revealed antibody and therefore a state of immunity. The seronegative cases have remained seronegative after Ig prophylaxis and have delivered normal children with no increased levels of IgM in the cord sample. On the other hand if seroconversion had occurred following the administration of Ig the patient and her medical practitioner could be advised accordingly, but this has not occurred in our experience. It would probably be preferable, as suggested by Drs. Forrest and Menser, to have an Ig preparation with a high antibody titre, but as this is not generally available and because there is some variation in the antibody titre of the normal pooled Ig product, we have used a large dose of 3 g administered in two separate doses. Though this is considerably greater than what is advocated we think it is justifiable in the circumstances.—I am, etc.,

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London W.C.1

1 Forrest, J. M., Honeyman, M. C., and Murphy, A. M., *Medical Journal of Australia*, 1973, 1, 745.

2 Public Health Laboratory Service Working Party on Rubella, *British Medical Journal*, 1970, 2, 497.

Puerperal Rubella Vaccination and Anti-D Immunoglobulin

SIR,—Drs. Jill M. S. Forrest and Margaret A. Menser report (25 May, p. 439) two patients in whom the use of immunoglobulin (Ig) prophylaxis caused delay in the serological response following exposure to rubella during pregnancy. Both infants were born with congenital rubella defects.

It may not be generally realized that the serological response obtained from vaccination against rubella in the puerperium may be altered by the concurrent administration of anti-D Ig to rhesus-negative women. We routinely take blood at the first antenatal visit for serological assessment of the patient's rubella immune status. In patients found to be susceptible vaccination against this disease is advised before discharge from hospital following delivery. An efficient form of birth control is recommended for three months following vaccination.

The manufacturers of Almevax and Cendevax recommend that vaccination with their products should not be carried out within six weeks following the administration of human immune serum globulin. The reason for this contraindication is that the Ig could contain anti-rubella antibodies, which if present would reduce or negate any effect of giving the vaccine. Normal Ig usually contains between 160 and 320 units/ml of rubella antibody, and anti-D Ig probably also contains rubella antibody within this range. The manufacturers of anti-D Ig state that it would be extremely difficult, if not impossible, to make a preparation free of rubella antibody. It would therefore be pointless to vaccinate against rubella and to administer anti-D Ig simultaneously.

It is essential to administer anti-D Ig to at-risk patients as soon after delivery as possible. On the rare occasions that these patients are also susceptible to rubella we recommend that the patient be informed of her susceptibility but not vaccinated against this in the puerperium. The patient's general practitioner is also informed of her suscep-

tibility in order that he may arrange vaccination for her at a later date. This should not be within six weeks of the administration of anti-D Ig.—We are, etc.,

BRIAN ALDERMAN
D. W. CHARTERS

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Kidneys from Living Donors

SIR,—In your leading article (18 May, p. 344), which refers to the use of live donors for kidney transplantation, emphasis is placed on the predominant use of cadaveric donors in Australia and in the United Kingdom and other European countries. While there is no doubt about the accuracy of this observation, it is not strictly true to say that in the United Kingdom kidneys from living donors are used only occasionally except in Newcastle. Kidneys from 48 live related donors have been used at Hammersmith, and at a conference in January 1973, which was attended by representatives of most major British and European dialysis centres, I quoted an incidence of 24% for live donor operations in our current practice. Many feel that cadaver kidneys are preferable, but unless there is a good supply and the organs are physiologically acceptable—and this implies national adoption of the concept of cerebral death—the results are not likely to be nearly as good as those achieved when living related donors are used.—I am, etc.,

RALPH SHACKMAN

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Ergotamine-induced Headaches

SIR,—We would applaud Dr. N. J. Legg (11 May, p. 331) for drawing attention to the incidence of intractable headaches in patients who are taking substantial amounts of ergotamine tartrate. Since the initial publication of the experience of this condition in the Princess Margaret Migraine Clinic (formerly the City Migraine Clinic)¹ we have kept a record of all cases. In a sample population of 1,000 patients referred for consultation between June 1973 and April 1974 we found that in 43 the presenting symptoms were considered to be due to excessive use of ergotamine.

Analysis of these cases shows that the condition is seen more often in women than men (F:M = 31:12) and more often in association with common migraine than classical. In 34 patients the initial diagnosis was common migraine, in four classical migraine, and the remaining five patients were considered to suffer from headaches not due to migraine. The duration of abuse was protracted. Of the 43 patients 23 had taken ergotamine regularly each week for more than a year, and of these 23 7 had taken it for more than five years. We have included in our 43 patients only those who have taken more than 10 mg of ergotamine tartrate each week. Twenty took 10–20 mg, 14 took 20–30 mg, and nine took 30–70 mg weekly. The patients had daily headaches relieved only by further ergotamine and accompanied by nausea and general malaise, and we would emphasize that there is always some difference between these headaches and the migrainous headaches for which the patient initially sought treatment. In addition, our experience does not support the contention that the symptoms disappear immediately nor that the headaches are worse while the patients are actually taking the ergotamine. Only five patients stated that they suffered no headache on discontinuing the ergotamine and, indeed, two patients refused to

do so on account of the severity of the headache they experienced. Twenty-three patients complained of the worst headache that they had ever had when they stopped taking the ergotamine, the symptoms lasting for from one day to two weeks. However, once this period was over the frequency and severity of the headache was considerably reduced.

We would welcome any suggestions which might lead to effective research into this syndrome, especially as we have no shortage of suitable patients. The term ergotamine-induced headaches seems suitable for this condition, as the headaches improve when the ergotamine tartrate is stopped once the initial withdrawal period is over. This seems especially important as one manufacturer continues to recommend doses of ergotamine² up to 24 mg/week, which, if taken regularly, would appear to be a level at which few if any patients could avoid this syndrome.—We are, etc.,

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GLYN VOLANS
MARCIA WILKINSON

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1 Rowsell, A. R., Neylan, C., and Wilkinson, M., *Headache*, 1973, 13, 65.

2 Association of British Pharmaceutical Industries, *Data Sheet Compendium*, p. 758. London, A.B.P.I., 1974.

Antibiotics and Farmers

SIR,—It seems a little hard to blame the farming community for the ineffectiveness of antibiotics in diarrhoea due to *Escherichia coli*, shigella, salmonellae, and other Gram-negative bacilli (leading article, 4 May, p. 235). It has been clear to many clinicians and clinical bacteriologists for some time that antibacterial agents do not have a beneficial effect on the course of the great majority of such infections, and this is unrelated to antibiotic resistance of the infecting strains.

It is probably true, as you suggest, that selection of antibiotic-resistant strains is favoured by the use of antibiotics. In the individual, whether animal or human, this selection is more likely to occur if lower dosages are used because of failure to eliminate the whole of the bacterial population. I think, however, it is wrong to suggest that the lower doses themselves are the cause of resistance. My own impression is that antibiotic-resistant strains become prevalent only where conditions are suitable for spread from individual to individual, and this situation is seen in relatively closed communities such as hospital wards. More attention paid to the general epidemiological principles which prevent contagion would be more likely to be effective in reducing the spread of resistant organisms in such environments than severe restriction of the use of antibiotics.

In the general population there is little evidence of the failure of treatment with antibiotics in proved bacterial infections. It is also probable that there are natural means for the elimination of R-factors and plasmids in the general community which balance their formation. After all, it is likely that these resistance factors and other mechanisms leading to genetic change which we measure by antibiotic resistance have been around for a long time and will continue to be so.

The problems created by antibiotic resistance of bacteria are small when compared with the good that has been done by

their use in the treatment and control of infection—and for some in enhancing animal growth—and while not advocating the indiscriminate use of antibiotics, I think a proper sense of proportion should be maintained when considering this problem in human medicine and any relationship there may be to animal biology.—I am, etc.,

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Healing of Duodenal Ulcer

SIR,—Despite the abundance of literature on duodenal ulcer very little information is available on the topographical progress of the ulcer after medical and surgical treatment. With the introduction of fiberoptic endoscopes the progress of the ulcer can be easily followed and recorded, particularly after highly selective vagotomy because of the undisturbed anatomy.

Since August 1973 in a consecutive series of 15 patients with duodenal ulcer I have recorded the endoscopic appearances before highly selective vagotomy and at monthly intervals thereafter. At one month after surgery there was no significant change in the ulcer, though previously noticed periulcerous congestion and oedema had disappeared. At two months three of the ulcers had completely healed and there was evidence of epithelization in the others. At three months only two of the ulcers remained unhealed: one of these had epithelized by the next examination but the other persisted at five months. All patients remained symptomless during this period.

From this observation it transpires that, though symptomatic relief may be obtained immediately after surgery, it may take many months before the ulcer actually heals. Similar follow-up is being carried out during medical management because from my experience it would seem that recommended courses of six weeks' treatment are too short for complete healing of duodenal ulcers.—I am, etc.,

M. SHAFIQ

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Attitudes to Abortion

SIR,—Miss Madeleine Simms, having bracketed by implication Professors H. C. McLaren and J. S. Scott with the Nazis (1 June, p. 501), proceeds to make a series of statements which have little relation to the truth.

Let us consider the facts: Among the first Acts the Nazis passed was one permitting mass sterilization of the congenitally abnormal. This was subsequently expanded by secret decrees which permitted the abortion, sterilization, and liquidation of the Jews and the Reich's other "enemies." It is for this reason that abortion methods were perfected in concentration camps. It is true that the Nazis were opposed to abortion for "wanted Aryans" (they, like Miss Simms, were interested in eugenics), but any society, including our own, is judged by how it treats its "unwanted" people. Careful thought

might determine whose beliefs are closest to the Nazis' we all affect to despise.—I am, etc.,

MARGARET S. WHITE

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Laparoscopic Sterilization

SIR,—The letters of Dr. K. R. Greene and others (6 April, p. 54) and Mr. F. E. Loeffler (25 May, p. 444) stimulate me to write again on the subject of sterilization by laparoscopic techniques. The performance of the Pomeroy sterilization under laparoscopic control was described by me in 1972 but I had reservations about the method. I regard it as a retrograde rather than a forward step as it carries with it the disadvantages of the Pomeroy ligation with its failure rate. From the point of view of long-term success I also described what I think is a much better procedure—namely, that of laparoscopic fimbriectomy. The extremity of the ampulla and the fimbria is much more mobile than the midportion of the oviduct and it can be lifted out of the abdomen in suitably selected thin patients without undue risk of causing haemorrhage from a mesenteric tear, to which Mr. Loeffler rightly calls attention in his procedure. Two separate incisions about 1 to 1½ cm in length are required each side of the midline in the pubic hair line, avoiding the epigastric vessels by laparoscopic transillumination using the angled telescope.

Fimbriectomy is much more likely to lead to permanent successful sterilization than the Pomeroy technique. Incidentally, accidental damage to a branch of the inferior epigastric vessels does not need laparotomy as a rule. Haemostasis can be secured by passing a large curved needle through to the peritoneum under laparoscopic vision. A figure of eight made in this manner will be seen to control the haemorrhage successfully without the need for opening up the incision. As regards the problem of reversibility, I hope to report soon on a large series of patients fitted with intratubal devices which are held in position by a single tantalum clip.¹ The principle of the procedure is *not* to achieve occlusion of the oviduct, so that function can return without any major operative procedure. In 1973 Frangenheim² and Semm³ each reported two methods of performing laparoscopic tubal ligation which do not require more than one ancillary incision for introducing the necessary special ligation instruments. Frangenheim places a suture around a loop of each tubal isthmus and Semm passes his ligature right round the tube, which is grasped with a ligating forceps. In the future, diathermy coagulation of the tubes will be safely achieved by the use of a disposable forceps which utilizes a direct current of 30 volts rather than high frequency diathermy. It may be that we shall use this method for permanent sterilization and intratubal devices for reversible sterilization. Obviously, careful selection of patients will be needed.

Incidentally, not only can Pomeroy ligations and fimbriectomies be performed under laparoscopic control but also appendicectomies. The appendix is lifted out of the abdomen through a small incision by means of a grasping forceps, and then removed in the usual way. Security of the ligatures can

be readily assessed by dropping the caecum back into the abdomen leaving the ligatures long until inspection by laparoscopy is complete. There are cases suitable for this postage-stamp type of surgery, which one did not undertake formerly.—I am, etc.,

PATRICK STEPTOE

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- 1 Steptoe, P. C., *Proceedings. American Society of Gynecological Laparoscopists*, 1973. In press.
- 2 Frangenheim, H., *Proceedings. American Society of Gynecological Laparoscopists*, 1973. In press.
- 3 Semm, K., *Proceedings. American Society of Gynecological Laparoscopists*, 1973. In press.

Abdominal Decompression in Pregnancy

SIR,—While agreeing with many of the points made by Professor P. Curzen (1 June, p. 499) about your leading article (4 May, p. 238) the possibility you hinted at that because the trial of Varma and Curzen¹ was not a blind one the observed difference in the results obtained in the two groups of patients may have been due to observer bias still seems to me a plausible interpretation of their findings. There is, however, another much less nihilistic interpretation which I thought was dismissed much too summarily in the original paper.

I refer to the possibility that the decompressed patients may have had better results with regard to their babies because they received less antenatal bed rest. Though the mean duration of bed rest was only 1.5 days less in the decompressed patients, this difference was found to be statistically significant and its possible effect upon fetal well-being may have been enhanced by a much larger difference in the strictness with which the antenatal bed rest was enforced in the two groups of patients. As I know from my own work on decompression,² getting into the suit, working the levers, and even breathing in it are no mean feats. This of itself will have been enough to ensure that the decompressed patients got more daily exercise than the control patients who were left in bed. It is also possible that the treated patients were walked to and from the suit and that the ward sisters, seeing them receiving what amounted to exercise on doctor's orders, will have allowed them considerably more freedom about the wards.

In their original paper Dr. Varma and Professor Curzen quote the review of Green *et al.*³ as providing support for their supposition that the greater amount of rest received by the control group of patients will, if anything, have improved the well-being of their fetuses and so have made the beneficial effects of decompression *per se* even more striking. The works reviewed by Greene *et al.*, however, show only that uterine blood flow, not even fetal well-being, can be increased by rest, or rest after exercise, over periods of time which are infinitesimal compared with the weeks and even months of bed rest that some of Dr. Varma and Professor Curzen's patients received. Assuming that the apparent beneficial effect of bed rest will hold for such long periods of time is like assuming that the fastest man over the first five of the 10,000 metres is necessarily going to be the leader 25 laps later.

I would respectfully submit that there is evidence⁴ suggesting that prolonged ante-