In this crossover study it was possible to achieve very satisfactory control of the blood pressure with few side effects during each treatment period in which a vasodilator (either prazosin or hydralazine) was combined with a beta-adrenergic blocking agent and a thiazide. Blood pressure control had not been satisfactory before addition of the vasodilator capsules. All blood pressures were estimated with the London School of Hygiene and Tropical Medicine sphygmomanometer.—We are, etc.,

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PRISCILLA KINCAID-SMITH

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Transfusion Policy

SIR,—I am prompted to write in support of your leading article entitled "Component Transfusion Therapy" (31 August, p. 544) and of the letter from Dr. J. Darnborough and others (p. 573).

Your leading article states that "if clinicians in hospitals would accept a greater part of the blood used as partially packed red cells, then the supply of AHG concentrate could be greatly increased to augment and gradually replace the supply of cryoprecipitate preparations for haemophilia, for the latter preparations vary widely in their AHG content." To the best of my knowledge and belief this policy has been carried out at the Royal Infirmary, Sheffield, for some years with no detriment to non-haemophilic patients.

Personal experience in the Leeds and Sheffield Regional Transfusion Centres impels me to support the views of Dr. Darnborough and his colleagues in Cambridge, Edgware, and Manchester respectively.—I am, etc.,

EDDIE BLACKBURN

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Royal Infirmary,
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Wartime Penicillin

SIR,—While supporting Sir Ian Fraser's contention (14 September, p. 685) that the use of penicillin by the army for treating gonorrhoea was restricted during the period of very short supply, I feel bound to place on record one qualification to this. I was a member of a committee of the three services and the Medical Research Council which debated this problem, exactly when I am not sure, but perhaps rather after the time of which he writes, and there were strong representations that an exception should be made for the sake of the morale as well as fitness for duty of certain key Service personnel. The committee therefore recommended that treatment of gonorrhoea with penicillin should be permitted only in fighter pilots, tank crews, and commandos. Whether this rather lame compromise was then translated into an official order I never heard.

The writer whom Sir Ian Fraser criticizes chose an unconvincing example; most pneumonia was still responding well to sulphonamides. The agonizing choice was whether to afford enough penicillin to treat a case of staphylococcal septicaemia or of

subacute bacterial endocarditis when the amount necessary would cure scores of people suffering from less lethal infections whose rapid restoration to health may have been important to the war effort. A frequently quoted example was sepsis following industrial accidents, which if untreated could cause prolonged disablement. Mercifully the period during which these desperate decisions had to be made soon came to an end as penicillin production gained momentum.—I am, etc.,

Wokingham, Berks

LAWRENCE P. GARROD

SIR,—In reply to Sir Ian Fraser's letter (14 September, p. 685) I confirm from personal experience that penicillin was used to treat gonorrhoea in Germany in 1946-8 while German children died with pneumonia, meningitis, etc. The special centre in which I worked frequently had to turn away distraught parents begging for this drug, often bringing their dying child as proof of their desperate need. Their only hope of obtaining this drug was via the Swedish and Danish Red Cross and the delay invariably meant death to the child.

Sex in this context did have priority over the sick child, but of course the child was unfortunately on the wrong side of the war. Perhaps this point was not made clear in the original quotation.—I am, etc.,

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The Disc Sensitivity Test

SIR,—Your leading article on this subject (13 July, p. 74) reviewed the current situation in regard to testing of bacterial sensitivity to antibiotics, in particular the regional and international attempts that have been made to standardize test techniques. As one of the editors of the report from the International Collaborative Study (I.C.S.) on antibiotic sensitivity testing,¹ I wish to point out some misunderstandings expressed in your article concerning the I.C.S. recommendations.

The I.C.S. report studiously avoided recommendation of *standard* methods for universal use. *Reference* methods were proposed against which locally accepted techniques could be evaluated. Methods closely resembling one of those described in the I.C.S. report—a quantitative variant of the paper disc diffusion test—have, however, been widely and successfully used in Scandinavia for more than 20 years.

According to your article essential features of both the I.C.S. method and the method evolved by Kirby *et al.*² and recommended by the U.S. Food and Drug Administration are "the use of large plates . . . and the translation of inhibition-zone diameters directly into categories of sensitivity by reference to tables." The I.C.S. report¹ recommended that plates be "poured to a depth of approximately 4 mm. This corresponds to 60 ml for plates of 14 cm internal diameter . . . and 25 ml for plates of 9 cm internal diameter" (p. 69). No preference was stated for plates of either size. In my laboratory we use 14-cm plates, but many other Swedish laboratories which strictly

observe the I.C.S. recommendations use 9-cm plates. The I.C.S. method does *not* directly translate inhibition-zone diameters into categories of sensitivity. Zone diameter is first translated into the minimum inhibitory concentration (M.I.C.) of antibiotic, using regression lines. These lines are evolved by the Swedish maker of test discs in collaboration with research laboratories. The categorization of microbial sensitivity is thereafter based on the relationship of the M.I.C. to antibiotic levels in blood and other tissue fluids and on clinical experience (p. 76).

Your article further states that "the main objection to the F.D.A. or I.C.S. method is . . . simply that it is uncontrolled." The I.C.S. report,¹ however, included detailed comparisons of regression-line studies in different countries. Statistical analyses of these comparisons were published in the report (p. 50). Moreover, checks against agreed bacterial strains now form part of the daily routine work with the I.C.S. method in Scandinavia. Whenever systematic deviations from expected results appear the laboratory is urged to search for the responsible factors.

The I.C.S. may, admittedly, have given inadequate consideration to variations in commercially available media, since one single batch was used and not all potential variables were investigated for influence on test results. The effect of gentamicin on magnesium was not known at the time of the I.C.S. and indeed this antibiotic was not studied. Now, however, an improved medium is available. The microbiological performance of each batch is checked in relation to regression lines and is guaranteed when the medium is marketed.

Standardization of techniques and materials for the disc sensitivity test has been achieved in Scandinavia by informal collaboration between colleagues and without the intervention of public bodies. Our policy in regard to single-disc testing is undogmatic. At the time when the F.D.A. approved use of separate discs for each proprietary brand of antibiotic Swedish bacteriologists maintained that only one disc was required to test sensitivity to a whole group of microbiologically and chemically closely related substances. But the need for special discs in certain circumstances has always been recognized and catered for, as has the need for discs of different potencies in tests against antibiotics with a wide dosage range, such as benzylpenicillin.

In conclusion, I should like to comment upon the Stokes method that appears to be popular in Britain. This method does not eliminate one of the most important sources of error—variations in inoculum of standard and test strains. Moreover, the importance of the concentration of antibiotic in the urine, which is recognized also in your article, necessitates the use of different standard strains in different clinical situations. For example, urine may be sent to the laboratory at a time when the clinical picture indicates simple cystitis, but when the result of the sensitivity test is delivered the infection may have progressed to deep-seated pyelonephritis. The clinician may then be misled in his assessment of the antibiotic dosage required to meet the clinical situation.

None of the I.C.S. members, and certainly not the two editors of the report, believed

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

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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