

the available evidence places it at about 120 males to 100 females conceived. It is easy to show that an increase in the abortion rate will lower the sex ratio at birth while a decrease in the abortion rate will raise it. One possible explanation, therefore, for the steady rise in the sex ratio during the last half-century is a progressive fall in the abortion rate, perhaps the result of steadily improving antenatal care.

By making use of the considerations in the above paragraph, I was led to the conclusion that the spontaneous abortion rate is probably nearer 20% than the 10% figure usually quoted. In a series of pregnancies occurring among women attending a fertility clinic (where the likelihood of overlooking spontaneous abortion was small) the abortion rate was 19.1%; the sex ratio of the babies born was 107 males per 100 females. Being so close to the general sex ratio at birth, it was reasonable to suppose that the abortion rate was not dissimilar from the general one. Had the latter in fact been only 10%, the sex ratio of the babies in this series should have been about 92 for a 19% abortion rate.—I am, etc.,

London, W.C.1.

G. I. M. SWYER.

SIR,—For the research worker much of the interest of variation in the sex ratio of live births has derived from the possibility that it reflects variation in the ratio at conception. Since the sex ratio of live births is influenced by the incidence and sex ratio of fetuses lost as abortions and stillbirths, reasons have recently been suggested for doubting whether this interpretation is correct.¹ The composition of abortions and stillbirths by cause of death changes from month to month of gestation, and the various causes have different sex ratios. Indeed, it is not even certain that the sex ratio of the same cause of abortion and stillbirth remains constant throughout pregnancy—for example, the sex ratio of anencephalus (expressed as the percentage of males) increases from about 20 at 7 months to 40 at 9 months. It is therefore certain that variation in the sex ratio of live births in association with war, birth order, maternal age, and social class is influenced by changes in the incidence and composition of abortions and stillbirths, and it is possible that there are no corresponding changes in the ratio at conception.

In view of these results the statement in your annotation (*Journal*, July 18, p. 142) that "variations in the sex ratio at birth are due to variations in the composition of the birth orders forming the population" is clearly inadequate. And since birth order is only one of the variables with which variation in incidence and composition of abortions and stillbirths is associated, the statement is also inaccurate.—We are, etc.,

Birmingham.

C. R. LOWE.
THOMAS MCKEOWN.

REFERENCE

- ¹ McKeown, T., and Lowe, C. R. (1951). *Hum. Biol.*, 23, 41.

A.C.T.H. in Delirium Tremens

SIR,—Within the last few months I have had the opportunity of treating two cases of delirium tremens with A.C.T.H. I am not aware of any similar published cases, though there may be many, but the results of treatment were so rapid and favourable in both cases that it might be worth while recording these.

Case 1.—A man aged 30 years was admitted to hospital acutely delirious and with the usual hallucinations. The attack may have been precipitated by a wound, accidentally sustained, on the back of the right hand which severed one of the extensor tendons. In a few days, after treatment with sedatives, vitamin-B complex orally, and large doses of vitamin B, parenterally, the mental condition had considerably improved and it was decided to suture the cut tendon. He was anaesthetized with thiopentone intravenously and the operation was completed in 18 minutes. From this day the patient sank into a state of low muttering delirium; his temperature rose and ranged between 100° and 103° F. (37.8° and 39.4° C.), and he became completely disorientated as to time and place. Blood films were negative for malarial parasites. He lay on his back and frequently reached forward with his hands, going through the "rope-climbing"

movements which are said to be typical of serous "meningitis" (wet brain). Lumbar puncture produced normal cerebrospinal fluid at a pressure of 110 mm. Penicillin, streptomycin, and sulphadiazine had no effect on the fever.

Fourteen days after the operation and 20 days after admission to hospital, it was decided to try 20 units of A.C.T.H. every six hours. After two doses the mental condition had improved almost to normal, and in 24 hours he was perfectly sane. The temperature dropped to normal in 24 hours and convalescence was rapid after that. The A.C.T.H. was gradually reduced to 5 units once daily, and this was continued for three days and then stopped.

Case 2.—A woman aged 57 years was admitted to hospital in subacute delirium, completely disorientated as to time and place, restless and running a temperature of between 98° and 100° F. (36.7° and 37.8° C.). After 24 hours she was given 40 units of A.C.T.H. every eight hours, and after one dose the mental condition was almost normal. Recovery was rapid and the patient was discharged in six days. The A.C.T.H. was stopped after four days, by which time it had been reduced to 20 units once daily.

The striking feature of these cases was the extremely rapid response to A.C.T.H. Case 1 was in a critical condition, so much so that a relative had been notified and was about to fly out from England, but he responded to treatment almost as quickly as the other case. Twelve hours after the start of treatment he was able to speak intelligently on the telephone.—I am, etc.,

Georgetown, British Guiana.

S. C. BETTENCOURT-GOMES.

Malaria in Ex-Servicemen

SIR,—Last year a number of attempts were made to draw the attention of practitioners to the possibility of malaria occurring in men recently returned from military service in the Far East.¹ A steady stream of such cases with *P. vivax* (B.T.) malaria is still being admitted to the Hospital for Tropical Diseases (U.C.H.), London. Unfortunately, owing to misdiagnosis, a proportion are unnecessarily ill on admission here. Some give a history of fever for two months or more, having a haemoglobin between 50 and 60%, and spleens palpable well below the left costal margin. They have been treated for pyelitis, bronchitis, or gastro-enteritis. The majority have served in Korea.

It should be realized that proguanil, which is issued daily to soldiers in malarial areas, is an excellent suppressive drug but not a causal prophylactic for B.T. infections. Therefore, if these men were taking it as ordered, they will give no history of a previous attack of malaria. The latent period is often long in the Korean cases. Six months or more may elapse between the stopping of proguanil and the onset of fever.

If the possibility of malaria as the cause of fever in men who have served in the Far East is remembered, a positive blood slide will establish the diagnosis, provided that no antimalarial drug has been given. It is with the hope of bringing this condition once again to the attention of general practitioners throughout the country that this letter is written.—We are, etc.,

London, N.W.1.

A. F. KNYVETT.
F. D. SCHOFIELD.

REFERENCE

- ¹ *Lancet*, 1952, 2, 45, 288.

Rubella

SIR,—During the last two years I have seen three cases of German measles associated with "rheumatic" pains. All three were women, between the ages of 25 and 35 years. All three complained of pains in the eyes, which they felt on moving the eyes, thus suggesting that the origin of the pain was in the external ocular muscles.

All three, however, did not recover immediately. One, a lady of about 27, was left with a pain in her left wrist joint, and she was referred to an orthopaedic consultant. His opinion was that this was a simple arthritis of infective origin. It did not respond to salicylates, but subsided eventually. The second lady, aged 34, who was treated with salicylates as soon as German measles was diagnosed, was left with generalized rheumatic pains in her back, arms, and

legs. This went on, although gradually subsiding, for about seven months. The third lady, aged 32, who was also treated with salicylates, recovered completely in ten days.

In J. J. Conybeare's *Textbook of Medicine*¹ there is the sentence: "Neuralgic and 'rheumatic' pains and mild encephalitis have occurred as complications [of German measles]." I have never seen the encephalitis, but neck stiffness, due to occipital lymph node enlargement, together with pain on moving the eyes (not photophobia), did once suggest it.

In allergic conditions, particularly in serum sickness, similar pains in the joints are encountered, which may persist long after the acute condition has passed off in one or in many joints. These pains are sometimes referred to as polyarthralgia, which, of course, describes the symptom but not the disease.

For my part, I regard these phenomena as similar to those which we find in the beginning of the majority of infections. The pains are usually in the muscles, and it would appear that in the more severe conditions, lesions, presumably exudative, occur in the joints. In my experience, in German measles there is a tendency for the pains to persist for some months afterwards. Conybeare has it that a "multiple arthritis may occur in the course of many diseases, such as pneumonia, dysentery, scarlet fever, pyaemia, syphilis, gout, and typhoid." Beaumont, in his book *Medicine*,² mentions that some authorities believe that focal sepsis produces "a chronic allergic state, which on stimulation results in acute rheumatism."

In my view, the similarity between the joint pains in serum sickness and those in German measles—and indeed many other infective conditions—should lead us to regard these phenomena as allergic.—I am, etc.,

Barnard Castle.

K. H. PICKWORTH.

REFERENCES

- ¹ *Textbook of Medicine*. E. and S. Livingstone, Edinburgh, 1949.
² *Medicine*. J. and A. Churchill, London, 1953.

Transverse Arrest of the Head

SIR,—Although not wishing to prolong this correspondence, I think a few simple facts concerning Kielland's forceps may call for emphasis. Dr. E. Parry Jones's letter (*Journal*, June 6, p. 1278), which I read with great interest, is a notable account of the history of these forceps, but it taught me scarcely anything about their use that I did not learn nearly 20 years ago. I certainly doubt whether as a result of its publication present-day British obstetricians is going to see a great surge back to Kielland's, nor, indeed, will Dr. Parry Jones expect it; they have a place, but essentially a limited one. So far as their use by general practitioners is concerned, surely comparatively few possess a pair, since their purchase would hardly be justified in domiciliary practice. I use my own pair quite rarely in spite of being very fond of them, and so far as I know I use them as aptly as most obstetricians. The method of application surely is very much a matter of personal preference. I do not care for the "wandering method," but every craftsman differs slightly in his methods, and it seems rather pedantic for Mr. Bryan Williams (*Journal*, April 18, p. 865) to declare that "the classical method . . . is nowadays unnecessary," though he may be assured I have always admired him as a sound obstetrician.

As a practising obstetrician I hope I shall be required to deal with transverse arrest of the head many more times, and I shall deal with each case slightly differently, as befits any artist loving his craft. I regard Kielland's forceps as another trick in the bag of the experienced obstetrician, but I cannot quarrel with one of Professor Gilbert Strachan's eminence when he wrote some years ago, "The main indication for their use is transverse arrest of the head . . . but this type of case is rare and can usually be treated by other methods, while the forceps are difficult to apply. Therefore, although they attracted some favourable attention when they were first introduced, their use has largely been abandoned."¹—I am, etc.,

Liverpool.

H. VINCENT CORBETT.

REFERENCE

- ¹ *Textbook of Obstetrics*, 1947, p. 630. London.

Generalized Actinomyces with Possible Cardiac Involvement

SIR,—I read with interest the case reported by Drs. R. S. Savidge and D. M. Davies (*Journal*, July 18, p. 136). I note that a blood culture yielded a spore-bearing organism which was rejected as a contaminant. In the final sentence of their discussion they stress that in cases of doubtful pyaemia an unusual spore-bearer isolated from a blood culture should not be lightly disregarded.

Had the spore-bearing organism from this patient's blood stream been further investigated it would have been of but little assistance in arriving at the correct diagnosis, since *Actinomyces bovis* does not, of course, form spores.—I am, etc.,

London, S.E.1

KEVIN ANDERSON.

Controlled Respiration and Apnoea in Anaesthesia

SIR,—Drs. G. J. Rees and T. C. Gray (*Journal*, July 11, p. 97) refer to an article by Cournand and his colleagues written in 1948.¹ I think it only right to point out that this article refers to some experiments carried out on the cardiac output of 29 human subjects when exposed to various types of intermittent positive pressure breathing in the conscious state, using respirators that "followed the slightest breathing efforts of conscious patients."

Undoubtedly there was little disturbance of cardiac output when the correct type of intermittent pressure was used, but can it be assumed that these results apply equally well to the apnoeic, curarized patient? They may give a sense of security to the anaesthetist who is using "assisted" respiration, but I should like to see some convincing results from experiments which measured the cardiac output during "controlled respiration" in the completely atonic, apnoeic patient. Further, the investigation should cover both cases with and without their chest cavities open, because I feel that the problem is different in the two types of case. We are all genuinely gratified (and perhaps sometimes surprised) to see how well thoracotomy cases do under modern anaesthesia, but for other non-thoracic cases I must agree with Professor E. A. Pask (*Journal*, May 9, p. 1048) "that there are critically ill patients who do better if they are allowed to breathe for themselves." It might be as well to recall the generalization made by Dr. F. Prescott in 1947,² that there was one maxim to be followed whenever a curarizing drug was used: Keep the patient breathing. Perhaps it is old-fashioned to try to keep to this maxim, but I would still strongly recommend its acceptance where possible, particularly by junior anaesthetists.—I am, etc.,

London, N.18.

G. S. A. KNOWLES.

REFERENCES

- ¹ *Amer. J. Physiol.*, 1948, 152, 162.
² *Proc. roy. Soc. Med.*, 1947, 40, 596.

Tic Douloureux

SIR,—Dr. R. Wyburn-Mason's article entitled "The Nature of Tic Douloureux" (*Journal*, July 18, p. 119) prompts me to correct a common terminological error which has arisen owing to the incorporation of the unfortunate diagnostic label "tic douloureux" into the English language. The seemingly appropriate translation "painful tic" is, in fact, incorrect and misleading. "Facial neuralgia" is preferable, since the condition is predominantly a sensory disorder, and it avoids the use of the term "tic," which has a specific definition completely unrelated to facial neuralgia.

The word "tic" has appeared in the French literature for the past 300 years to describe such characteristic motor phenomena as eye-blinking, nose-twitching, and head-flinching. It was first defined precisely by Meige and Feindel¹ as:

"a co-ordinated purposive act, provoked in the first instance by some external cause or by an idea; repetition leads to its becoming habitual, and finally to its involuntary reproduction