to tell his patients, quite frankly, to smoke less, to drink less, to stop eating too much rubbish, to exercise more, and to behave generally on less pathogenic lines. He could then, perhaps, spare a little time to do battle against what may be called the social or corporate factors of a kind inimical to health.

I read Professor Miles's lecture¹ with much more interest and respect than I did that of Sir Henry Dale; but it is not correct to say that "he neatly turned the tables on the critics of animal experiments." It is obvious that many species have organs and functions related to those in man; but there are wide differences, and one can rarely argue with any confidence from one species to another, or even in some cases from one member of a species to another of the same kind. The kinship is based, not on a similarity of organ or function, but on the two facts that all have conscious life and all can suffer. The more religious would say that all come from God, and that therefore an offence against one of them, whether human or other, is a sin.—I am, etc.,

WILFRID TYLDESLEY,

Secretary,

British Union for the Abolition of Vivisection.

London, S.W.1.

REFERENCE

¹ Miles, A. A., *Conquest*, 1957, 45, 2.

Benactyzine

SIR,—We regret not having included in our paper (*Journal*, February 9, p. 306) the data necessary for the statistical calculation Dr. F. K. Taylor makes in his letter (*Journal*, April 27, p. 1006). The contrast between drug and control was increased by the rejection of four, and decreased by the rejection of two, of the six cases, which, to use Dr. Taylor's model, is equivalent to throwing four heads and two tails in six spins of a coin. *P* is therefore not 0.016 but 0.34, which is of much lower significance than any of the *P* values reported in the paper.—I am, etc.,

Leeds, 2.

G. R. HARGREAVES.

Prevention of Tetanus

SIR,—The present correspondence on tetanus and its prevention following the inquest at Hanley on two men who died from post-operative tetanus (*Journal*, April 13, p. 897) revives interest in the unsatisfactory measures employed for the sterilization of rubber gloves, which was discussed in a leading article two and a half years ago (*Journal*, January 15, 1955, p. 156).

The Report of the Central Pathology Committee of the Ministry of Health on the Sterilization of Hospital Equipment, issued in December, 1954, recommended that rubber gloves should first be boiled and then autoclaved at 5 lb. pressure for 30–40 minutes. Your leading article was critical of the details of the process, but you did not condemn the process itself, although you implied disapproval when you spoke of the intolerable position that "gloves, of all things used in an operation, should be of doubtful sterility." Trillwood¹ has recently discussed what happens when steam is admitted to a drum containing rubber gloves and has pointed out the crux of the matter—that rubber is impervious to steam. He concluded that "if surgeons' gloves are required to be sterile a method other than autoclaving is indicated."

Surely the time has come to consider whether the solution to this problem may not be a re-examination of surgical techniques. If rubber gloves cannot be sterilized by autoclaving, and if sterilization by chemical means is uncertain (there are additional dangers if rubber is in contact with phenolic solutions), then may not the only alternative be a change in surgical procedure? One could make several suggestions: (1) a no-touch technique, almost impossible in most types of surgery; (2) the use of dry "clean" gloves, washed with a detergent and finally rinsed in a long-acting antiseptic, or smeared with chlorhexidine cream (where this is not contraindicated, as it is in brain surgery); (3) if tetanus is the biggest risk, a careful search for its source in hospital and the immediate banning of such common offenders as talc and tow; (4) the prophylactic protection by tetanus toxoid of

all patients for operation, and not just those with accidental injuries as suggested by Dr. J. W. Myles (*Journal*, May 4, p. 1064).

There are of course many other facets to this problem, but these suggestions may provide a basis for a useful discussion.—I am, etc.,

Oxford.

JAMES CALNAN.

REFERENCE

¹ Trillwood, W., *Pharm. J.*, 1957, 178, 298.

Dimenhydrinate in Chicken-pox

SIR,—Dr. G. I. Watson, in his article on infectious diseases in general practice (*Journal*, January 5, p. 39), mentions the use of mepyramine maleate in chicken-pox. In January, 1956, while suffering from chicken-pox, the irritation drove me to trying antihistamines because of the reasonable theoretical proposition that the great irritation is due to release of histamine. The most effective proved to be dimenhydrinate 50 mg. twice a day with pentobarbitone 1½ gr. (97 mg.) in the morning. Mepyramine maleate 100 mg. alone made me sleepy and depressed. In August, 1956, I used dimenhydrinate 50 mg. with pentobarbitone ½ gr. (50 mg.) on 57 children between 2½ and 12 years old. There were ten children additionally who were siblings and also had chicken-pox; these were used as controls. The results were striking, because the treated children became quite happy and tractable. Further, because there was no cause for scratching, there was only one case of secondary infection of the pocks, yet other doctors in the same district were much troubled by secondary infection. Later, in another area of Sydney, it seemed that dimenhydrinate 50 mg. was sufficient for children up to 5. However, in the older children pentobarbitone did relieve them from worrying about their pocks. Since chicken-pox is a dangerous, a depressing, and a disfiguring disease, it should be worth while having several clinical trials in general practice.—I am, etc.,

Tasmania.

MICHAEL V. PERKINS.

Local Analgesia and Kielland's Forceps

SIR,—Drs. J. S. Scott and R. L. Gadd have made a very useful contribution to the art and technique of forceps delivery (*Journal*, April 27, p. 971). I read their paper on the use of local analgesia and Kielland's forceps with great interest, but was surprised to find myself quoted as giving 70 lb. (31.8 kg.) as the critical level of traction force.¹ When I looked over my old paper I was even more surprised to find I had been quoted correctly, for I had forgotten my first efforts to find some guide to the safety of traction force.

The figure of 70 lb. (31.8 kg.) was based on theoretical considerations and some experimental work which was not very convincing. Over the years I have acquired a considerable experience of measured traction force, but I still do not know the level of critical traction. It probably varies from foetus to foetus, and a fixed figure is extremely unlikely. I have found that a force of 100 lb. (45.4 kg.) may be used with safety provided the foetus is at term, the blades of the forceps applied along the cheeks or mento-vertical diameter of the fully flexed head, and traction force applied slowly through a spring handle. I have little experience and no data relating to forceps delivery of premature infants. The excessively moulded head of the term foetus may not withstand the same compression force as the unmoulded head, and I have always assumed that the compression force is proportionate to the traction force and is supplied by the resistance of the maternal tissues. To commit oneself to the forceps operation when the head is at the mid-cavity station or higher, without due consideration of all the factors involved, is to court disaster, and, it seems to me, no rule of thumb can ever replace obstetrical judgment and the technical skill which come with experience.

Anyone who has used a recording instrument for the obstetrical forceps operation must have been rather shocked at the ease with which the magnitude of the traction force

can be raised, and alarmed at the effects such force might have on the foetal brain. Seated on a sterilizing drum 10 in. (25 cm.) high, and pulling with one hand, 100 lb. (45.4 kg.) is the maximum force which I can exert with the Milne-Murray axis-traction instrument. A powerfully built man may exert a much greater force under the same conditions. Infants delivered with a force of this magnitude have been examined at intervals up to 10 years of age, and nothing has been found to suggest that such force has been harmful.

The great majority of these difficult forceps cases were due to contraction at the pelvic outlet. The rickety flat pelvis which was common in this locality 20 years ago has gradually been replaced by a type of contraction more difficult to detect because more often it involves the outlet. This preponderance of outlet contraction has been confirmed by x-ray pelvimetry. About 30% of the women admitted to this hospital are under 5 ft. (1.5 m.) in height, and many of these have some degree of outlet contraction. In the past we usually allowed trial of labour in this group and had little trouble when the foetus was no more than 6½ lb. (3 kg.). Not infrequently, however, when the foetus was larger, we were faced with the unhappy choice of delivery by caesarean section after the head had become deeply engaged, or by a difficult forceps operation. In recent years we have been inducing labour more often in this group, and the difficult forceps delivery has become something of a rarity.

When dealing with persistent occipito-posterior and deep transverse arrest positions, I have always preferred manual rotation followed by application of the Milne-Murray instrument. My objections to Kielland's instrument include the difficulty of accurate application of the blades along the cheeks and the greater traction force apparently required to effect delivery. This latter is merely an impression, but it has seemed to me that infants delivered with Kielland's forceps show more bruising due to the impress of the blades. I have a device which allows attachment of a spring recording handle to the Kielland's instrument, but have collected no useful data. Scott and Gadd are on rather thin ice when they argue traction force on the basis of one case of brow presentation. From the position of the head in their illustration delivery would be effected by the movement of flexion alone, and if more than slight traction force had been required after complete flexion was reached the blades would probably have slipped. Moreover, the head must have been small to rotate from the brow to the vertex position when so low in the pelvis. This movement of the head between the blades was probably due to the fact that the blades were not designed for application to the short arm of the head lever in the biparietal diameter, and the authors were fortunate to deliver so easily, for this application is dangerous. Some years ago I produced an indented fracture with a somewhat similar application and I have been careful to avoid it ever since. Movement of the head between the blades properly applied to a vertex presentation occurs for very different reasons and is disturbing because it presages slipping of the blades. I attempted to demonstrate these reasons some time ago.¹ However, there can be little doubt that in the hands of competent operators different types of obstetrical forceps produce equally good results.

During the war years we began to use local analgesia for the forceps operation in the Rankin Memorial Hospital. We have gradually increased this practice, and in recent years we have been performing over 90% of these operations using no other analgesia or anaesthetic. This has required a variation in technique which has developed along much the same lines as those described by Scott and Gadd, although we deliver face to pubis when this can be easily accomplished—a technique which at one time we regarded with some suspicion and generally avoided. In all cases we perform a generous episiotomy before applying the blades.—I am, etc.,

Greenock.

JAMES BAXTER.

REFERENCES

- ¹ Baxter, J., *J. Obstet. Gynaec. Brit. Emp.*, 1946, 53, 42.
- ² ——— *ibid.*, 1947, 54, 35.

SIR,—Our experience at Perivale Maternity Hospital fully confirms that of Drs. J. S. Scott and R. L. Gadd (*Journal*, April 27, p. 971) regarding the safety and efficiency of local analgesia combined with the use of Kielland's forceps. Two hundred and twenty-three of the last 231 forceps extractions were done using local analgesia alone. Of these, 184 were simple outlet extractions, 23 were forceps rotations and extractions for deep transverse arrest, and 16 a similar

manoeuvre for persistent occipito-posterior position. Ninety-seven per cent. of all forceps extractions were done using local analgesia.

Of the eight cases requiring general anaesthesia, four were extremely agitated mothers who would probably have responded well to the supplementary sedation suggested. Otherwise trichlorethylene self-administered with the "emotril" or "cyprane" inhaler has been adequate here. Three were delivered by temporary residents not practised in the technique. One was a compound presentation requiring much intravaginal manipulation. A straight sacrum is almost the only common pelvic abnormality in this area. This may explain our higher percentage of delivery under local analgesia alone.

We found that the technique of injection as described by Gate and Dutton¹ was effective but complicated by formation of paravaginal haematomata in a fair number of cases. Latterly we have had excellent results by the simple injection transvaginally around the pudendal nerve of 10 ml. 1% lignocaine on each side, using a technique similar to that described by Wilds.² Only two injections are required in all.—We are, etc.,

M. D. MANION.

F. G. FEINSTEIN.

Greenford, Middlesex

REFERENCES

- ¹ Gate, J. M., and Dutton, W. A. W., *British Medical Journal*, 1955, 2, 99.
- ² Wilds, P. L., *Obstet. Gynaec.*, 1956, 8, 385.

A Verbal Shock

SIR,—The English in the *British Medical Journal* is usually exemplary, witness your insistence on the correct spelling of "disk" and "connexion," and therefore any lapse comes as a shock. In your annotation on the M.R.C. report on emmetropia (*Journal*, April 27, p. 1000), the last word (if it may be called such) is "emmetropization." I hope that this is indeed the last appearance of this word. Such neologizations should be blue-pencilled.—I am, etc.,

Liverpool, 1.

JOHN MATTHEWS.

Congenital Ring Constrictions

SIR,—I am at present working on a hypothesis of the cause of congenital ring constrictions of the limbs and body. I should be most grateful if anyone who is in attendance at the birth of a child with this condition would send the placenta and membranes in a jar containing 10% formal saline to the morbid anatomy department, the Hospital for Sick Children, Great Ormond Street, London, W.C.1. Any expenses incurred will be paid with thanks.

I should also like to hear from anyone who has seen one of these congenital constrictions round the trunk, or any part of the body excluding the limbs.—I am, etc.,

London, W.1.

DENIS BROWNE.

Post-maturity

SIR,—The article by Professor F. J. Browne on foetal post-maturity and prolongation of pregnancy (*Journal*, April 13, p. 851) draws attention once again to the divergence of opinion in relation to post-maturity. There is one aspect of the subject which does not appear to be covered by his article, so you will perhaps excuse me if I draw attention to it.

In my experience post-maturity is only of serious obstetric significance if it occurs in association with what De Lee aptly termed the "dystocia dystrophica syndrome."¹ In this syndrome we find stocky, obese women, who are often about 5 ft. (1.5 m.) in height, with short thighs, broad shoulders, and a short thick neck. It is said that these women exhibit certain well-marked and unfavourable clinical features. First, the cervix is small and the vagina narrow and rigid. Secondly, there is a far greater tendency to pre-eclampsia. Thirdly, pregnancy commonly continues for a week or more beyond full term. Fourthly, primary uterine inertia, slow dilatation of the cervix, and early rupture of the membranes