deliveries of suitable bread and biscuits can be arranged. (Welfare Foods (Stockport) Ltd., Swallow Street, Stockport, Cheshire.)—I am, etc.,

The Children's Hospital,
Sheffield 10.

K. S. HOLT.

REFERENCES

- ¹ Holt, K. S., *Acta paediat.*, 1963, **52**, 417.
- ² Blainey, J. D., and Gulliford, R., *Arch. Dis. Child.*, 1956, **31**, 452.
- ³ Allan, J. D., and Moss, A. D., in *Neurometabolic Disorders in Childhood*. Ed. K. S. Holt and J. Milner, 1964. Livingstone, Edinburgh.

Waldenström's Macroglobulinaemia

SIR,—The pace of scientific progress is now so great that "facts" tend to be disproved before they appear in print. In your leader (9 January, p. 73) you state that: "All three [immunoglobulins] are composed basically of two pairs of polypeptide chains." Franklin's¹ latest chemical studies (published in November 1964) suggest, however, that the "H chain" is not a single polypeptide as had been commonly assumed, but that it consists of two chains linked by an unusual bond. These latest observations support Comings's² model of immunoglobulin structure, which was based upon an analysis³ of genetic and autoimmune⁴ evidence. Cohen and Porter⁵ cite additional biological evidence in support of a 6-chain model. It is possible that classical (L-chain) Bence-Jones proteins which reveal homogeneous and heterogeneous peptide fragments may also prove to be complexes where the homogeneous portion corresponds to classical type I or II L chains, and the heterogeneous portion corresponds to Fd chains,⁶ which—according to the autoimmune evidence—should be encoded by X-linked genes.^{3, 6} The Fd chain is of the utmost biological importance because it contains the antibody-combining site, and it should therefore largely determine its specificity.¹ Complete agreement between the theoretical deductions from the autoimmune evidence and direct chemical observation of the basic structure of IgG globulins would appear to be imminent.—I am, etc.,

The General Infirmary,
Leeds 1.

P. R. J. BURCH.

REFERENCES

- ¹ Franklin, E. C., *J. exp. Med.*, 1964, **120**, 691.
- ² Comings, D. E., *Lancet*, 1963, **2**, 786.
- ³ Burch, P. R. J., *ibid.*, 1963, **1**, 1253.
- ⁴ Lawrence, J. S., *Ann. rheum. Dis.*, 1961, **20**, 11.
- ⁵ Cohen, S., and Porter, R. R., *Advanc. Immunol.*, 1964, **4**, 287.
- ⁶ Burch, P. R. J., Burwell, R. G., and Rowell, N. R., *Lancet*, 1964, **1**, 720.

Renal Hypertension and Polycythaemia

SIR,—On the basis of the interesting case which they describe, Dr. R. G. Luke and his colleagues (16 January, p. 164) suggest that where polycythaemia and hypertension occur together renal-artery stenosis should be considered, and this suggestion is further advocated in your leading article (16 January, p. 138). While accepting this recommendation, we wish to point out that in the presence of hypertension it is more desirable than usual that the diagnosis of polycythaemia should rest on measurements of total red-cell volume, as in Dr. Luke's patient, rather than on venous haematocrit and haemoglobin levels only. In a group of patients with uncomplicated essential hypertension it was found that

neither red-cell volume nor plasma volume showed a systematic deviation from normal values, but that venous haematocrit and whole-body haematocrit varied over a wider range than normal.¹ Moreover, we have studied three patients with essential hypertension in whom the venous haematocrit was 57%, equal to that of Dr. Luke's patient, but in whom the red-cell volumes were not elevated (25.3, 26.6, and 30.2 ml./kg.).

It is also noteworthy that in a patient with polycythaemia, hypertension, and unilateral hydronephrosis (Case 1 of Jones *et al.*)² nephrectomy was followed by disappearance of the polycythaemia but not of the hypertension. This result exemplifies the difficulties of forecasting the effects of nephrectomy in such a patient.

Hypertension and polycythaemia may also simultaneously complicate phaeochromocytoma.³ Both complications disappeared after removal of four phaeochromocytomas from the patient described by the latter authors. In addition to renal-artery stenosis a phaeochromocytoma should therefore be considered when polycythaemia and hypertension co-exist, and in either case the diagnosis of polycythaemia should depend preferably on measurement of red-cell volume.—We are, etc.,

N. F. JONES.

M. A. BARRACLOUGH.

W. F. CLAPHAM.

St. Thomas's Hospital,
London S.E.1.

REFERENCES

- ¹ Jones, N. F., Clapham, W. F., Barracough, M. A., and Mills, I. H., *Clin. Sci.*, 1964, **26**, 307.
- ² ———, Payne, R. W., Hyde, R. D., and Price, T. M. L., *Lancet*, 1960, **1**, 299.
- ³ Waldmann, T. A., and Bradley, J. E., *Proc. Soc. Exp. Biol. (N.Y.)*, 1961, **108**, 425.

Gas-cylinder Keys

SIR,—The group engineer's department has made for me a key which avoids the difficulties set out by Dr. H. Lister Wilson (19 December, p. 1591). It consists simply of a T made from mild steel pipe 7 in. (17.5 cm.) long in its lower limb, the end of which is shaped to fit the cylinder.—I am, etc.,

Hastings, Sussex.

HARRY MIDDLETON.

Code for Drugs

SIR,—Dr. W. A. L. Collier (26 December, p. 1657) suggests that the stilboestrol tablets in the Co-Tabs range should have been marked "SJ 2 . . . SJ 7" instead of "S 3 . . . S 20." I am told that, for technical reasons, his suggested markings would not have been possible on the smaller stilboestrol tablets. Manufacturing considerations aside, the code used on Co-Tabs was chosen because it was simple. A coded tablet may have to be identified by a general practitioner late at night in a country cottage, or by a tired hospital casualty officer in the early hours of the morning; and the code may have been little studied by them beforehand. Most of the systems of tablet coding that have been suggested in the past do not satisfy this criterion of simplicity; and neither does any system yet devised for identifying existing uncoded tablets.

Dr. H. J. Cronhelm (9 January, p. 129) says that all we need for tablet identification is a universal code number to be applied to the prescription label at the time of dispensing. Similarly Dr. W. A. L. Collier (26 December, p. 1657) suggests that a system of abbreviations for drug names should be used in the marking of tablets. I think both suggestions have merit, but who would, in fact, devise the universal code numbers for the labels or the drug name abbreviations? Who would ensure their use? And above all, when might usage be expected to start? The Co-Tabs system exists, is in everyday use, is simple, and it works. What more is to be looked for?—I am, etc.,

S. BRADSHAW,

Medical Adviser,

Arthur H. Cox and Co., Ltd.

Amersham, Bucks.

Bacterial Agglutinating Antisera and Immunofluorescence

SIR,—A word of warning should be given to those who may contemplate using routine diagnostic agglutinating antisera for the identification of micro-organisms by immunofluorescence.

Immunofluorescence detects antigens, but it does not differentiate between organisms having antigens in common. The diagnostic antisera issued by the Standards Laboratory are not cross-tested against organisms which are routinely excluded by means of initial biochemical tests, particularly the use of selective media. The content of antibodies to such organisms is therefore not known, nor the extent of variation of these antibodies from one batch of antiserum to another. Because of these limitations some antisera may prove successful in a study of this kind whereas later batches of the same antisera may not.

We issue bacterial diagnostic antisera in the belief that they will be used only in established agglutination tests. The intelligent use of the antisera in any other way requires full knowledge of the method of preparation. We therefore hope that anyone trying out new techniques will, in their own interest, let us know details of the investigations contemplated before applications for antisera are made.—I am, etc.,

PATRICIA BRADSTREET.

The Standards Laboratory for Serological

Reagents,

Central Public Health Laboratory,
London N.W.9.

Cephaloridine in Chronic Bronchitis

SIR,—The manufacturers of cephaloridine (Ceporin) state in the literature supplied with this antibiotic that the mean inhibitory concentration (M.I.C.) for *Haemophilus influenzae* is 2.0–3.0 µg./ml. and that the maximum levels obtained in the blood with the dose recommended is of the order of 12 µg./ml. Barber and Waterworth¹ showed that the M.I.C. of 12 strains of *H. influenzae* was 4–16 µg./ml., and Muggleton and his colleagues² that it was 1–8 µg./ml. for three strains. Stewart and Holt³ found the M.I.C. to be 0.1–5 µg./ml. with capsulated strains.

We have tested 52 strains of *H. influenzae* recently isolated from patients with acute exacerbations of chronic bronchitis, none of

whom had received cephaloridine. We used a Levinthal agar-dilution technique with an inoculum of 0.004 ml. of a 1:250 dilution of an overnight broth culture (10^4 organisms).

The Table shows that for 27 of the 52 strains the M.I.C. was above the level likely to be obtained with the recommended dose.

M.I.C. (μ g./ml.)	40	20	10	5	4	3	2	1	Total
No. of strains	6	21	13	1	3	6	1	1	52

If this drug is used for the treatment of respiratory infections due to *H. influenzae* it is possible that it may not always be effective.—We are, etc.,

Withington Hospital,
Manchester 20.

D. M. JONES.
P. DAVIS.

REFERENCES

- Barber, M., and Waterworth, P. M., *Brit. med. J.*, 1964, 2, 344.
- Muggleton, P. W., O'Callaghan, C. H., and Stevens, W. K., *ibid.*, 1964, 2, 1234.
- Stewart, G. T., and Holt, R. J., *Lancet*, 1964, 2, 1305.

Uricosuric Drugs in Gout

SIR,—Your correspondent in "To-day's Drugs" on the treatment of gout (16 January, p. 174) probably passes over the place of sodium salicylate in modern treatment because in these days of slick tablet therapy it has been out-moded, but it was the first uricosuric drug to be used and as it has been a therapeutic agent for over ninety years the statement that "uricosuric drugs have been in use for fourteen years" reads rather strangely. Moreover, it has a very effective uricosuric action in doses of 4–6 grammes daily,^{1–3} a dose which is much less irritant to the gastric mucosa than the same amount of aspirin daily, the only form of salicylate treatment of gout mentioned by your writer. I do not suggest that this form of therapy is preferable to modern drugs, but it is of interest to remember that nearly thirty years ago a more recent drug, cinchophen, had to give way to the oldest salicylate therapy when cases of liver poisoning had resulted from it. I feel sure that your writer, who has not forgotten his colchicine and local lead and opium applications, will not object to this brief excursion into the past.—I am, etc.,

G. H. JENNINGS.

Edgware General Hospital,
Edgware,
Middlesex.

REFERENCES

- Jennings, G. H., *Rep. chron. rheum. Dis.*, 1937, 3, 106.
- Marson, F. G. W., *Quart. J. Med.*, 1953, 22, 331.
- Jennings, G. H., *Rheumatism*, 1954, 10, 58.

Reactions to Intravenous Iron Dextran

SIR,—The article by Dr. Barbara E. M. Clay and her colleagues concerning reactions to the total-dose infusion of iron dextran (2 January, p. 29) is most interesting. However, the results are somewhat exceptional compared with ours and we would like to comment on them.

The incidence of acute reactions is 4.5%, while in our series one case occurred in 150 infusions to gynaecological patients; no acute reactions were noted in 50 obstetric cases.

Dr. K. N. Varde¹ reports one acute reaction in over 300 obstetrical infusions and the manufacturers have submitted to us on request the details of 1,807 fully documented cases of iron-dextran infusion from all over the country. Excluding the Hull series, the anaphylactic reaction rate is 0.28%. We all recognize the need to explain this difference, and the following suggestions may be helpful.

In our gynaecological patients the acute reaction and other minor reactions occurred early in the series, coincidental with the use of old stock iron dextran in use before intramuscular administration temporarily ceased in 1960. Change was made to currently manufactured Imferon, and since then the reaction rate has become negligible. It is interesting to note that Dr. Varde met with reactions at the outset of his trial, although he attributed this to a high concentration of iron dextran.

The incidence of folic-acid deficiency in pregnancy has risen—Hibbard² quotes 4.2% in the Liverpool area, Glasgow being 4%. Indeed 64% of pregnant patients infused here were folic-acid deficient and received folic acid prior to infusion. All Dr. Varde's cases received folic acid, but Dr. Clay and her colleagues do not refer to the folic-acid status of their patients, apart from stating that cases remaining resistant to oral iron appeared to be those with a folic-acid deficiency—a point which may be pertinent in view of our experience with the following case.

A para 6+1, age 31 years, gave a history of gross facial oedema, rash, and malaise developing to oral iron administered in another hospital in her previous pregnancy. She was warned to refuse iron thereafter. In her current pregnancy she was found to have a severe iron and folic-acid deficiency anaemia. Folic acid was given two weeks before commencing oral iron, and this time no reaction occurred.

Thus it is possible that in unsuspected folic-acid deficiency pre-treatment with oral iron prior to infusion, as in the Hull series, may trigger off a sensitivity reaction on occasions. This problem of hypersensitivity to drugs in pregnancy is a difficult one. Whilst the reactions here do not histologically resemble the Schwartzman phenomenon the reaction time after commencement of infusion might suggest some similar mechanism.

Though the points raised are debatable they merit investigation because the iron-dextran infusion is a valuable method of treatment and the causes of acute reactions need to be defined in order to minimize them.—We are, etc.,

R. S. LANE.

Research Department,
JEAN M. SCOTT.
Glasgow Royal Maternity Hospital,
Glasgow C.4.

REFERENCES

- Varde, K. N., *J. Obstet. Gynaec. Brit. Cwlth.*, 1964, 71, 919.
- Hibbard, B. M., *ibid.*, 1964, 71, 529.

Iodophors

SIR,—Your article in "To-day's Drugs" (12 December, p. 1513) was an informative critical evaluation of currently used anti-septics. In the section on iodophors, however, the writer stated that they are "inactivated by blood and serum and are probably of no value in the treatment of wounds." While this statement might be valid for some iodophors, published evidence indicates the contrary in the case of povidone iodine

(P.V.P.-I). The cultures of a variety of micro-organisms were killed within fifteen seconds by P.V.P.-I in dilutions up to 1:10, even when human blood or serum (5%) was added to a 4 mm. loopful inoculum.¹ P.V.P.-I was found to be effective in clinical cases of infected or bleeding wounds.² A study of 125 patients with infected wounds or ulcers and 70 with infected burn wounds has shown that P.V.P.-I (Betadine) markedly reduced the bacterial flora in 86% and 84% of the respective groups. The authors also pointed out that when necrotic tissue was present this had to be removed in order to obtain optimal effect.³

Thus, following the criteria of an ideal antiseptic postulated in your article, P.V.P.-I seems to be one that fulfils all of the requirements—i.e., rapid action, lack of toxicity, and resistance to blood, pus, and tissue fluids.—I am, etc.,

British Drug Houses
(Canada) Ltd.,
Toronto, Ontario,
Canada.

M. NADASDI.

REFERENCES

- Gershenfeld, L., *Amer. J. Surg.*, 1957, 94, 938.
- Shelanski, H. A., and Shelanski, M. V., *J. int. Coll. Surg.*, 1956, 25, 727.
- Connell, J. F., and Rousselot, L. M., *Amer. J. Surg.*, 1964, 108, 849.

Clofibrate and Androsterone in Hypercholesterolaemic Xanthomatosis

SIR,—Dr. Bernard Mason and Professor C. Bruce Perry in their article (9 January, p. 102) state, "A much less marked effect on the serum cholesterol was observed in two patients with hypercholesterolaemia and xanthelasma palpebrarum, but there was no change in the xanthelasma." The following case would support their findings:

A male patient aged 54 attended at Outpatients in March 1961 suffering from ischaemic heart disease. He had the characteristic yellow plaques on both upper eyelids. The result of his serum cholesterol was as follows:

Date	Serum cholesterol (mg./100 ml.)	
March 1961	536	
21 April 1961	564	
25 May 1961	446	
18 August 1961	444	
9 December 1961	478	
2 March 1962	458	
19 April 1962	410	
23 August 1962	486	
13 December 1962	512	
10 January 1963	492	} on Atromid
7 February 1963	442	
4 April 1963	417	
13 June 1963	424	
4 September 1963	446	
10 December 1963	388	
4 June 1964	554	

Eight capsules of Atromid daily was started on 13 December 1962 and continued for 12 months. There was no appreciable change in the level and no effect on the xanthelasma palpebrarum.

—I am, etc.,

Dorchester, Dorset.

EDWARD J. JARRETT.

Chlorhexidine and Suture Materials

SIR,—May I, through the courtesy of your journal, reply to Mr. A. W. Fowler's letter (23 January, p. 252) in which he recommends