half truths, and on top of this, it does not give the sources of the known data. Allow me to quote some examples.

"A review of the literature shows no evidence . . ."—of course not, how could it. Our (statistical) science never proves anything. It indicates "significance." "Experienced otologists do not agree on the time . . ." is only true if one stopped reading the relevant literature five years ago. Today they do agree; surgery should be undertaken as soon as progressive denervation is found. "Once degeneration of the nerve has begun, decompression cannot help." Many authors suggesting the positive influence of decompression in well selected cases of Bell's palsy have stated that degeneration is not an end state, but a dynamic process. It is certainly possible to save a number of living fibres when the first symptoms of denervation of some other fibres become evident.

"Secondly, the lesion may be proximal to the site of the operation . . ." This remark is based on the wrong idea that the compression takes place inside the bony Fallopian canal. In reality it has been shown many years ago¹ that only in the vertical part of the facial nerve the sheath is very thick and without elastic fibres; the rest of the sheath is thin and elastic. In the entire course of the bony canal the space inside the bone is ample and compression is very unlikely. In some 300 cases I have never seen compression more central than the site of the stapes. "J. Groves . . . found five cases to do worse" is an ambiguous argument. In a leading article such an argument without any statistical analysis is unacceptable. If the author of the article had read the recent international literature, he would have found figures indicating that statistics in some better evaluated groups point to significant positive influence of surgery in well selected cases of Bell's paralysis.

If McGovern and co-workers² and Jain and Sharma³ are quoted as defenders of a hypothesis on the aetiology of Bell's palsy, why not quote their experimental work which suggests (on statistical grounds) that decompression has a very favourable effect on various traumas inflicted upon facial nerves included artificial oedema of the facial nerve? "Taverner and colleagues with special experience—(why this distinction of one group of workers?)—have shown how to prevent . . ." Why doesn't the author, so critical when he discusses the surgery of Bell's palsy, warn the reader that the groups which these authors compare are not completely comparable?

Finally, it is well known and accepted even by those who do not favour surgery that surgical decompression takes away the often severe spontaneous pain. I have never heard any complaints about the pain inflicted by the operation. Usually patients disliked electrical treatment more than the operation.—I am, etc.,

L. B. W. JONGKEES.

Academisch Ziekenhuis,
Universiteit van Amsterdam,
Holland.

REFERENCES

- 1 James, J., *Zeitschrift für Laryngologie, Rhinologie, Otolgie und ihre Grenzgebiete*, 1961, **40**, 336.
- 2 McGovern, F. H., and Fitz-Hugh, G. S., *Laryngoscope*, 1956, **66**, 187.
- 3 Jain, S. N., and Sharma, A. P., *Journal of Laryngology and Otolgy*, 1964, **76**, 266.

Rational Oxygen Therapy

SIR,—Modern equipment used to administer oxygen to patients breathing spontaneously may be subdivided into devices which administer precise concentrations and those in which there is no control over concentration.¹ Examples of the former devices are the Ventimask series giving 24%, 28%, and 35% oxygen mixtures without rebreathing² which is achieved by high air flow with oxygen enrichment (HAFOE). All other devices belong to the second category and are used for varied indications in which control over oxygen concentration is not currently considered necessary.

The history of oxygen therapy is studded with misconceptions as to dosage. One patient may receive a very different concentration from another patient with the same device at the same oxygen flow rate. Moreover there can be considerable variation in oxygen concentration from breath to breath even in an individual patient. It is a precept of medicine that the dosage of a drug should be known. The pioneers of oxygen therapy, Barach and Eckman,³ stated that the dosage of oxygen should be known and controllable.

This is still not fully accepted even though it has been reiterated many times, the most recent and authoritative being in a report to the Scottish Home and Health Department.⁴

Additionally it does not appear to be widely appreciated that the dangers of giving too much oxygen may be as great as not giving enough (3 October, p. 5). There are thus many circumstances when we do not know whether we are giving too little or too much oxygen, although patients are not necessarily mistreated.

It is undeniable that the entire state of affairs would be improved if oxygen dosage was always known. Under such circumstances repeated assessment of patient response to a given oxygen concentration, either clinically or by direct blood measurements, would be of both diagnostic and prognostic significance. Greater understanding of the pathophysiology of oxygenation and the increasing employment of blood oxygen measurements makes a more rational approach to all oxygen therapy highly desirable. The recent introduction of a 40% Ventimask is therefore opportune. Masks delivering 50% and 60% oxygen could also usefully be employed under certain circumstances. A prototype 60% mask has been produced. This value was selected as the probable upper limit of clinically useful oxygen concentrations for spontaneously ventilating patients. It is the concentration above which patients with normal lungs cannot be exposed without running the risk of pulmonary oxygen toxicity. A cyanosed patient who cannot maintain a reasonable arterial P_{O_2} while breathing such a concentration should be ventilated artificially. With the range of masks indicated above, patients can be subdivided into groups and treated accordingly.

Group 1.—Patients in chronic ventilatory failure with hypoxaemia and CO_2 retention. High oxygen concentrations should be avoided. Masks giving 24%, 28%, and 35% and possibly 40% can be used.

Group 2.—Patients with mild hypoxaemia often arising from non-respiratory conditions

and with normal lungs in whom oxygen therapy is beneficial, but in whom prolonged administration of high oxygen concentrations will produce a risk of pulmonary oxygen toxicity. 28-40% masks (limit 60%).

Group 3.—Patients with severe hypoxaemic conditions without CO_2 retention who may require maximal oxygen concentrations to produce small increases in arterial oxygenation. 40%, 50%, and 60% masks.

Group 4.—Patients with such severe hypoxaemic conditions that intermittent positive pressure ventilation is necessary.

Provision of controlled oxygen concentrations, and particularly of 50% and 60% by the high air flow with oxygen enrichment (HAFOE) system would be more costly than present methods. However, the informed use of such devices, perhaps only in specialized intensive care units, would improve clinical care.—I am, etc.,

JULIAN M. LEIGH.

Magill Department of Anaesthetics,
Westminster Hospital,
London, S.W.1.

REFERENCES

- 1 Leigh, J. M., *Anaesthesia*, 1970, **25**, 210.
- 2 Bethune, D. W., and Collis, J. M., *Anaesthesia*, 1967, **22**, 43.
- 3 Barach, A. L., and Eckman, M., *Anesthesiology*, 1941, **2**, 421.
- 4 Scottish Health Services Council, *Uses and Dangers of Oxygen Therapy*. Edinburgh, H.M.S.O., 1969.

Vasectomy

SIR,—Although regeneration of the vas deferens is frequently cited as a possible cause of failure of the operation of vasectomy there appear to be very few accurately documented reports of this process. The following case therefore seems worthy of record.

A patient, a normal adult of 41, had previously had a vasectomy carried out by another surgeon on eugenic grounds, but continued to produce normal quantities of spermatozoa in his seminal fluid. Fortunately, histological specimens of the previous operation were available. These showed that although vas tissue was in the specimen taken from the left side, only blood vessels were found in the specimen on the right. It was therefore thought to be a simple situation where the vas had not been ligated on the right side and therefore vasectomy in the normal fashion on the right side was carried out, and a normal vas was in fact present. Seminal specimens were examined in the usual fashion post-operatively but were still found to contain normal numbers of spermatozoa.

Although there was no clinical evidence of a double vas on either side this seemed to be the most likely cause for the persistence of spermatozoa in the seminal fluid. It was therefore decided to re-explore both sides under general anaesthesia. Satisfactory division of the vas on the right side was found. Histological examination confirmed that there was no continuity in the vas, and no second vas was found. On the left side there was complete continuity of the vas and no second vas was present. At one point there was scar tissue around the vas. This area was excised together with a length of healthy vas on either side. Histological examination of this specimen showed that regeneration had occurred. There was normal epithelium present and there was continuity between the two ends past the site where inflammatory reaction had occurred. Following the operation the patient rapidly developed azoospermia and there has been no sign in the six months since this was carried out of any further appearance of spermatozoa in the seminal fluid.

An important point in this case would appear to be that regeneration almost indistinguishable from normal tissue had occurred in less than six months from the date of the primary operation in a vas, which had been divided and resected with histological proof of the fact. Obviously, therefore, precautions to prevent this are extremely important. The exact length of vas removed at the original operation is not known but it is believed to have been about 1 cm. In my experience, a very reliable method of preventing such an event has been to separate the planes of tissue in which the two ends of the vas lie as well as to take the normal precautions of coagulation and ligaturing the two ends of the vas. I have found that a purse string suture around one end of the vas allows it to be invaginated into a deeper plane of the areolar tissues than the other end. I have had no failures following this method of performing the operation.—I am, etc.,

GEORGE T. WATTS.

General Hospital,
Birmingham.

SIR,—I am sure Dr. L. N. Jackson (14 November, p. 436) is right to point out that male sterilization need have no untoward psychological effects, but I doubt whether he is justified in the criticisms he makes of Mrs. Helen Wolfers's carefully written paper (31 October, p. 297).

When he states that the Simon Population Trust "has heard from many thousand men who have undergone vasectomy and not one of these, nor their wives, has reported any adverse long-term psychological or physical effects attributable to the operation" he is merely showing up the defects of relying on the answers to a postal questionnaire as an adequate means of follow-up.

Mrs. Wolfers made it clear that in five of the seven couples interviewed by her there was a history of psychological disturbance prior to the operation as well. Surely the point is that vasectomy may be unsuitable for couples with psychosexual problems.—I am, etc.,

MICHAEL HUMPHREY.

Department of Psychiatry,
St. George's Hospital Medical School,
London S.W.17.

Fibrinolytic Activity and Oral Contraception

SIR,—Some time ago we had an opportunity of studying the blood fibrinolytic activity in ten women before and approximately three months after they had started taking chlormadinone acetate (0.5 mg. daily) for oral contraception. In all these subjects the blood fibrinolytic activity as estimated by the euglobulin lysis time (E.L.T.) method using an E.L.T. recorder¹ showed an increase at the end of three months (Fig. 1). Since the E.L.T. method is a specific method for estimating the plasminogen activator content in the blood the results indicate that these levels in the blood were increased by the drug. On the other hand, when the overall fibrinolytic inhibitor levels were estimated by the method of McNicol *et al.*² it was found that there was a fall in these levels at the end of three months while on chlormadinone (Fig. 2).

These results are in contrast to our previous findings,^{3,4} when a decrease was found in the blood fibrinolytic activity (estimated by E.L.T. method) in women taking chlormadinone compared with normal women. No significant difference in fibrinolytic inhibitor content was observed when normal women were compared with those taking chlormadinone. This discrepancy is surprising, and it is suggested that when comparisons of fibrinolytic activity are made it would be advisable to do so "within patient basis." Comparing subject to subject may not show the true picture as it is well established that the fibrinolytic activity

varies from person to person.—We are, etc.,

I. SUDHAKARAN MENON.
MARY PEBERDY.

Royal Victoria Infirmary and
University of Newcastle,
Newcastle upon Tyne.

REFERENCES

- 1 Menon, I. S., Martin, A., and Weightman, D., *Laboratory Practice*, 1969, 18, 1186.
- 2 McNicol, G. P., Gale, S. B., and Douglas, A. S., *British Medical Journal*, 1963, 1, 909.
- 3 Menon, I. S., Peberdy, M., Rennie, G. H., Weightman, D., and Dewar, H. A., *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 1970, 77, 752.
- 4 Menon, I. S., and Dewar, H. A., in *Clinical and Physiological Aspects of Fibrinolysis*. London, Churchill (in press).

Brucellosis in Veterinary Surgeons

SIR,—I have only recently received the *B.M.J.* in which Dr. W. R. Thrower (11 April, p. 69) makes the following statement concerning brucellosis: "A recent survey (to be published) was made among veterinary surgeons in Northern Ireland, a region where cattle keeping standards are high. Out of 123 subjects examined, 100 had significant agglutination titres in their sera and 50 showed clinical evidence of infection."

I can only presume that Dr. Thrower is referring to the survey carried out by me in 1967 with the bacteriological assistance of Dr. W. J. McCaughey, as I know of no subsequent clinical and serological survey of this type in Northern Ireland.

I should like, therefore, to make several comments on Dr. Thrower's statement. Firstly, the figures quoted are not accurate and refer only to the anti-human globulin (Coombs) (A.H.G.) test. Such a high percentage of positive tests was found only in veterinary surgeons working in private practice. Another group of veterinary surgeons, employed by the Ministry of Agriculture in Northern Ireland, had less evidence of exposure to brucella antigen, which they may even have acquired in previous private practice.^{1,2} Secondly, to say that 100 had significant agglutination titres in their sera is misleading. One hundred and seven out of 118 veterinary surgeons in private practice had an A.H.G. titre of 1/20 or greater against *Br. abortus* antigen,¹ but there is no evidence that I am aware of to suggest that this means anything more than previous exposure to infection. Previous authors have not specified what they consider to be a significant level of A.H.G. titre,^{3,4} but a complement-fixation titre of 1/16 or greater has been said to represent activity of infec-

tion.³ I was able to show with this group of veterinary surgeons that it was very difficult to relate serological titres obtained by either the A.H.G. test or the C.F. test with symptomatology. There was some suggestion that an A.H.G. titre of 1/160 or greater, or a C.F. titre of 1/128 or greater, was more likely to be obtained in those with recent symptoms of brucellosis, but 33 veterinary surgeons out of a total of 110 had A.H.G. titres of 1/160 and no symptoms and 15 had C.F. titres of 1/128 or greater and no symptoms.² I suggest that the meaning and, therefore, the value of these tests in clinical practice is still far from clear.

I would particularly take issue with the statement that 50 of the veterinary surgeons showed clinical evidence of infection. In fact, only 17 out of 121 veterinary surgeons in private practice had symptoms which were consistent with a diagnosis of brucellosis and serological evidence of exposure to infection.⁵ A total of 48 veterinary surgeons admitted to symptoms, past or present, which were in the brucellar range. Of these, 31 claimed to have had brucellosis previously, but a maximum of 19 of these diagnoses appeared to have been justified. Of the 25 veterinary surgeons who complained of symptoms within the previous one year, six were probably suffering from some other disease and two had negative serology, thus excluding the diagnosis.³

I do not wish to suggest that brucellosis is not a serious health hazard to veterinary surgeons, but rather that it tends to be over-estimated. When one considers the vagueness and the multiplicity of the symptoms of brucellosis and the large number of veterinary surgeons who have evidence in