of the "stationary" period, he may continue to be described as quiescent rather than as stationary.

(11) Among the definitions not mentioned is the Ministry's own term "inactive" as used in the classification of mass radiography findings. I think it a pity that the opportunity has not been taken to make clear the implications of the use of this term.

(12) The inclusion of directions for the medical assessment of patients suffering from pleurisy has the appearance of an intrusion in a document dealing with the classification of patients suffering from tuberculosis. I consider that the direction to regard pleurisy as tuberculous unless proved otherwise is inconsistent with the facts (a) that less than half of the patients who suffer from pleural effusion ultimately develop pulmonary tuberculosis, and (b) that pleural effusion is frequently part of a primary tuberculous process. The requirement that cases of pleural effusion should be regarded, and presumably notified, as tuberculous will have repercussions in the matter of the notification of primary tuberculosis in general.

(13) The new Non-Respiratory classification will not show the part affected. The old classification was in four groups: (1) bones and joints, (2) abdominal, (3) other organs, (4) peripheral glands. It is desirable that the subdivisions of the old classification be incorporated in the new groupings.

(14) If the old method of classification is to be amended, let us make sure it is soundly based, because it will involve the reclassification of all the cases on the dispensary registers so as to afford comparison with previous years.

In issuing their memorandum the Ministry have no doubt acted on the advice of the Joint Tuberculosis Council, but it is regrettable that no opportunity was given to tuberculosis workers in general to express an opinion before the recommendations were forwarded to the Ministry.—I am, etc.,

Preston.

F. C. S. BRADBURY,
Central Consultant Tuberculosis Officer.

Artificial Insemination

SIR,—There can be few subjects which call forth more emotional (as opposed to intellectual) judgments than A.I.D. The letters from Dr. Eustace Chesser (May 24, p. 738) and Dr. Farquhar Murray (June 7, p. 826) are cases in point. The latter is so carried away by his disgust of the whole procedure that he does not present the facts quite as they are. The only donors I have ever met are unselfish and intelligent men who do not sell or barter their spermatozoa but who agree to give them, as they would blood for transfusion, to help those in need—i.e., equally unselfish and intelligent couples who, because of the husband's complete sterility, have been deprived of begetting, bearing, and rearing the children they earnestly desire, more often than not through no fault of their own. Dr. Chesser, on his part, paints a somewhat lurid picture of the formidable hazards surrounding those rash enough to indulge in A.I.D. He says he has abandoned the practice but omits the facts and figures that induced him to do so. For example, he does not tell us how many cases he has dealt with by A.I.D., nor how old the resulting children are; neither does he give any indication as to what proportion of his cases has ended in psychological disaster; he seems to infer that there are few, if any, good results. Possibly facts and figures are lacking and he has arrived at his conclusions on theoretical considerations only. His letter would have carried more conviction had he also painted the other half of the picture, which undoubtedly exists.

The marriage in danger of being wrecked by the smouldering resentment of the maternally inclined woman against her sterile mate and the profound sense of failure and regret which must be felt by any but the most insensitive husband: in my experience this is the situation which, unless resolved, besets two people with "formidable hazards." There are of course a number of ways out, and it lies within the doctor's sphere to try honestly and without prejudice or emotional bias to help such couples, should they consult him, to find their particular solution. The possibilities are: to be sufficient unto themselves and to sublimate in various directions; to part and start afresh; to adopt; or, finally, to secure a child or children by A.I.D. A small proportion may after careful thought choose this last, the husband conscious of his wife's strong desire to bear a child and preferring to fend for one that is at least hers rather than

one that comes of neither of them. His sense of inferiority and of guilt tends to remain while the cradle is empty and is not, in my experience, replaced by jealousy of the child but by great tenderness towards it and towards a wife who has, in the sense that matters to him, kept faith; while she in her turn feels a lasting gratitude towards her husband for his generosity in allowing A.I.D. So far I have not met the unhappy situation Dr. Chesser so vividly depicts; quite possibly I have not dealt with so many cases as he has and my experience of the outcome of A.I.D. is therefore more limited than his. His warning will ensure a still more careful selection of cases. Finally I would suggest that it is early days to pass sweeping judgments in either direction. It is well to bear in mind that the demand for A.I.D., no less than for control of fertility, has come from the lay public. It is of prime importance that doctors should think about and weigh these things in the light of their special knowledge and experience, setting aside in so far as they can, or at any rate recognizing, their own particular emotional bias in the matter.—I am, etc.,

Crediton, Devon.

MARGARET HADLEY JACKSON.

Acid Drinks and Sulphonamide Therapy

SIR,—It is pertinent to ask the advocates of freely drinking of the fruit juice beverages by what processes the vegetable acids or their salts, citrates, etc., come to be passed off in urine so as not to increase its acidity. The acid part gets oxidized and the other parts also get oxidized, CO_2 and KHO or H_2O being end-results. The amount of oxygen required to achieve this end is not negligible. The citric acid molecule for its change must have added to it 9 molecules of oxygen; its molecular weight, given as 210.1, requires of oxygen 9×16 , or 144. Where and how is the citrate to get it?

The bodily state in each case must be considered in regard to this important matter. It is of course the doctor's privilege to deal with this. The sulphonamides themselves do make some demands upon the respiratory functions, all of whose details, including of course the R.B.C.'s, may be affected; and in many cases the disease may be one directly affecting respiration. My colleagues in the eye specialty need not be told that prolonged or drastic degradation of oxygen supply and possibly the presentation of un- or imperfectly de-toxicated products is of importance to the retina and the lens. Others should be told.

The drinking of large quantities of water may be rather relished as an uncommon item, and if taken cold or even chilled or iced may, if thought advisable, send more of it to the kidney rather than the skin or lung for elimination. A lump of sugar in it used to be liked and would be welcome as a variant. Lemon and orange juice drinks are taken far too little diluted in many cases, and it might be wise to prescribe the actual dilution considered safe as well as the total daily allowance. In the sick there are limits to the capacity for dealing with excess in H_2O and CO_2 , methaemoglobin, and the disease products and those due to our drugs. The wisdom of the ancients is still apropos: *Est modus in rebus! sunt certi denique fines. Quos ultra citraque nequit consistere rectum.*—I am, etc.,

Glasgow.

ED. J. PRIMROSE.

Nicotinic Acid Tolerance Test

SIR,—I read Drs. P. Ellinger and S. W. Hardwick's excellent paper (May 17, p. 672) and Capt. A. Erdei's letter (June 7, p. 822) with great interest. Both contributions suggest the intimate relationship of the functional state of the liver parenchyma to nicotinic acid metabolism, a problem elucidated mainly through the systematic work of Ellinger and collaborators in recent years. At the beginning of the war I had the opportunity of studying the physiology of the small intestine. I was able to submit evidence for the assumption that the gradient of absorption in the small intestine is to a great extent maintained by the catalysts of the intracellular respiratory enzyme systems. It is known that at least 3 water-soluble vitamins—thiamine, riboflavine, and nicotinic acid—are closely related to these catalysts, and it could be tentatively suggested that a deficiency in these vitamins and a liver "dysfunction" may be instrumental in the production of the sprue syndrome (Leitner, 1942). During subsequent years injections of crude liver extract (containing most of these enzymes) became a routine in the treatment of sprue.

Capt. Erdei's interesting experience that acute sprue symptoms were provoked by giving 1,800 mg. nicotinic acid daily for 5 days, and that the symptoms were quickly cleared up by subsequent massive doses of riboflavin, is a case in point. It also presents an interesting example of an imbalance of the vitamin B equilibrium. During recent years attention has been directed repeatedly to the importance of the interrelationship among the various factors of the B group of vitamins. It could be demonstrated that subclinical latent deficiencies might become manifest after giving large doses of one single crystalline vitamin of the group (Richards, 1945; Leiter, 1945, 1946, 1947). Another interesting feature of Capt. Erdei's case is that sprue could be produced experimentally by disturbing the vitamin B equilibrium—a fact which to my knowledge has not been reported before. For this reason it would be of great interest to have a more detailed description of the symptoms in his case.

Although a considerable number of minor symptoms due to the imbalance of vitamin B factors are frequently encountered, major disturbances appear to be comparatively rare. I should therefore be greatly interested to learn of any such cases.—I am, etc.,

London, W.1

Z. A. LEITNER.

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Folic Acid

SIR.—Dr. J. G. A. McSorley (June 14, p. 863) evidently misunderstood my letter in the *Journal* of May 24 (p. 740). I stated that folic acid, 10 mg. daily, was given plus a maintenance dose of liver weekly. Although intensive liver therapy was given for three weeks previously, the nervous disability was but slightly improved. After a few days of the combined treatment a marked and steady improvement followed. This improvement has been maintained and is progressing. The patient, a joiner, is able to walk a considerable distance without even the aid of a stick and work for several hours at his bench.

The spastic type of subacute combined degeneration of the cord has, I think, always been regarded as the least responsive to any form of treatment. It was the disappointment following liver therapy alone that made me try folic acid in addition, and the result has more than justified the action.—I am, etc.,

Birkenhead.

CECIL L. FORDE.

Hypereosinophil Phase in Allergy

SIR.—In my first communication (June 7, p. 823) it was shown how blood hypereosinophilia proved to be one of the leading signs, pointing to allergy as a possible causative factor in a case of pruritus ani, a condition so far not considered as connected with allergy. Scepticism was expressed against this idea when a repeated blood count showed normal figures of 2% eosinophils. The "allergist" is well aware of quick and transient "organic" changes in allergic conditions. Swellings, catarrhal signs, local and blood hypereosinophilia come and go as quickly as spasms, and are also caused by the parasympathetic irritation which is at the root of allergic signs and symptoms, due to an almost inexhaustible variation of causative factors.

Seen in this light the positive findings appear of diagnostic value, whereas the negative are inconclusive, similar to a negative W.R. in syphilis.

Blood hypereosinophilia occurs in experimental anaphylactic shock as an expression of the parasympathetic irritation, together with the other signs of it. Clinically it is known to occur in allergy, particularly in bronchial asthma, in parasitic infections such as worms, in certain fevers during the "crisis," at the moment before immunity and recovery is setting in—e.g., in Weil's disease (infective spirochaetal jaundice)¹, in scarlet fever,² and in such conditions as Loeffler's syndrome, all now well connected with allergic reaction. Apart from this it is observed in Hodgkin's disease, a condition of unknown origin and pathogenesis. Otherwise response to "foreign protein" is a common factor, and in my opinion the probable cause of hypereosinophilia.

Authors agree that blood hypereosinophilia of 5% or more occurs in 35-55% of the allergic patients. More recently it has become a clinical custom in U.S.A. allergy clinics to do three weekly differential blood counts and to use the average figure obtained as expression of the eosinophil count. I am, however, of the opinion that this is not a suitable method, and that any observation of a "hypereosinophil blood phase" is the characteristic allergic response to parasympathetic irritation which we wish to take into account for our diagnostic considerations in allergic conditions, however transient it may be. Ups and downs in the figures may prove to be of prognostic value or may be due to the effect of therapy, but that has still to be established.

Among 100 L.C.C. hospital patients with bronchial asthma examined during a given period I obtained 0-4% of eosinophils (normal figures) in 44% of the patients, hypereosinophilia of 5% or more in 56% of them. The blood films were examined in the group laboratories. Among 100 private patients with bronchial asthma examined lately and also chosen at random normal eosinophil counts of 0-4% were observed in only 37% and hypereosinophilia of 5% or more in 63% of the patients. The blood films were examined by myself. The suspicion that this was due to a personal bias of the examiner was removed when an analysis of the counts revealed that the difference was due to a higher figure of patients with a hypereosinophilia of over 20% eosinophils. Changing figures in the same patient had been observed frequently before in reports and by myself during the course of observation.

Eosinophils per cent	In 100 L.C.C. Hospital Patients	In 100 Private Patients	Average in 200 Private and Hospital Patients
0-4%	In 44%	In 37%	In 40.5%
5-9%	In 36%	In 37%	In 36.5%
10-20%	In 18%	In 19%	In 18.5%
Over 20%	In 2%	In 7%	In 4.5%

All this suggested that the hospital patients, who had been under treatment before I saw them and had mostly been given adrenaline, did not show the high eosinophil counts so frequently because their parasympathetic irritation had been diminished by treatment and the "hypereosinophil blood phase" had been diminished or had disappeared for that reason. A clinical experiment proved this view to be correct. A boy came under private observation with bronchial asthma. A blood film was taken and 7 min. (0.45 ml.) of adrenaline solution 1:1000 was injected subcutaneously. Ten minutes later another blood film was taken and both were sent to a laboratory. The first blood film before adrenaline showed 13%, the second one after adrenaline 9%, of eosinophils. The asthma attack had subsided, but rhonchi were still present when the second film was taken. In another boy of 14 years with asthma the same experiment showed 9.5% before and 1% of eosinophils after injection of 7 min. of adrenaline, when asthma attack and rhonchi had subsided.

The "hypereosinophil blood phase" seems therefore to be the characteristic reaction to "allergic" irritation and to disappear with it. It is of diagnostic value, and preferable to an average estimate of three weekly repeated eosinophil blood counts.—I am, etc.,

London, W.1.

E. M. FRAENKEL.

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² Fraenkel, E., *Berl. klin. Wschr.*, 1931, **58**, 1,040.

Oxygen Poisoning in Man

SIR.—I feel I must answer certain points raised in Sir Leonard Hill's letter (June 21, p. 900). I would like to make it clear that, so far as I was concerned, the Admiralty provided the experimental facilities. The financial and other arrangements were not my affair, nor were they made known to me. Even if I had been aware of these arrangements I would not have presumed to acknowledge or discuss such matters, but would have left this to the "proper levels." This is an elementary Service procedure which most people will appreciate from personal experience. I am sure that Sir Robert Davis, the head of a highly progressive and successful industrial organiza-