIR,—As Drs. D. C. Flenley, D. C. S. Hutchison, and Professor K. W. Donald (November 2, p. 1081) have clearly shown, the performance of the current production version of the Venturi mask is not satisfactory. We too have noticed this and I would like to amplify their observations.

The current mask differs from the prototype in having a shorter barrel and a narrower (and therefore higher velocity) additional oxygen nozzle. On returning to the dimensions of the prototype we have, however, found that the performance, although better, is still unsatisfactory. It seems that the measurements I reported in 1960¹ were misleading because, not having a rapid oxygen analyser, I used a technique of multiple sampling which gave only an average concentration in the mask. We are now able to explore the whole air-stream using a mass spectrometer which gives a virtually instantaneous measurement of oxygen concentration. This instrument has shown us that it is difficult to eliminate regional variations due to inadequate mixing in a mask with two oxygen sources. In view of the other disadvantages of this mask, such as the need for a double regulator, its lack of robustness, and its cost, we have suggested to the makers that it be replaced.

We are developing other approaches to the problem. The first to reach production is a single-concentration single-nozzle Venturi mask ("ventimask," Oxygenaire), which can be used with any regulator and is sufficiently inexpensive (7s. 6d) to be regarded as disposable. The concentration delivered by this mask is intended to be 27% at an oxygen flow of 4 l./min. We have analysed the concentration in six masks taken at random from stock. At an oxygen flow of 4 l./min. the observed concentration was 26.6% (S.D. 0.51); on reducing the oxygen flow to 2 l./min. the concentration fell by 0.15% (S.D. 0.27), and on increasing the flow to 8 l./min. the concentration rose by 0.45% (S.D. 0.26). There was little variation in composition

throughout the mask and the inspired CO_2 at all flows was virtually zero.

A Venturi can be made to give an adjustable oxygen concentration by throttling the air intake. This is rather too heavy a device to be incorporated in a mask, but can be connected to it. It also seems to work well in a small tent covering the head and shoulders and can also be used to control oxygen concentration in a standard oxygen tent. We do not yet know whether to recommend the production of a range of single-concentration masks or to use some other device to provide a range of concentrations. I suspect that the mask now available giving 27% oxygen will cover most requirements, but that another one giving a concentration of 24% will also have to be available; if so, it can be made. I now doubt if great precision will be required at higher levels. The problem is not technical or physiological but clinical: we need to know more about the response of patients.

Users of the existing Venturi mask should realize that, although it is not as good as the prototype, it is still better than other commercially available masks from the standpoints of oxygen concentration and CO_2 washout. I have, however, been at fault in not recommending Messrs. Oxyginaire to modify the claims made for its accuracy.

Finally, I must stress that these technical shortcomings should not obscure the facts that oxygen therapy must be continuous, not intermittent; that the oxygen concentration must be controlled, and that the easiest way of achieving this is to use the principle of high air-flow with oxygen enrichment.—I am, etc.,

MORAN CAMPBELL.

Postgraduate Medical School,
London W.12.

REFERENCE

- ¹ Campbell, E. J. M., *Lancet*, 1960, 2, 12.

Cystic Myxomatous Degeneration of Popliteal Artery

SIR,—In their interesting article on this unusual condition (October 5, p. 847) Mr. B. P. Bliss, Dr. J. Rhodes, and Professor A. J. Harding Rains raise two questions concerning its diagnosis from Buerger's disease, and its late prognosis, which can be partly answered from the findings in an unreported case of mine diagnosed clinically and operated upon a little over three years ago.

The cause of calf claudication in a young subject should be considered to be cystic degeneration if, as happened in this case, an arterial murmur can be heard on auscultation over the popliteal fossa and the patient is a non-smoker. These two findings are strong evidence against the diagnosis of Buerger's disease, which is almost unknown in non-smokers and which produces thrombotic occlusion of the distal arteries and not narrowing, as in atheroma or cystic degeneration.

The prognosis with operation should be good, for, as Mr. Bliss and his colleagues

point out, cystic arterial disease appears to be localized and unrelated to the more serious form which occurs in the thoracic aorta leading to dissecting aneurysm. Grafting the popliteal artery has not proved successful in long-term analysis, but local operations conserving the artery may be better, as Cockett and Maurice have found.¹ Evacuation of the cysts and simple suture-reconstruction of the popliteal artery should be effective unless aneurysmal dilatation should develop.

Mr. Bliss and his colleagues wished for information on cases operated upon several years ago: at three years my patient's condition remains satisfactory and he has full use of this limb despite a fractured tibia in the meantime.—I am, etc.,

London W.1.

H. H. G. EASTCOTT.

REFERENCE

- ¹ Cockett, F. B., and Maurice, B. A., *Brit. med. J.*, 1963, 1, 353.

Surgery of Peptic Ulcer

SIR,—I beg the courtesy of your columns to correct certain inaccuracies in reporting the views which I expressed (or believed I had expressed) in the course of the Panel Discussion on "Surgery of Peptic Ulcer" during the B.M.A. Annual Clinical Meeting at Stoke on October 20 (*B.M.J.*, November 2, p. 1116).

Firstly, I am prepared to consider the use of vagotomy with pyloroplasty as treatment for carefully selected patients with perforated duodenal ulcer—not, as inferred, for perforated gastric ulcer. Secondly, the only acceptable method of testing for completeness of vagal nerve section at the time of operation is that described and used routinely by Mr. Harold Burge. No more reliable method has been proposed for use in patients, and Burge's technique merits further evaluation. Thirdly, in reply to Mr. R. V. Cooke, who asked the Panel what should be done for the patient with haematemesis if nothing was found at laparotomy, I emphasized the need for wide gastrotomy and pyloro-duodenotomy. If nothing is found after the meticulous inspection of the gastro-duodenal mucosa afforded by this procedure, I would do nothing more—not, as reported, a pyloroplasty with vagotomy. Finally, while "sprouting" from the central cut end of the vagus has been beautifully demonstrated by Mr. Greig Murray and his colleagues, I am not aware that "regrowth" has been demonstrated (in the sense of restoration of vagal continuity, both anatomically and functionally).—I am, etc.,

Royal Infirmary,
Sheffield 6.

A. W. KAY.

Anticonvulsants

SIR,—I have a few comments to make about the most excellent article on anticonvulsants which you published under "Current Practice" on October 12 (p. 919).

Firstly, with regard to phenytoin you imply that it is difficult to obtain a parenteral preparation of phenytoin. Parke-Davis make a preparation, "epanutin parenteral," and this can be extremely useful in the treatment of status epilepticus in adults as well as in children. Hypertrophy of the gums occasionally can be a serious complication of phenytoin therapy in that it can become so gross that the drug has to be stopped.

Secondly, with regard to ethosuximide, skin rashes have been reported with this drug. I have seen a case personally, and the fact is stated in the manufacturers' literature, where they quote personal communications to the company and also by Lossius.¹—I am, etc.,

CHARLES A. SIMPSON.

Department of Neurology,
The Royal Victoria Infirmary,
Newcastle upon Tyne 1.

REFERENCE

- ¹ Lossius, H. M., *Acta psychiat. scand.*, 1961, 36, Suppl. 150, 200.

Diagnosis of Coarctation

SIR,—Dr. R. A. Strang's letter (November 2, p. 1129) has drawn attention to a necessary part of the examination of a child—feeling the femoral pulses. This is important in the diagnosis of coarctation, but may I suggest that the brachial pulse be palpated at the same time. About 25% of such cases do have femoral pulses,¹ but there is a significant delay in the latter which can only be appreciated by practice.—I am, etc.,

P. ROBSON.

Farnborough Hospital,
Farnborough, Kent.

REFERENCE

- ¹ Keith, J. D., Rowe, R. D., and Vlad, P., *Heart Disease in Infancy and Childhood*, 1958, p. 174. Macmillan, New York.

Cystic Fibrosis Group

SIR,—The Cystic Fibrosis Group was formed in October, 1960, mainly as a friendly link between parents of children with fibrocystic disease of the pancreas. We feel that the time has come to expand the group. Until now we have been mainly a social group, waiting for people to contact us. Now we feel that we can extend our work, and would welcome other parents who would like to join our group. We have a quarterly newsletter giving general news and having articles of interest for the parents.

The group is a happy association of parents and certainly not a "grumble group." It is characterized by a positive approach to the problems that we all share. We would be most grateful if your readers would tell any parents that they think would be interested in our group of our work. We would be pleased to welcome them into the group and send them newsletters. We have a limited number of past newsletters available on loan.—I am, etc.,

The Cystic Fibrosis Group,
3 Hawkridge Road,
Bridgwater, Somerset.

JUNE MEAD.