Correspondence

Letters to the Editor should not exceed 500 words.

Deaths from Smoking

SIR,—Your leading article of 6 June (p. 1451) comments on remarks which I made recently. I would like to take this opportunity to answer some of the points raised.

It was not my intention to deny or to minimize the association of cigarette-smoking to the incidence of bronchial carcinoma. I agree that the frequency of this disease is reflected in the extent of individual cigarette consumption.

I recognize that although the development of bronchial carcinoma is materially less in light smokers than in heavy smokers the evidence available shows that smoking cigarettes, even in small numbers, adds to the hazard in comparison with non-smokers.

I would like, therefore, to make it clear that the views of the Imperial Cancer Research Fund on this subject do not differ from those expressed in the reports of the Medical Research Council, the Royal College of Physicians, and the Surgeon General of the United States, which have been reinforced more recently by the latest publications of Dr. Richard Doll and Sir Austin Bradford Hill in the *Journal* on 30 May and 6 June (pp. 1399 and 1460).—I am, etc.,

CECIL WAKELEY.

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Typhoid Fever Due to Salmonella typhi, Phage Type 34—An Isolated Case in 1955

SIR,—The present epidemic of typhoid fever in Aberdeen, where the infecting organism has been shown to be due to *S. typhi*, type 34, makes the following case of interest, since in 1955 no other case of typhoid fever due to this type had been isolated. Moreover, no known secondary case occurred after this particular case.

The patient was a married woman aged 59, who was admitted to Gateside I.D. Hospital, Greenock, from her home in the adjoining town of Port Glasgow on 3 October 1955. She had taken ill seven days previously with symptoms of slight malaise, anorexia, slight vomiting, and, during the two days before admission, diarrhoea, without abdominal discomfort. Her general practitioner requested her admission as a case of possible dysentery.

She was examined on admission by the late Dr. John Reid, who had been a well-known consultant physician in infectious diseases in the west of Scotland. Dr. Reid recorded that she was mildly ill but febrile on admission. Examination then revealed no evidence of pulmonary disease and there was no suggestion of any heart disease. Neither was there any rash or splenomegaly. Indeed, the only condition present seemed to be rheumatoid arthritis (present for some years) and fever.

During the first seven days in hospital she remained febrile, the temperature being inter-

mittent—97° F. to 102° F. (36.1°–38.9° C.). Constipation was a feature during her first few days in hospital, but thereafter diarrhoea recurred, the motions being brown and watery. Blood cultures during this period were sterile and the Widal reaction only positive at H 1/50. Daily faecal specimens were negative on culture for intestinal pathogens from 4 October until 10 October. On 10 October the patient became rather toxic-looking and rose spots appeared over the trunk. At this stage too the spleen was definitely enlarged. A clinical diagnosis of typhoid fever seemed obvious and treatment with chloramphenicol was instituted. A stool specimen on the day after this diagnosis had been made was positive for *S. typhi*. The treatment with chloramphenicol was quickly effective and the patient seemed to be making a full recovery, but on 3 November (four weeks after admission) she became febrile again and diarrhoea and vomiting recurred. Moreover, she complained of abdominal discomfort and there was some distension. This time, however, the rash did not reappear and the spleen was not palpable. However, since recrudescence seemed likely, a further course of chloramphenicol was prescribed. Again there was a good response and the patient had an uneventful convalescence, being discharged to her home on 26 November 1955 after a total stay in hospital of 54 days. *S. typhi* had only been isolated twice and that from faecal samples on 11 October and 25 October. All later faecal samples were negative, as were six serial samples of urine.

A culture of this *S. typhi* was submitted to the Central Public Health Laboratory at Colindale for phage typing, and the report, issued on 4 November 1955, stated that the specimen belonged to phage type 34. This was the first time that this type had ever been encountered by that laboratory in this country.

In view of this result we made some inquiries about our patient's movements. First of all, she was emphatic that she had never been outside this country. She lived with a son and daughter in a three-apartment, prefabricated house in Port Glasgow, where the sanitation was good. She was the only member of the household to be ill. The only powerful source of infection might have been dulse, a form of dried seaweed sent to the patient from County Antrim in Northern Ireland. The patient ate some of this on 1 September and 2 September, and on 3 September she was admitted to the Glasgow Western Infirmary for treatment of her rheumatoid arthritis. During her stay, over a period of 22 days, she had no symptoms referable to gastro-intestinal infection. However, on the day of her discharge from the Western Infirmary (25 September) she ate some more dulse and five days later developed symptoms of diarrhoea, vomiting, and fever, which prompted her doctor to request her admission to hospital.

The first batch of dulse had been obtained from Ballymena, County Antrim, and the second from Port Fach, also in County Antrim. Northern Ireland authorities stated that in 1955 there had only been 10 cases of typhoid fever in that country and one carrier had been detected. All 11 of these cultures had been sent to the Colindale Laboratory for phage typing, four belonging to type A, two to type O, and five were untypable. Moreover, they had no knowledge of dulse ever being responsible for the spread of typhoid fever and it was considered that dulse was a highly improbable medium for the growth of typhoid organisms.

The interesting features of this case are:

(1) The isolation of a phage type of *S. typhi* not previously known in this country.

(2) No further cases occurred and the other two members of this family were free of infection.

(3) If the patient did not contract her infection in the Glasgow Western Infirmary then she must have contracted it within the six days after her discharge from that hospital, because her symptoms had commenced at the end of that short time. Regarding this point, it should be noted that the two students who were among the earliest victims of the Aberdeen epidemic developed their symptoms five days after eating the suspected corned beef, and on the sixth day they had gastroenteritis.

Finally, this case has remained on my files as an interesting case, but in view of the recent epidemic it seems to merit an "airing."
—I am, etc.,

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ROBERT LAMB.

Hepatitis from Corned Beef?

SIR,—According to current theory it seems possible that outbreaks of typhoid can be caused by faults in the techniques of canning meat. The particular hazard which is cited is that after canning the tins are cooled in raw river water or in river water that has not been chlorinated. The seam of a large tin may be opened slightly during heating; it would close again during cooling, but might admit a small amount of water first and so allow organisms to enter. *Salmonella typhi*, it is suggested, could then grow out and infect the whole tin. If this theory is correct the field seems open for ingress of any other pathogen, and in particular enteric viruses, e.g., polio and infectious-hepatitis virus—the latter certainly would be expected to be plentiful in a large, sewage-contaminated river.

Apart from survival in raw river water the hepatitis virus is not apparently very susceptible to chlorination. If a large bacterium like *S. typhi* can get into tins, how much more likely that a very small virus can do so. This virus could presumably survive in the anaerobic conditions in the tin. Most people will be wondering how often batches of tins are cooled in raw water: has it often happened in the past?

Is this an unsuspected source of hepatitis virus and other enteric viruses? In the large hepatitis outbreak which Bristol had during 1960 to 1963 we did not ask about corned beef in any form—in the light of current information it seems that we should have. We have no evidence at all that corned beef, sliced or otherwise, had any role in introducing hepatitis here. Once hepatitis is established there seems little doubt that it spreads by interpersonal contact. But how does it get established in the first place? If the thesis about the contamination of corned beef and other canned food by raw river water is

correct, it seems to open up a new dimension in the epidemiology of enteric virus infection.—I am, etc.,

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Salmonellae in Canned Foodstuffs

SIR,—In your leader on the typhoid epidemic (6 June, p. 1453) you say, "The suspicion that canned meat may be contaminated with *Salmonella typhi* is supported by strong circumstantial evidence. Nevertheless, the organism has never been isolated from a suspected can." That related organisms have been isolated from other and less likely canned food products, however, the following episode will demonstrate.

Some years ago four persons turned up at a London meals centre for lunch. They were late, and as there was no green vegetable left the cook heated up a can of pre-cooked spinach. Two of the lunchers refused it, a third had a moderate helping, while the fourth helped himself most generously. A few hours later this man developed a violent attack of gastro-enteritis; the second partaker of the spinach had a later and milder attack. The two who had eaten no spinach were unaffected.

On investigation neither can nor spinach was available. I therefore asked that a random sample of six tins from the same batch of spinach should be sent for laboratory examination. Two of these gave a good growth of *S. paratyphi* B.

The canning had been done by a very reputable British firm. How the organisms obtained entry to the cans in the first place, and how they seemingly proliferated in such an unpromising medium, was never satisfactorily explained. I feel, however, that the episode is worth recording as yet another illustration of the rugged tenacity of life so characteristic of the typhoid group.—I am, etc.,

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Isleworth, Middx.

G. E. BREEN.

Blood Loss in Paediatric Surgery

SIR,—Your leader "Blood Loss in Paediatric Surgery" (9 May, p. 1201) draws attention to an important problem and gives much good advice. However, I believe that some important points are understressed or omitted.

In mentioning that blood-transfusion was considered dangerous 40 years ago but that since then considerable advances have been made in techniques, the editorial tempts the reader to conclude that the administration of blood is safe and that the main danger lies in not giving enough of it. Blood transfusion is still hazardous and there is growing evidence that it is a more risky business than we thought it to be 10 or 20 years ago. Apart from the obvious errors of accidental air-embolism and mismatching, viral hepatitis is still a real threat. Allen,¹ in a careful follow-up survey, showed that the over-all hepatitis attack rate is over 3%. Another problem is that the low free-calcium of banked blood caused by the use of citrate as an anticoagulant,² together with its high serum-potassium levels, can seriously weaken the heart's action if blood is administered rapidly. The danger of cardiac depression is thought to be further increased if the blood is cold or

if hypothermia exists.³ This is more likely in infants and small children, since they easily develop unsuspected hypothermia during anaesthesia and operation because of their relatively large surface-area.

The suggestion to set up an intravenous drip and to have matched blood available before major paediatric surgery is a wise one, but the statement that "in the newborn baby almost any surgical procedure is apt to cause sufficient loss of blood to demand transfusion" must be challenged. With careful technique and use of the diathermy for rapid haemostasis it is often possible to do safe surgery on the newborn without transfusing blood. This applies not only to procedures such as herniorrhaphy but also to major operations such as ligation of a patent ductus arteriosus, or banding of the pulmonary artery. As for the assessment of blood loss we still badly need a reliable method; the rapid estimation of blood volume, while promising for the future, is as yet far from being a practical tool to the anaesthetist and the surgeon. In addition to the methods mentioned in the editorial, the use of a small accurately graduated glass cylinder as the suction receptacle is sometimes helpful if the use of gauze swabs can be minimized.

Finally, the editorial fails to discuss the alternatives to blood transfusion. Plasma may often be life-saving, yet it carries less risk. If it is stored in the liquid form at 32° C. for six months the virus of hepatitis dies.⁴ More recently Shires⁵ has offered a convincing argument that the "shock" of trauma and haemorrhage is in large part due to internal shifts of large volumes of extracellular fluid, and that when these losses are compensated by the infusion of a solution resembling extracellular fluid the hypotension is frequently prevented or reversed. Clinical experience with the administration of fairly large amounts of a lactated Ringer's solution during major operations has tended to bear out Shires's theory and has markedly reduced the number of blood-transfusions given in many American surgical centres.—I am, etc.,

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* Dr. Vernon Thomas is quite right in drawing attention to the dangers of blood-transfusion and to the alternative methods of restoring the circulatory blood volume. The risk to the cardiac muscle of cold blood with a high serum-potassium level is particularly important when the blood is being injected through an umbilical catheter, the tip of which may be lying in the right atrium. While it is quite true that with scrupulous attention to haemostasis even extensive procedures can be undertaken in the newborn without serious blood loss, the leading article said, "apt to cause sufficient loss of blood to demand transfusion," and the emphasis was on being prepared to transfuse blood and not being forced into the position of having to give a rapid large transfusion.—Ed., *B.M.J.*

Fatal Poliomyelitis after Homoeopathic Vaccine

SIR,—Like other countries in Europe, Italy has carried out poliomyelitis vaccination programmes in the past with Salk vaccine and more recently with Sabin oral vaccine. While the Sabin vaccine has been prepared by a local firm, the Salk vaccine made available to our general practitioners has been either imported or produced locally. Recently, however, a fatal attack of poliomyelitis in a 5-year-old child who attended the Institute of Paediatrics in Rome has brought to our notice a third, imported, poliomyelitis preparation. This vaccine, with which the child had been "vaccinated" three years previously, is a homoeopathic preparation of British origin and it consists of formalized poliomyelitis virus of Types I, II, and III incorporated in a lactose tablet. It is for oral administration under the directions of a physician.

We understand that homoeopathic poliomyelitis vaccines are not licensed for sale in Great Britain and we have been unable to find in the literature any proof of the efficacy of homoeopathic poliomyelitis vaccines such as may be obtained by properly conducted clinical trials. It is the purpose of this letter, therefore, to draw attention to the existence of homoeopathic poliomyelitis vaccine and to the fact that neither the safety nor the potency of this vaccine is officially controlled.—We are, etc.,

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G. MARZETTI.

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Treatment of Cystinuria

SIR,—In your leading article on the treatment of cystinuria with penicillamine (30 May, p. 1396) you warn that this compound may cause nephrotoxic reactions and quote an article in the *American Journal of Medicine*¹ in support of this contention. Unfortunately, as I have pointed out previously in these columns (3 August 1963, p. 322) there is a widespread failure to realize that there is a differential toxicity between D and L penicillamine, and the authors in the article quoted unfortunately fail to state which isomer they used. Until very recently DL penicillamine was the most readily available and widely used form in the United States, and it is a reasonable assumption that it was the L isomer in racemic penicillamine which caused nephrotoxicity. I have records of 40 patients with Wilson's disease treated with D-penicillamine for periods from seven months to nine years, and I have yet to see a nephrotoxic reaction.

The therapeutic usefulness of penicillamine in the management of cystinuria has been called into question by Eldjran and Hambræus.² These authors confirmed the findings of Dr. Crawhall and his colleagues (*B.M.J.*, 2 March 1963, p. 588, and 30 May 1964, p. 1411) that the administration of penicillamine led to the excretion of a penicillamine-cystine mixed disulphide, but they also reported a simultaneous increase in the excretion of cystine which is in keeping with the observations of Hartley and Walshe,³ who showed that the administration of penicillamine to two patients with Wilson's disease resulted in a tenfold excretion of half cystine in the urine. Furthermore, Eldjran and Hambræus were unable to demonstrate that