There have been others, too, in which no parasites were found in thick and thin blood films at the time of examination, but where the balance of evidence was in favour of subtertian malaria. It is undoubtedly true that occasionally, especially in the most acute and dramatic cases, the ring forms of the parasite may be particularly difficult to find in blood preparations. I have seen relapses of benign tertian malaria as well in which the same obtained but where the character of the rigor, the subsequent hot and sweating stages, and the enlarged painful spleen could leave no doubt in the clinician's mind of the correctness of the clinical diagnosis. Sometimes the discovery of malarial pigment in the mononuclear cells is confirmatory evidence.

In quartan malaria I have in mind two cases in which, although the fever and quartan periodicity were suggestive, thin blood films were searched for hours with a negative result. Finally, in one instance, however, after 2½ hours' scrutiny a single quartan parasite hove into view in a thick blood film. Intramuscular injections of quinine in these instances clinched the diagnosis by abolishing the fever when other antimalarial drugs, including mepacrine ("atebrin") and proguanil, had failed.

The same principle similarly obtains in amoebic dysentery. It is curious that here, too, in the most acute cases with profuse diarrhoea *E. histolytica* in its active stages may be most difficult to demonstrate. I can recollect a fulminating case of this disease of an acuteness and toxicity rarely encountered, but when the proctoscope was brought into use, and scrapings made from the bleeding mucosa, these parasites were present in large numbers and the diagnosis became obvious. Another instance I can call to mind was that of an officer who when playing tennis at Wimbledon was attacked with acute diarrhoea with blood and mucus. Such was the character of the stools and the severity of the illness that he was regarded as suffering from ulcerative colitis till some three weeks later emetine was injected as an experiment and at this point the attack began to subside. Sigmoidoscopy was then performed, and active amoebae were present in the exudate in large numbers, in spite of the fact that the stools had been examined microscopically many times with a negative result. It then transpired that as a boy of 11, some 15 years previously, he had contracted amoebiasis in Mauritius.

Another difficulty crops up in intestinal amoebiasis where the lesions in the large intestine are typical of amoebic ulceration but no actual evidence of the parasite can be secured. I have had experience of many of these—some of which had been subjected to treatment for ulcerative colitis on previous occasions. Here surely is a basis for experimental therapy and for the verification of the "proof of the pudding is in the eating." If the patient recovers, and the ulcerations heal on emetine and emetine bismuthous iodide therapy, then the original clinical diagnosis must have been correct.

In these somewhat exceptional instances I am always reminded of the analogy of the rabbit. How many of us have seen their very characteristic burrows and other evidences of occupation, without obtaining so much as a glimpse of the owner, but it would be a foolish man who could swear that the occupant of the burrow is not somewhere or other ensconced inside, and this little parable is peculiarly applicable to *E. histolytica*.—I am, etc.,

London, W.1.

PHILIP MANSON-BAHR.

Primary Red-cell Aplasia

SIR,—We were interested in the medical memorandum from Dr. Michael Donnelly (*Journal*, February 21, p. 439) and the letter from Drs. A. G. V. Aldridge and P. Kidd (*Journal*, March 28, p. 729) regarding cases of red-cell aplasia in infants.

We have described a case of this condition fully in a male adult African (*J. Path. Bact.*, 1953, in press). He was referred to us for anaemia, and when we saw him was gravely ill. He said he had been getting progressively weaker for about nine weeks, and complained of general body pains, weakness, and dyspnoea. His heart and lungs were normal, there was no oedema, the spleen and liver were not enlarged, there were no petechiae, there was no icterus and no sign of riboflavin deficiency. There was histamine-fast achlorhydria. He gave no history of having been exposed to toxic substances. He had been taking mepacrine, but only in the usual doses, for suspected malaria. Blood examination on admission showed: red cells 1,320,000 per c.mm.; haemoglobin 2.3 g.%; mean corpuscular volume 54 μ^3 ; reticulocytes nil; white blood cells 5,650 per c.mm.; platelets 700,000. A sternal puncture done on admission showed complete red-cell aplasia, with normal white and megakaryocytic elements.

He was kept under observation in hospital for eight days, during which period there was no reticulocytosis. He was then

given 4 mg. daily of riboflavin for 14 days and 2 mg. daily for nine days. Seven days after the first dose of riboflavin reticulocytes began to appear in the peripheral blood (0.5%) and reached 27% on the twelfth day after the commencement of treatment. The reticulocyte response was therefore specific and reached the expected level. The red cells and haemoglobin doubled themselves in 21 days. The patient was discharged after six weeks in hospital with a blood count of 4,290,000 and 11 g.% of haemoglobin. Eight months after commencement of treatment the patient is perfectly well and has a red cell count of 5.5 million per c.mm. and a haemoglobin of 14.5 g.%. A sternal puncture done five days after the commencement of treatment showed 5% early erythroblasts, no late erythroblasts, and no normoblasts. A third marrow biopsy 12 days after the start of treatment showed 15% early erythroblasts, 27% late erythroblasts, and 34% normoblasts. Two more marrow punctures were done 23 and 30 days respectively after the commencement of treatment, and revealed that the early and late erythroblasts had decreased in number and the marrow had become predominantly normoblastic.

In view of the fact that so far all cases of true red cell aplasia have proved fatal, with the exception of one of Heilmeyer's cases and the two mentioned above, we think that our findings, if confirmed, may have a useful place in the treatment of this grave condition.—We are, etc.,

HENRY FOY.

ATHENA KONDI.

Nairobi, Kenya.

Who Was Geminus?

SIR,—To the many with a dilettantish interest in medical history the articles in your May 30 issue must have given special pleasure, although some statements may have caused surprise—for example, that the 1511 Act governing medical practice "did not apply to medical graduates of Oxford and Cambridge" (p. 1179). Were there any home-produced university medical graduates in 1511? By that date Aberdeen alone had given academic recognition to medicine, but it was merely studied as one subject in the general course, and no separate medical degrees were awarded at that time.

Some of the information about Geminus (p. 1187) seems to go further than the ascertained facts warrant. What is the evidence that he was "almost certainly a surgeon"? An individual (or individuals) with the name of Thomas Geminus (Gemynous, Gemynus, Geminie, Gemyne, etc.) is mentioned in various contemporary records—as a surgeon in the list of Edward VI's disbursements (1547–52), in the *Annals of the Royal College of Physicians* (1555), in the *Register of the Stationers' Company* (1555) as a stranger (foreigner) who was fined for calling a brother of the company a false knave and later as a benefactor to the "Brydewell," but he is not mentioned in the *Annals of the Barber Surgeons*. In addition the cognomen is associated with an excellent engraver, a printer, a cartographer, an instrument maker, and a putative author of books on anatomy. If one man possessed such widespread skill and erudition he must have been versatile indeed, and, although it is possible there was only one Geminus, an unusual name does not necessarily imply the same individual.

It is doubtful if the Geminus associated with the *Compendiosa totius Anatomie delineatio* was a medical man. Nicholas Udall in his "Address to the Reader" refers to him as a "workman," an unusual designation even in those days for a surgeon, anatomist, and author. When Vesalius complained of the plagiarism of his work by "that Englishman," he was not referring to Geminus, as your contributor infers, but almost certainly to Dr. John Caius, who had once lived and studied with Vesalius at Padua and who had returned to England in 1544, the year after the *Fabrica* and *Epitome* were first published. The Latin text used in the first two editions (1545, 1552) of the *Compendiosa* is a résumé of the *Fabrica*—it is the Vesalian *Epitome*—but in the third edition (1553), which purported to be a translation by Udall from the original Latin into English, the text actually used was a modified version of Vicary's (1548) *The Anatomie of the Bodie of Man* (itself largely pirated from de Mondeville's work), rearranged to follow the order of Mundinus's *Anathomia* (1316), which may be regarded as the first manual of practical anatomy, whereas Vesalius's *Fabrica* is a systematic treatise.

As Vicary raised no objections, while Vesalius did, we must assume that the former knew and approved of this use of his recently published work; but it is difficult to understand why the help of Nicholas Udall, *quondam* headmaster of Eton, prebendary at Windsor, and author of *Ralph Roister Doister*, was reputedly required to translate a book already in the vernacular (perhaps he translated only the plate legends which are copied

from the original Latin), or how the deception was not apparently commented upon by those familiar with Vicary's book. Caius has been blamed for instigating the plagiarism of Vesalius's great work, and it has also been suggested that Geminus is a punning pseudonym hiding the identity of Dr. Thomas Twyne, who translated other Latin works, but the association of the Vesalian plates with the Vicary script seems rather to implicate Vicary as the possible moving spirit behind this piece of artistic piracy.

There are other mysteries concerning Geminus, and not the least intriguing are how exactly his copperplates were acquired for French editions of the *Compendiosa* by Jacques Grevin, the French humanist, or how the same or similar copperplates were available for the *Anatomia Deusch*, first published in Nuremberg in 1551, and for other German and Dutch anatomical works. Apparently Geminus had widespread connexions. Other points arise from Dr. Underwood's remarks on Geminus, and one of topical interest will be mentioned. Is the engraving on the title-page of the 1559 edition of his book a portrait of Elizabeth I? It was intended to be Queen Mary, but Queen Elizabeth had ascended the throne before the edition could be published, so the engraving (possibly with a few alterations) remained, with a fulsome dedication to the new Queen added.—I am, etc.,

Manchester.

G. A. G. MITCHELL.

The Judet Operation

SIR,—Mr. T. M. Hennebry (*Journal*, May 23, p. 1166) has stated a case in favour of the Judet operation for osteoarthritis of the hip. I would like to question some of the points he raises.

It is not reasonable to compare the results of the Judet operation for osteoarthritis with those following a fractured femoral neck. In osteoarthritis the acetabular cartilage is grossly abnormal at the time of operation. In my experience it has been unusual to find a fractured neck of femur with marked osteoarthritis. It is likely that many of the cases of fracture following the Judet operation will subsequently become osteoarthritic and painful owing to the incongruity of the surfaces. This certainly occurs after other causes of incongruity. It may, however, require a number of years for these changes to occur, and I think it is fair to say that no one yet is in a position to assess the frequency of this complication. As the majority are in the elderly, with a high mortality in the ten years after operation, it is probable that a satisfactory statistical survey on this point will take many years. This last fact justifies its use in these cases.

I would agree that many cases of osteoarthritis have a thickening of the capsule and periarticular structures which restrict movement and are certainly, in part, responsible for the pain. Your correspondent suggests that it is in this type of case with a marked contracture that the Judet operation may fail. Osteotomy in this case is almost uniformly successful in relieving the pain. This, after all, is the predominant, and often the only, complaint of the patient.

As Mr. Hennebry says in his letter, this prosthesis is "far from being the final solution." This is becoming increasingly obvious with the passage of time. His suggestion that displacement osteotomy is a retrograde step is surely a failure to recognize its indications.—I am, etc.,

Harlow Wood, Notts.

J. P. JACKSON.

Treatment of Pulmonary Emphysema by Pneumoperitoneum

SIR,—We have read with particular interest the report of Drs. R. D. C. Brackenridge and A. Thelwall Jones on the treatment of pulmonary emphysema by pneumoperitoneum (*Journal*, May 23, p. 1135). For the past twelve months we have been engaged on, and have now terminated, a similar investigation on this very problem. Without wishing to anticipate the results of our report, which are now ready for publication, we wish to pass the following comments on their paper.

Vital capacity is an obsolete yardstick in the assessment of respiratory function. Three-second spirometric tests presuppose a high degree of experimental accuracy, and assume the absence of inspiratory dyspnoea.¹ Maximum breathing capacity is a much more accurate index of lung function, and there is much to commend its employment.² However, even here the skill, degree of experience, co-operation, and education of the patient are factors which have to be taken into consideration in the assessment of readings.

Broadly our findings are not as encouraging as those obtained by Drs. Brackenridge and Jones. Yet in a number of instances our emphysematous patients, after pneumoperitoneum, have expressed themselves as being subjectively improved in health and have stated that their dyspnoea has lessened. However, a critical evaluation of some of the variable factors involved in the assessment of dyspnoea—season, climate, intercurrent infection, bronchial spasm, and wishful thinking on the part of the patient—ought to make for the greatest caution by investigators in their interpretation of slightly beneficial or equivocal results. We feel that this view is probably true of the role of pneumoperitoneum in the assessment of pulmonary emphysema.—We are, etc.,

BERTRAM MANN.

E. A. MURPHY.

Halifax.

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- ² Donald, K. W. (1953). *British Medical Journal*, **1**, 415.

The Degradation of Flour and Bread

SIR,—I was interested to see Dr. E. H. M. Milligan's letter on the above subject (*Journal*, May 2, p. 994), for L. J. Harris¹ states that "most expert opinion now agrees that the average working-class diet in this country is so frequently low in vitamin B₁ that it would be an advantage to encourage an increased consumption of wholemeal bread." But of late there has been an increasing tendency for cases of vitamin B₂ deficiency to arise, as instanced by the frequency of beefy sore tongues and undoubted pellagrous dermatitis masquerading under the pseudonyms of neurodermatitis or even industrial dermatitis. According to Harris,² it is not uncommon for pellagra to occur in mental patients in this country, although it appears that the medical and nursing staffs escape this condition. It is open to question whether the hospital diet is at fault, or whether they are long-standing unrecognized cases of B₂ deficiency which had reached the last stage of all—dementia. When Sydenstricker visited this country during the late war he found a similar state of affairs. The fact that, despite the so-called restoration of flour, cases of nicotinamide deficiency do arise makes one ponder that all is not well; and it appears that matters will worsen when the extraction rate of flour is again raised. Furthermore, cases of acute encephalopathy and confusional insanity are known to arise, especially in the elderly, after severe operations such as hysterectomy or prostatectomy, which are similar in most respects to delirium tremens and curable with adequate doses of nicotinamide.—I am, etc.,

Heanor, Derbyshire.

PHILIP TURTON.

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Antabuse Addiction

SIR,—All those who deal with alcoholics have observed the variability of the antabuse-alcohol reaction in different subjects. This variability is to be expected, for the aversion effect of antabuse therapy does not depend on the physical symptoms when alcohol is taken—the vasodilatation, headache, tachycardia, and nausea—but on the concurrent subjective state of the patient. This is a feeling of impending personal catastrophe which, like tolerance of pain, varies from subject to subject. A great many psychopathic drinkers show a magnificent tolerance for acetaldehyde, and considerable bravado. They often claim that a small gin