or wished that research on antibiotic sensitivity testing would cease with the publication of the I.C.S. report. On the contrary, we recognized the need for further regional and collaborative studies. The I.C.S. working group deplores that no international body has offered to organize continuation of the work.

In international discussions, however, it is essential that previous work and recommendations arising therefrom be correctly quoted. This applies equally to practical experience acquired after publication of the I.C.S. report which has largely been based on this report and is well known to leading British bacteriologists. Your leading article has regrettably, failed in these respects.—I am, etc.,

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¹ Ericsson, H. M., and Sherris, J. C., *Acta Pathologica et Microbiologica Scandinavica*, 1971, Supplement No. 217.

² Bauer, A. W., et al., *American Journal of Clinical Pathology*, 1966, 45, 493.

** We cannot accept the charge of misunderstanding or incorrect quotation in regard to the method of interpretation of this type of test. That "the translation of inhibition-zone diameters directly into categories of sensitivity" is "by reference to tables" is strictly true of the American version of the method. The substitution of "charts" for "tables" makes it equally true of the I.C.S. method. The Swedish maker of test discs referred to by Professor Ericsson provides a "zone-size interpretative chart," which is a series of scales for individual antibiotics from which zone diameters can be read off as indicating one of four categories of sensitivity or resistance. It is true that these scales also indicate "break points" in M.I.C. and that the basis of the proceeding is as described by Professor Ericsson, but the direct translation which his letter denies is the evident intention of this chart.—ED., *B.M.J.*

Data Sheet Compendium

SIR,—The *Data Sheet Compendium 1974*, issued free to the medical and pharmaceutical professions by the Association of British Pharmaceutical Industry, is a most helpful document and I am surprised that your correspondence columns have not been filled before now with congratulatory letters from medical users.

Perhaps users of the compendium and official bodies such as the Joint Formulary Committee could make occasional suggestions and help the compilers maintain the very high standard they have set themselves. I should like some of the drug information to be expanded. For example, the entry for Warfarin WBP merely states that a list of factors influencing anticoagulant requirements is available on request, whereas the makers of Dindevan are much more generous in giving a list of drugs and other factors which may effect anticoagulant therapy. I should also like to see an index of approved names as well as the proprietary names, so that one has two ways of finding the excellent information contained in the book.

Incidentally, I am now only prepared to see drug representatives from firms who publish within the compendium, and I feel

this is wholly commendable bit of market pressure.—I am, etc.,

RONALD LAW

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Larrey and Débridement

SIR,—Mr. D. H. Patey (14 September, p. 686) believes I have "fallen into the error of regarding débridement as carried out by Larrey with the procedure as now practised." He credits Larrey with establishing the importance of one aspect only of débridement—namely, the "unbridling" (literal translation) of constricting tissues and implies that the removal of tissue debris had to await knowledge of microorganisms. However, I maintain that Larrey *did* practise a surgical treatment of wounds that can quite justifiably be termed débridement in the modern sense—though what other surgeons may have done or not done is another matter entirely. He did not need to know about bacteria; he just knew that wounds failed to heal if they were not cleaned up surgically. I think a browse through Larrey's case histories makes this abundantly clear.

Mr. Patey's quotation from p. 13 of my book¹ does not describe Larrey's practice but comes from my paraphrase of Desault's teaching which was deliberately modern in tone so that I could define débridement for the non-medical reader. I then continued, "Larrey at once appreciated its significance and profited greatly from the lesson." Larrey wrote in his *Memoirs*,² "Desault taught us, that in order to change the nature of wounds, from a complicated to a simple state, it was not sufficient to make the part bleed: that in order to attain this end, it was necessary to remove the bruised edges with a sharp knife, and then to unite the wound with a suture; and that this method is practicable only in wounds of the face, and in solutions of continuity of the soft parietes of the mouth. In my campaigns in Germany and Egypt, I have profited by the practical lessons of this man of genius, who appears to have here made one of the most important discoveries in surgery" (p. 19). (I agree with Dible³ that the restriction of the method to wounds of the face and mouth refers only to immediate suture and not to the whole surgical procedure. This is, in fact, obvious from Larrey's case histories.)

Unfortunately Mr. Patey has missed another of Larrey's comments. In *Observations*⁴ (an English translation of the first volume of *Clinique Chirurgicale*) Larrey wrote on the subject of lacerated wounds: "The first indications to be fulfilled consist in making divergent incisions through the integuments which form the edges of the wounds, through the fibrous sheaths and through the aponeuroses; in cutting out all those disorganized shreds of muscle or tendon which protrude beyond the level of the solution of continuity, and in tying as much as possible all vessels which are visible, as well as those after which search may be made without danger" (pp. 8-9). Pages 28-34 of *Observations* deal with the importance of removing foreign bodies and give three successful case histories where clothing had been driven in with the bullet to illustrate the point.

Finally we come to Mr. Patey's comments on Larrey's dressing technique (*pancement*, if you will). This was, in fact, used by Larrey whenever appropriate and not just for wounds that required no surgery. It is described and placed in perspective on p. 242 of my book (note 3 to chapter 3). Moreover, alone it was not the forerunner of Trueta's practice. Trueta and Larrey both emphasized immobilization; the former with plaster of Paris,⁵ the latter with his rigid splint (*appareil inamovible*) which he used as early as the Egyptian campaign.

Larrey's treatment of General Claude Pajol at Borodino (related on p. 161 of my book) brings all this to a focus. Pajol received a gunshot wound of the left forearm. Both bones were broken and the fractured ends fragmented. Larrey débrided the wound, including the removal of all fragments of bone not attached to periosteum, dressed it, and put the forearm in an

appareil inamovible. Any other surgeon at that time, and in the turmoil of battle, would certainly have amputated, but Larrey achieved a result that both anatomically and functionally was excellent. Can there still be doubt?—I am, etc.,

ROBERT RICHARDSON

Shortlands, Kent

- ¹ Richardson, R. G., *Larrey: Surgeon to Napoleon's Imperial Guard*. London, Murray, 1974.
- ² Larrey, D. J., *Memoirs of Military Surgery, and Campaigns of the French Armies*, trans. R. W. Hall, 1st American edn. from 2nd Paris edn. Baltimore, Cushing, 1814. (I have intentionally quoted from contemporary translations rather than the original French in order to avoid the possibility of error introduced by modern interpretation.)
- ³ Dible, J. H., *Napoleon's Surgeon*. London, Heinemann, 1970.
- ⁴ Larrey, D. J., *Observations on Wounds, and their Complications by Erysipelas, Gangrene and Tetanus, and on the Principal Diseases and Injuries of the Head, Ear and Eye*, trans. E. F. Rivinus. Philadelphia, Key, Mielke, and Biddle, 1832.
- ⁵ Trueta, J., *Abbotempo*, 1970, book 1, p. 30.

Complication of Proximal Gastric Vagotomy

SIR,—With reference to the letters from Mr. J. H. Wyllie (8 June, p. 561) and Mr. J. F. Newcombe (10 March 1973, p. 610) we report a further fatality from ischaemic necrosis and subsequent perforation of the lesser curvature of the stomach after highly selective vagotomy for duodenal ulcer.

The patient was a fit 42-year-old man with a four-year history of indigestion. He had had a melaena in 1973 and another in early 1974 which needed 3.2 l. of blood replacement. Gastroscopy and a barium meal showed an ulcer on the anterior wall of the duodenum. The bleeding stopped but at laparotomy on 12 June 1974 slight thickening and reddening were found on the anterior aspect of the upper border of the apex of the duodenal cap compatible with mild past or present ulceration. A highly selective vagotomy was done, baring the oesophagus and lesser curve from 5 cm above the cardia to 7.5 cm above the pylorus without any particular difficulty in relation to the lesser omentum. The patient made quite good progress at first and was beginning to enjoy food by the fifth day, when he rapidly deteriorated, becoming distended and dehydrated with low blood pressure, some vomiting, and oliguria. As in Mr. Newcombe's case, paralytic ileus without obvious cause was diagnosed and there was considerable improvement from reinstituting nasogastric suction and vigorous intravenous replacement. He did not improve as much as he should have, and on 19 June 700 ml of right pleural effusion containing *Candida* sp. was withdrawn. A subphrenic abscess was confirmed by x-ray examination and explored the next day. His condition by then had become critical. It was realized that ischaemic necrosis of the lesser curvature was likely to be the underlying cause of the deterioration, but his condition was so poor that the anaesthetist forbade further laparotomy. The patient died on 22 June. Necropsy confirmed peritonitis, right subphrenic abscess, pleural effusion, and collapse of both lower lobes and it revealed an ischaemic 3-cm perforation at the lesser curvature, half way between the pylorus and cardia, which initially had been partly walled off by adhesions between peritoneum and omentum but recently had totally perforated and had pus around it. Histological examination confirmed the presence of a zone of ischaemic change in the stomach wall extending 2.3 cm around the perforation.

It is ironical that we should report a fatality from an operation performed only four days after you published Mr. Wyllie's letter reporting his successful management of his case. The timing and nature of the post-operative complication in our case closely resemble those in Mr. Wyllie's and Mr. Newcombe's. Obviously these three similar cases suggest that ischaemic necrosis of the lesser curvature of the stomach may be regarded as a definite though rare complication of highly selective vagotomy. Whether this

is an acceptable risk in any individual case is, of course, for the surgeon performing the operation to decide. We now suture the greater omentum up over the lesser curve at the end of a highly selective vagotomy procedure to minimize the risk.—We are, etc.,

ROBERT HALL
G. A. C. SUMMERS

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York

M. A. GREEN

Department of Forensic Medicine,
Leeds University

Fainting during General Anaesthesia in Supine Dental Patients

SIR,—With reference to the letter from Dr. J. G. Bourne (7 September, p. 627) the following case would be of interest.

A nervous but healthy man aged 46 was placed horizontally for dental treatment under intravenous sedation with pentazocine, atropine, and diazepam. At the outset venepuncture was difficult, and a further venepuncture had to be carried out on the other arm. While this was being given the patient became very pale and lost consciousness. His head was lowered approximately 6 in (15 cm) below the level of his feet and consciousness immediately returned, the patient making the remark that he had not expected to go to sleep so quickly. The induction was then completed and he again lost consciousness. He was very pale, cold, and clammy and apparently there was no peripheral pulse—he appeared to stop breathing. His lungs were inflated with oxygen for about 1½ minutes, when shallow breathing and a weak pulse returned. He was given further atropine, and 100% oxygen was administered for five minutes, after which there was no further cause for anxiety. The operation took a total of 1½ hours, at the end of which the patient was very happy and apparently normal. The total amounts of pentazocine and diazepam given were 30 mg and 18 mg respectively.

It is important to note that the chair used is completely horizontal so that there is no "abdominal well." I consider that the horizontal position is not adequate for very nervous patients and one should be watching the colour of the patient—particularly during the opening minutes of the operation. A chair which raises the feet a few inches above the rest of the body is, in my opinion, essential.—I am, etc.,

J. D. PARSONS

Chesterfield

Potential Danger of Hydatid Disease of Horse/Dog Origin

SIR,—We believe the attention of the medical profession should be drawn to a potential change in position with respect to human hydatid disease in Great Britain.

Human hydatid disease of sheep/dog origin is fortunately not common in Britain and is largely confined to Wales. However, within recent years a dramatic and alarming increase of hydatid disease in horses has become apparent (see fig.). We believe that this rise is due to an increase in the feeding of raw meat and offal, including horse offal, to hunting dogs since the end of the second world war. This feeding practice, brought about mainly by economic pressures, has led to a widespread prevalence of the tapeworm *Echinococcus granulosus* in hunting dogs in this country. With the co-operation of relevant hunting authorities we have examined 121 hunting dogs, mostly foxhounds, from 21 kennels and have found 28.8% to be infected.⁹ These dogs, of which there are at least 20,000 in this country, cover extensive

Incidence of Equine *Echinococcus* in Great Britain. (Nos. 1-8=references listed at foot of letter. Figures in parentheses represent nos. of horses tested.)

areas of countryside and there must therefore be widespread contamination of the countryside with infective eggs of *E. granulosus*.

Though eggs from sheep/dog infections are known to be highly infective to man, there is little evidence available regarding the potential infectivity to man of the horse/dog parasite. Experimental work in vitro¹⁰ suggests that "sheep" and "horse" hydatid may represent different "strains" of *E. granulosus* and it is possible that the horse/dog "strain" may prove not to be infective to man. Nevertheless, evidence from experimental infections of laboratory rodents indicates that the cystic stages of the horse "strain" develop more slowly than the sheep "strain" and, if infective to man, it might be some years before this became evident in the population.

Present figures relating to hydatid disease in man have yet to show any increase in the incidence of the disease, but we feel that the attention of radiologists and surgeons in particular should be drawn to this potential health hazard.—We are, etc.,

R. C. A. THOMPSON

J. D. SMYTH

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Halothane and Liver Damage

SIR,—Your well-balanced leading article (7 September, p. 589) is most welcome.

I am not a great upholder of halothane as a perfect drug, but I am, insofar as I understand them, an upholder of the laws of evidence as applied to scientific research. The Committee on Safety of Medicines continues to note the occurrence of jaundice

after anaesthesia and to point out that halothane was used in the cases of which it happened to receive reports. No doubt it would be equally true, though perhaps less apposite, to observe that thiopentone or another barbiturate was used. Yet it is scarcely an exaggeration to say that on the basis of this kind of relationship, not evidently causal, the committee appears to have drawn conclusions strong enough to motivate its ill-received letter.

Private individuals or independent groups may draw whatever conclusions they wish and build upon them at their peril, but the Committee on Safety of Medicines is a statutory body which must command the respect of all relevant professions in all countries. Is it a proper part of its function to enter the arena of clinical judgement in the use of drugs and, if so, on what basis?

I apologize for the tone of these remarks, but I believe they are not impertinent. The functions and the utterances of the Committee on Safety of Medicines are the concern of us all. Perhaps there is a principle here which should not be obscured by the details of a specialized controversy about one single drug.—I am, etc.,

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Withington Hospital,
Manchester

Withdrawal of Rifamide

SIR,—I have recently read the letter by Professor J. D. Williams on rifamide (6 July, p. 44).

I wish to point out that no in vitro data on the activity of rifamide on bacteroides were reported in our paper¹ mentioned by Professor Williams. Results on the in vitro action of rifamide against *Bacteroides fragilis* were given by Ingham *et al.*,² while Kislak³ and Nastro and Finegold⁴ (references 5 and 6 in Professor Williams's letter) used rifampicin and not rifamide in their experiments.

The same misunderstanding seems to have occurred also elsewhere,⁵ at least as regards the paper by Nastro and Finegold, so that it seems appropriate to clarify the actual situation.—I am, etc.,

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Gruppo Lepetit S.p.A.,
Milan, Italy

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Forebodings for the Future

SIR,—I would like to concur with nearly everything Dr. S. Bradshaw says in his Personal View (14 September, p. 682). I am not well into the middle age, but have the same forebodings for the future and dissatisfaction with the present. It is interesting that many of the recent Personal Views have been along these lines, and it is of course unfortunate that like Ivan Illich, whose philosophy I feel sure these contributors would appreciate, they cannot offer a prac-