to acknowledge the power these images have in determining our so-called attitudes.

Having now (at least on paper) relegated emotional contexts of the controversy to their rightful origins, can we not bring ourselves to examine a little more dispassionately to what extent "natural motherhood" is possible or expedient in a highly artificial society? In the "natural" society, no duties are imposed on the lactating mother (the Aberdeen report substantiates the benefit two women similarly placed derived). The baby can be put to the breast in public or, if not, privacy is assured. The "natural" mother has never been a clockbound precision worker. And, perhaps most important, she has never before been as isolated in her own four walls, with all the terrors that a crying baby can arouse, as to-day. In this state, it is no consolation that one can go to the clinic to-morrow or sit in the doctor's waiting-room to-night, "feeling an awful fool," when what is needed, and what the State cannot replace adequately, is the older, experienced woman who in a "natural society" is always present to reassure or take over at the inevitable critical moment.

Must we not then, being what we are, re-examine the premises of "the bad, rejecting mother who can only offer substitutes and second bests"? The alternative may well prove to give potentially excellent modern mothers a feeling of inferiority and guilt that must alienate them from the child, whereas it can in no way alter the social realities conducive to "the bottle." By all means let us attempt to arrange optimum lactating conditions for those who want to be helped to them, or those who would like "to try," but let us no less enthusiastically support the right of the differently conditioned mother to enjoy *her* motherhood with the reassurance she will derive from weights and measures. There may well be a swing of the pendulum towards a way of life less traumatic to all our human needs, and none would welcome it more than I, but in the meantime we shall only help one another and ourselves by humbly seeking to understand, instead of condemning manifestations of the moment. Perhaps we are less bad than we think.—I am, etc.,

London, W.11.

ELEANOR ETTLINGER.

SIR,—It was left to Dr. R. N. Compton Smith (*Journal*, March 8, p. 581) to mention the real difficulty about infant-feeding. A girl is shy of putting a baby to the breast, even before her mother-in-law: yet if the same girl spent the day half-naked on a crowded beach, no one would bat an eyelid. A few film-stars appearing on cinema and television screens suckling their babies might do more to correct this anomaly than all the efforts of the Health Services.—I am, etc.,

Isle of Wight.

N. P. BRUCE.

Artificial Insemination

SIR,—I read with interest Dr. Sheila Wood's letter on A.I.D. (*Journal*, March 8, p. 582), as it represents a familiar opinion. Her solicitude for the emotional welfare of a childless wife of an infertile husband provides, however, a convenient channel for the emotional cowardice which epitomizes this "enlightened" age. If a woman feels an urge for a child so great that consideration for her husband will not quell it, then let her admit to her need and have a child by the more usual means and go out to work to keep it. One cannot have everything.—I am, etc.,

London, S.E.15.

H. J. ANDERSEN.

SIR,—There are three points on which I must join issue with Dr. Sheila Wood (*Journal*, March 8, p. 582). First, I feel that most right-minded men and women including the vast majority of infertile couples, can and do judge the "merits" of A.I.D. far better than those who become emotionally involved, either personally or professionally. A.I.D. may give an individual, or couple, a desired result, but only at the cost of secrecy, deception, conspiracy, fraud, and a breach of the marriage contract.

Secondly, a woman to whom the denial of pregnancy would mean "the total failure of [her] existence" is both

emotionally and spiritually immature; not just endowed with a highly developed maternal instinct, as Dr. Wood suggests. A woman whose life is centred solely round her reproductive function puts her whole reason for existence on the purely animal level and is in sore need of both psychological and spiritual guidance.

Lastly, I am convinced that an adopted child is as precious to a normal mature woman as one she has borne herself; even where a natural child of the marriage has followed on an adoption. Where adoption has failed the cause has been either the method by which it was arranged (e.g., direct adoption) or faults in the adopters themselves, both of which are happily preventable in adoptions arranged through a society of proved merit. The adopted child, unlike that born of A.I.D., bears an equal relationship to both parties of the marriage. Their love and relationship are further enriched by the fact that an open and honest explanation to the child, from its earliest years, is a *sine qua non* of a successful adoption. Only foolishness and weakness in concealing the facts lead to disaster in the relationship—a danger which is obviously all the greater in a case of A.I.D., since the truth cannot be told without disastrous results, while concealment results in a permanently false and dishonest relationship to the family and to the community. I speak as an adopter, as an Anglican, and as a practitioner with long experience of investigating and, I hope, helping childless couples.—I am, etc.,

Upton-by-Chester.

HORACE INGHAM.

Tetanus

SIR,—We have read the papers by Dr. D. D. Johnstone and Drs. R. S. Packard, T. B. Cartmill, and J. G. Henry on the treatment of tetanus (*Journal*, January 4, pp. 12 and 16) with great interest. No one could reasonably dispute the importance of assessing prognosis before deciding the method of treatment.

According to Cole¹ the period of onset is the best prognostic yardstick, and Dr. Johnstone states this is usually more accurately measured than the incubation period. But is it? Our difficulty in this hospital (the General Hospital, Penang) has been to define what is meant by the term "generalized spasm or convulsion" so that the crucial onset period may be estimated (and so that the attending nurses and doctors know what they—and others—are writing about when they record such events). Cole described a reflex spasm as consisting of the following: (1) The whole body suddenly becomes rigid; (2) the jaws are tightly clenched; (3) the neck and trunk are arched backwards; (4) the limbs are extended; (5) the patient foams at the mouth and becomes cyanosed; (6) respiration may be stopped. The severity of these spasms can be graded according to duration (few seconds to three or four minutes), frequency, and whether they occur spontaneously or only follow a precipitating factor. This is an admirably concise description which might well be adopted as the criteria of a "generalized spasm" (although the severity remains to be defined). But in patients observed here there has appeared to be an infinite variation—in extent—from a local twitch involving, say, the hand only or the neck to the full-blown spasm with the six components enumerated. At what stage do these "minor" spasms qualify for the term "generalized"? Patients have been observed to die during or immediately after a spasm in which no limb extension, frothing, or cyanosis was seen. Are such spasms "generalized"? In many instances the patient is admitted after the onset of spasms, and in these cases it is virtually impossible from the descriptions available to estimate the period of onset with any accuracy if the five criteria have to be fulfilled.

Doubtless the period of onset if clearly defined is helpful in retrospective grading of cases, but is it reliable in assessing individual patients on or shortly after admission? Dr. Johnstone says it is not justifiable to use hazardous methods of treatment in "mild" cases. Yet one-third of his "mild" cases ended in death. To us it is often difficult to explain why some patients survive yet others die. Excluding fulminating toxæmia, the two main lethal factors seem to be chest infection and spasms. In pre-

venting chest infections, we believe the prone position is all-important. "Keep them off their backs" should be the constant watchword to nurses, who may not remember that the trachea runs backwards and that unconscious or drugged patients on their backs will therefore retain respiratory secretions which can become infected in a few hours. Inhalation of vomit is much less likely to occur if the patient is lying prone. We are convinced this simple manœuvre is much more important to the patient than "preventive" antibiotics, "frequent turning," "laryngeal toilet," etc. Another excellent reason for "keeping them off their backs" is lessening the danger of phlebothrombosis, particularly in the soleus intramuscular veins. Gibbs² showed this to be the most common site of phlebothrombosis and noted that dependent drainage here will be obtained when the patient is lying prone.

Prevention of spasms can only be achieved with certainty by relaxants such as curare. But this automatically entails artificial respiration with all the complexities, strain on medical and nursing staff, etc. If one advocates such a regime for severe tetanus, what are to be the indications for adopting it in a particular case? Is it to be the first "generalized spasm" as defined above? A patient recently observed died in the first such spasm. Is failure of conservative methods to be the criterion? Another patient was admitted with frequent spasms which were apparently controlled satisfactorily by thiopentone and chlorpromazine. On the fourth day, without any obvious warning, he then had a general spasm and died. If curarization and intermittent positive-pressure artificial respiration (I.P.P.R.) regime is started, when should it be stopped? One patient with a 24-hour onset period was treated by the respiration unit here for 13 days with the I.P.P.R. regime, but continued to have general spasms for 48 hours after the machine was stopped. Are such spasms hazardous to life even though the intoxication is on the wane?

There are so many questions to which there seems no satisfactory answer. Indeed, there are so many variables in tetanus that the greatest caution is needed in concluding any particular line of treatment is an advance—for the patient. Creech and his colleagues,³ in a masterly review of probably the largest published series ever treated in one hospital, showed a falling trend in mortality interrupted by "bad years" over the last 50 years, a fall rather similar to tuberculosis before the days of streptomycin and isoniazid. The trend could not be convincingly correlated with any of the so-called advances such as antiserum, sulphonamides, antibiotics, tracheotomy, or gastrostomy. In 1949 they had 19 successive patients, all of whom recovered. As they remark, if some new treatment had been tried out during these admissions, it would have been tempting to attribute the success to the innovation. But the final mortality for that year was higher than any other year from 1946 to 1956. Dr. Johnstone had the courage to withhold antiserum. Would it be justifiable to withhold sedative drugs as well? How many patients are killed by the "treatment"?—We are, etc.,

H. A. REID.
D. P. BOWLER.
L. T. SCOTT.

Penang, Malaya.

REFERENCES

- ¹ Cole, L., *Brit. med. J.*, 1953, 1, 150.
- ² Gibbs, N. M., *Brit. J. Surg.*, 1957, 45, 209.
- ³ Creech, O., Glover, A., and Ochsner, A., *Ann. Surg.*, 1957, 146, 369.

Aural Deformity and Renal Agenesis

SIR,—I wish to refer to the report by Dr. D. Hilson entitled "Malformation of Ears as Sign of Malformation of Genito-urinary Tract" (*Journal*, October 5, 1957, p. 785). Dr. Hilson's findings are of unusual interest, and are borne out in a report entitled "Bilateral Agenesis of the Kidneys," by Dr. James H. Duxbury.¹ A photograph in Dr. Duxbury's report (Fig. 1) of a profile view of the head and face of the patient under discussion reveals abnormalities of the ear strikingly similar to those noted in the patient pictured in Fig. 11 of Dr. Hilson's article. It is possible that Dr. Hilson has described an association of findings which, if confirmed by other reports of similar findings, might warrant permanent recognition and incorporation into the teachings of physical diagnosis.—I am, etc.,

California, U.S.A.

GEORGE X. TRIMBLE.

REFERENCE

- ¹ Duxbury, James, *Canad. med. Ass. J.*, 1958, 78, 123.

Acute Otitis Media

SIR,—There are some comments I would like to make relating to your leading article on acute otitis media (*Journal*, February 8, p. 328). You state that "few will disagree with F. W. Davison, who found that otitis media occurs more often in children with enlarged adenoids; experience in Great Britain would generally confirm that adenoidectomy prevents recurrences in a high proportion of cases." Earlier, however, you report that "such data as were obtained [by the M.R.C. Survey] on the significance of tonsillectomy and adenoidectomy in relation to recurrence of acute otitis media showed no difference that could be attributed to the operation." It therefore appears that the report does not justify your statement.

Secondly, you state that "the lower incidence of acute otitis media in adults is in part due to the atrophy of this lymphoid tissue [involving the Eustachian tubes], which usually takes place after puberty." This must surely be regarded as a very minor factor, since the dramatic decline in the incidence of acute otitis media occurs at about 7 years of age.—I am, etc.,

Buxton, Derbyshire.

J. MCA. WILLIAMS.

Research Workers and Pre-registration Year

SIR,—Professor L. J. Witts suggests (*Journal*, January 25, p. 222) waiving the pre-registration year for full-time research workers, who have, by implication, qualified in medicine to get more money. As an ex-student of his, without disloyalty, may I suggest that that is infamous? On qualification, it is now up to a prospective doctor to learn his job, and to prove that he can discharge it, in the compulsory pre-registration year. If in the future any of these half-fledged doctors have to practise medicine—for instance, in the unhappy event of an atomic war—I shudder to think of the consequences.—I am, etc.,

Newquay, Cornwall.

M. C. FFRENCH-CONSTANT.

Carpal Tunnel Syndrome

SIR,—Encouraged by the good results of treating stenosing tendovaginitis of the radial styloid and the trigger fingers with the local injections of hydrocortisone, I have during the last 18 months been treating cases of carpal tunnel compression syndrome by the same method. It was Nissen¹ who reported thickening of the flexor tendon sheaths in the carpal tunnel in cases of the syndrome and who suggested that this thickening might reduce the capacity of the tunnel and so compress the nerve. Phalen and Kendrick² observed thickening of the "flexor synovialis" in 70% of cases on which they operated. They too have treated 20 of their more recent cases with hydrocortisone injections, with good results in 16. In two of my cases treated by operation, granulation tissue was observed to be investing the tendons, and in others a thickening, as described by Nissen, was noted. It would appear therefore that a chronic tenosynovitis is the most likely cause of the syndrome.

Twenty-four cases of the syndrome have been treated by local injections of 0.5–0.75 ml. of hydrocortisone acetate into the tunnel. The needle has been inserted from the wrist at a point on the ulnar side of the palmaris longus tendon. Three patients were eventually operated on; the first, having osteoarthritis of the wrist and carpus, did not obtain any relief from the hydrocortisone; the other two obtained a dramatic response to the injections, but one was submitted to operation to find out why she could not flex her fingers fully, and this patient was one of those who had granulation tissue covering the tendons in the tunnel and in the finger sheaths. The symptoms of the third case recurred when she returned to work as a weaver on three or four occasions, and, having developed a phobia about injections, she asked for operative treatment.

Eight patients have been completely relieved for nine to eighteen months; four had only one injection, the other four had received two or three injections. Three cases have been relieved for periods between three and nine months; one relapsed slightly five months after his first visit, another ten months afterwards, but both responded to another injection. Seven patients have been completely free of symptoms for periods varying from one