as sufficient to contraindicate the use of the compound except for the treatment of conditions which themselves threaten life immediately, or cause such morbidity that only short survival may be expected. —I am, etc.,

G. E. PAGET.

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Imperial Chemical Industries Limited,
Macclesfield, Cheshire.

Psoriasis and Arthritis

SIR,—Dr. H. Baker and his colleagues in an article published in your journal (August 10, p. 348) in the first instance accept the mathematical proof that the association of psoriasis and arthritis occurs with a greater frequency than can be explained by coincidence. But, in the second instance, they state that the joint changes may precede the psoriasis by many years. No authoritative reference is given for this, neither do the authors cite such a case of their own. But, even if such cases have been seen, they must be very few indeed, and then the mathematics of probability would indicate the likelihood of coincidence, both psoriasis and rheumatoid arthritis being very common conditions. It is illogical to invoke the science of the laws of probability in one instance and ignore them in the other.

The authors then proceed to label some patients as psoriaform arthritis even though they did not have psoriasis, because they had arthritis of the hands with negative Rose-Waaler tests and some relative had this extremely common disease, psoriasis. About 20% of all cases of rheumatoid arthritis have negative serological reactions, although reasonable clinicians, who do not slavishly worship laboratory results, would consider them to be cases of rheumatoid arthritis. Because the negative serological results cannot be easily explained, this is no justification for introducing new nomenclature, such as psoriaform arthritis without psoriasis.

All this is on a par with the increasing frequency of reports of patients as cases of rheumatoid arthritis even in the absence of any clinical evidence of actual joint involvement, the diagnosis having been made on serological grounds. On this basis the frequent association of Hashimoto thyroiditis and rheumatoid arthritis has been reported although many of the patients have not had joint swelling. All this is illogical and obfuscating.

Are we not in danger of degenerating into the quackery whereby people are requested to send a sample of their blood without any medical details of symptoms or signs, and a diagnosis will be forwarded by return. If the diagnosis appears to be obviously wrong it can be claimed that, "in many years" to come, it may indeed turn out to be correct. Thus the mantle of Elijah has descended upon Dr. Baker and his colleagues.—I am, etc.,

London W.1. M. H. PAPPWORTH.

Treatment of Skin Tuberculosis

SIR,—In "To-day's Drugs" (October 19, p. 981), on the use of antibiotics in skin disease, I note that it is recommended that tuberculosis of the skin should be treated with isoniazid alone, unless there is a risk of infecting others by the shedding of resistant organisms from ulcerated areas.

Surely this misses the point? I understand that skin lesions treated with isoniazid alone tend to improve for three to four months and then to deteriorate again. It is also well known to bacteriologists that tubercle bacilli tend to become resistant to isoniazid when given alone in just that period of time. The implication of this is obvious, and if those who treat tuberculosis in other parts of the body know that they must always use two efficient antituberculous drugs at once, why should dermatologists be the exception? If this is current practice in dermatology, then it surely needs reappraisal.—I am, etc.,

General Hospital, Rochford, Essex. R. G. BENIANS.

*** We showed Dr. Benians's letter to our expert contributor, who replied as follows: Although Dr. Benians is correct in his assumption that it is unwise to use only one antituberculous drug, in practice lupus vulgaris of the skin responds remarkably well to isoniazid alone, and the criticism appears to be a theoretical one. Russell and Thorne[1] showed that 99 of 103 lupus patients treated with isoniazid alone achieved clinical cure, and concluded that isoniazid resistance in lupus was unusual. This is supported by Wehnert and Marcussen,[2] who reported a cure rate of 92.5% in 168 lupus patients treated with isoniazid. Resistance to isoniazid occurred in one patient only, and the addition of P.A.S. did not shorten the time of treatment.—ED., *B.M.J.*

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1 Russell, B., and Thorne, N. A., *Lancet*, 1956, 2, 808.
2 Wehnert, R. A., and Marcussen, P. V., *Acta derm. venereol. (Stockh.)*, 1961, 41, 461.

Anti-epileptic Drugs and the Foetus

SIR,—With reference to the question (October 19, p. 983) concerning the effect of phenytoin sodium and phenobarbitone on the foetus when the drugs are being taken by an epileptic mother, the reply of your expert stated that there is no evidence that at birth any baby has suffered deleterious effects in this way. On October 21, 1963, we admitted to this hospital a baby, 3 days old, in which there was strong evidence that it was suffering from the effects of these drugs, which the epileptic mother had been taking for many years in doses of phenytoin sodium 1½ gr. (0.1 g.) b.d. and phenobarbitone 1 gr. (65 mg.) b.d.

The baby boy was born in an outside maternity unit, and both pregnancy and the delivery had been normal except for the fact that the cord was tightly round the neck and had to be cut before the baby was born. It was found when birth took place that the baby had sustained a small nick of the skin of the neck from which there was a steady ooze of blood. Bleeding continued for the next 24 hours, despite the administration of Vitamin K, and the next day the baby developed a fairly severe epistaxis, when it was transferred to this hospital. On admission the baby weighed 7 lb. (3.2 kg.) and was not jaundiced. The small nick in the neck was still oozing slightly, and there was also some epistaxis. There was a well-marked cephalohaematoma and numerous small petechial haemorrhages on the head and neck, and also some purpuric areas on the trunk. There was no bleeding from the cord nor any evidence of melaena. Blood examination showed: haemoglobin 40% (5.84 g.), red blood cells 1,400,000/c.mm., white cells 14,000/c.mm., platelet count 20,000/c.mm. The blood film showed a macrocytic picture with the presence of occasional megaloblasts. The mother's blood group was O Rh-positive and that of the baby O Rh-negative. The direct Coombs test was negative. No urine was collected from the baby, but it was noted that the nappy staining was a similar orange colour to the phenytoin elixir used for epileptic children.

It was decided that the thrombocytopenic purpura and macrocytic anaemia were due to the phenytoin and phenobarbitone from the mother, and no treatment was given in the hope that once these had been excreted recovery would rapidly take place, and this in fact has happened. Two days later the haemoglobin had risen to 56% (8.18 g.) and the platelets to 100,000/c.mm. The blood picture was still macrocytic. At the time of writing—i.e., one week from birth—the haemoglobin has risen to 65% (9.49 g.), red blood cells 3,000,000/c.mm., platelet count 200,000/c.mm., and the blood picture is rapidly reverting to normal and the child is recovering rapidly.

The mother has one other child, aged 4, and there is a history of prolonged bleeding from the cord after birth in its case.

I should like to thank Dr. S. J. R. Macoun for clinical details.

—I am, etc.,

A. LAWRENCE.

Pathological Laboratory,
St. Luke's Hospital,
Guildford, Surrey.

Hand, Foot, and Mouth Disease

SIR,—We would like to draw attention to outbreaks of "hand, foot, and mouth" disease which are occurring in different parts of the country at the moment.

The condition is a mild one, and constitutional upset, if present, is slight, and symptomatic treatment only is required. It is due to a Coxsackie A-type virus. The eruption occurs on the hands and feet, particularly on the fingers and toes, and consists of red papules with very characteristic, central, flaccid, grey-white blisters which vary in size from 2 to 10 mm. and in number from 1 to 50. Small ulcers, usually few in number, occur on the lips and tongue, and in the mouth generally, and may have the appearance of aphthous ulcers. The whole condition settles in one to two weeks. Most cases occur in children, but all members of a family may quickly become affected.

The condition was first described in this country by Alsop, Flewett, and Foster,[1]

and a small outbreak was recorded in Bristol in 1961.[2] A week ago two cases were seen in Bristol, and within the last ten days at least 20 cases have occurred in a small area of Buckinghamshire. A very similar outbreak has occurred in Swindon involving about the same numbers.

Recent observations suggest that some patients in these outbreaks may only suffer from mouth ulcers, and this fact and the mildness of the condition may not cause it to be recognized immediately. It seems, however, that the disease may well be occurring in many different parts of the country, and it would be of interest to hear of further outbreaks that have occurred.—We are, etc.,

Swindon, Wilts. K. D. CROW.
Bristol. R. WARIN.
Amersham, Bucks. D. S. WILKINSON.

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1 Alsop, J., Flewett, T. H., and Foster, J. R., *Brit. med. J.*, 1960, **2**, 1708.
2 Flewett, T. H., Warin, R., and Clarke, S. K. R., *J. clin. Path.*, 1963, **16**, 53.

*** This disease was the subject of an annotation in the *Journal* of May 25, 1963 (p. 1363).—ED., *B.M.J.*

Persistence of Barbiturates

SIR,—Your leading article "Persistence of Barbiturates" (September 21, p. 695) gives an erroneous impression. The patients described by J. M. Hinton,[1] whom you cite, were psychiatric cases. In spite of some being less severe clinically than others Hinton by means of an 8×8 Latin square determined, to quote his words, that "there was no follow-on effect from night to night." He used amylobarbitone sodium, butobarbitone, and quinalbarbitone sodium. He found there was no significant difference between these, in onset or duration of hypnotic action. Although phenobarbitone is cumulative,[2 3] it seems clear from Hinton's work that your statement that "there is little or no evidence to justify the use of a sedative night after night for the treatment of insomnia" is not supported by the worker to whom you yourself refer.—I am, etc.,

G. de M. RUDOLF.
Mount Pleasant Nursing Home,
Clevedon, Somerset.

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1 Hinton, J. M., *Brit. J. Pharmacol.*, 1961, **16**, 82.
2 Butler, T. C., Mahaffee, C., and Waddell, W. J., *J. Pharmacol. exp. Ther.*, 1954, **111**, 425.
3 Hinton, J. M., *Brit. J. Pharmacol.*, 1963, **20**, 319.

Antihistamines and the Ovum

SIR,—My attention has been drawn to a letter which appeared in the March 30 issue (p. 887) under the title "Antihistamines and the Ovum," written by Dr. B. E. Finch.

The role of histamine in nidation, specifically in the induction of decidualization, has been explored mainly in the rat, and to a lesser extent in the monkey, guinea-pig, and rabbit. The administration of antihistamine drugs to rats by systemic routes does not result in the suppression of decidualization, since the absorption and distribution of these drugs is such that extremely little (if any?) reaches the endometrium. The suppression of decidualization by antihistamines is achieved only by topical application.

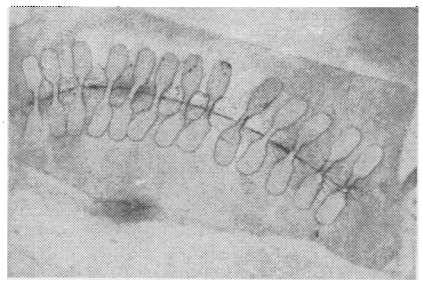
There is no evidence of which I am aware that the ordinary clinical use of antihistamines influences nidation in a deleterious way. If an antihistamine with a predominant absorption by the uterus could be designed, it might possibly be a fertility control agent—but such a drug would have to be synthesized first and then tested.—I am, etc.,

M. C. SHELESNYAK.
Weizmann Institute of Science,
Endocrine and Reproduction Physiology,
Rehovoth, Israel.

Sutureless Skin Closure

SIR,—I write in support of the claims made for sutureless skin closure by Mr. N. G. Rothnie and Mr. G. W. Taylor, and by Mr. P. J. B. Murray, in their excellent articles (October 26, pp. 1027 and 1030). I have used this principle for the past two years in about 100 cases and I can confirm their views on the advantages of this method.

These articles both give the impression that the main factor in success is the use of a special surgical tape. I have used only conventional adhesive strapping



(Dalmas' "dumb-bell" and Johnson and Johnson's "butterfly" dressings) and I think the results are just as good. The accompanying photograph of the upper abdomen was taken on the ninth post-operative day after vagotomy and pyloroplasty. With all due respect I submit that the type of strapping is of minor importance, provided that it sticks well and air-spaces are left between the strips.—I am, etc.,

London W.1. GEORGE QVIST.

Sutures and the Burst Abdomen

SIR,—In his analysis of the burst abdomen Dr. J. R. Hampton (October 26, p. 1032) states that there is no evidence that the type of suture material used is of any importance. This is an altogether unjustifiable conclusion from the evidence he adduces. Apart from one case in which silk was used there were equal numbers of wounds sutured with nylon and catgut. Since both these materials have a low knot-strength this factor may be the main cause of wound dehiscence. A striking omission in the analysis is the lack of any details as to the state of the suture materials at the time of secondary repair. The only information on this vital point was that concerning seven patients whose wounds separated in the first three days. Two were said to have knot failure and in two the suture material broke, but no indication was given as to the type of suture material used.

Wound-bursting is by no means confined to the abdomen, but its effects in this region are dramatic and compelling. In orthopaedic wounds in which the deep layers are repaired with catgut the patient may complain of sudden pain several days after operation due to giving way of deep sutures. The bulge on the outer side of the knee following repair of meniscectomy wound with catgut is a familiar sight.

For over 15 years I have used nothing but plain cotton for wound repairs, and during this time I can only recall one patient in whom a buried suture had to be removed. Cotton is the ideal suture material because it is non-irritant and has a very high knot-strength. I am convinced that if interrupted cotton were used in place of catgut and nylon wound-bursting would become extremely rare. There must be a few abdominal surgeons who use interrupted cotton. If so, perhaps they could give us their experience of burst abdomens.

Finally, it should be emphasized that cotton must not be confused with linen thread. If the surgeon asks for "thread" while operating he will almost invariably be offered linen thread, but this material has a bad reputation for sinus formation and has no advantages over cotton.—I am, etc.,

A. W. FOWLER.
Bridgend, Glamorganshire.

Fluothane and Jaundice

SIR,—In the past few months you have published a great deal about jaundice and fluothane; may I report a further case of this?

A female patient, aged 34, received two uneventful exposures to fluothane six weeks apart. One week after the second she developed dark urine, pale stools, itchy skin, and jaundice. She had a smooth, sharp-edged, slightly tender hepatomegaly; liver-function tests and transaminases showed hepato-cellular damage. Alkaline phosphatase remained normal. She had evening pyrexia for the next week and then developed anorexia with nausea and vomiting, leucopenia with relative eosinophilia, and a drop both in haemoglobin and in plasma proteins (especially albumin).

She responded dramatically to steroids and was physically and biochemically well in eight weeks. No cause was found for her jaundice. There was no evidence of co-existent liver, renal, or pancreatic pathology, which one would expect, since jaundice in these cases is normally associated with hypoxia during a single fluothane anaesthetic, and carries a bad prognosis.

The double exposures and time relationships, together with the good response to steroids, form a pattern suggestive of a "sensitivity reaction" similar to that which has been reported with other