

## THE CARBON FACTOR IN THE GENESIS OF GOUT.

*To the Editors of THE LANCET.*

SIRS,—In THE LANCET of Feb. 11th (p. 348) Dr. D. F. Shearer contributed an article entitled "Carbonic Acid as a Factor in the Genesis of the Gouty State." This article is noteworthy in that it directs attention—for the first time, so far as I know—to a retardation of carbonaceous excretion as the fundamental factor in gout. The writer argues that in the gouty the intake and absorption of carbonaceous material are excessive as regards the amount of physical exercise; that consequently the arterial blood is flooded with carbonic acid; that this excess of carbonic acid is responsible for the retention in the blood of uric acid which is known to occur in gout; and that the salutary influence of physical exercise in the gouty depends upon the increased exhalation of carbonic acid from the blood. A weak point in this hypothesis is that exercise increases, not only the exhalation, but the production, of carbonic acid.

Further than this, the argument rests upon the assumption—widespread but premature in my opinion—that carbonaceous material absorbed from the alimentary canal is of necessity katabolised or anabolised. Normally the blood is relieved of its carbonaceous material mainly by the capacities of the nitrogenous tissues for katabolism and anabolism; but we have no warrant for assuming that these capacities are invariably adequate for the task. Indeed, it seems a larger assumption to postulate an invariable adequacy than an occasional inadequacy.

Now an inadequacy of carbonaceous metabolism, even though slight, would lead to an accumulation of carbonaceous material of some kind in the blood. And if this be granted a large number of observations (experimental and others) could be adduced to show that such an accumulation leads to deficient renal excretion of uric acid. The result would be the progressively increasing uricæmia which is known to antecede the paroxysm of articular gout. Such would eventually involve extravascular deposition of a portion of the uratic load; and should this occur, as commonly happens, in some structure such as a joint which is capable of responding by acute inflammation to the irritation of the urate then we should have a paroxysm of acute articular gout. On this view the mechanism and influence, not less than the causation, of acute gout become clear. Acute arthritis involves pyrexia; pyrexia increases the rate of combustion and so disperses the carbonaceous accumulation; the dispersion of the carbonaceous accumulation releases the uric acid accumulation and permits of free renal elimination; through renal elimination the blood recovers its solvent power and reabsorbs the extravascular deposits of uric acid. Thus is established convalescence not only from the arthritis but also from the condition of depraved general health which so commonly antecedes the arthritis and which is concurrent with the double progressive accumulation referred to. As Mead long ago said, "Gout is the cure of gout."

I believe that a progressive carbonaceous accumulation is a common humoral factor of disease and leads under varying conditions to various paroxysmal affections (amongst them acute articular gout), most of which result in relief from the carbonaceous accumulation.

I am, Sirs, yours faithfully,

Feb. 27th, 1905.

FRANCIS HARE.

## OLD EPSOMIANS AND THE JUBILEE OF EPSOM COLLEGE.

*To the Editors of THE LANCET.*

SIRS,—May I through your columns make known to old Epsomians generally (a very large percentage of whom enter the medical profession) that the Epsomian Club, in conjunction with the present head master, Dr. Hart Smith-Pearse, are arranging to commemorate the jubilee year of the school (1905) by publishing a register of old boys and their various professions, distinctions, &c., together with a brief sketch of the history of the college since 1855 and other items of interest?

Some dozen or so of us have undertaken the task of collecting the necessary statistics and circulars have been sent to all those whose addresses we can find. There are many hundreds of old boys, especially those who joined the

College in its earlier years, whom we cannot trace and therefore I venture to ask, through your columns, any old Epsomians who have not received any intimation and who know the addresses of any old school fellows, especially those who have not entered the medical profession, to be kind enough to send me a postcard so that we may get into communication with them.

I am, Sirs, yours faithfully,

PERCIVAL TURNER.

4, Adam-street, Adelphi, London, W.C.,

March 13th, 1905.

## THE MEDICO-PSYCHOLOGICAL ASSOCIATION OF GREAT BRITAIN AND IRELAND.

*To the Editors of THE LANCET.*

SIRS,—From personal knowledge of the experience and the exceptional ability of the reporter of the Medico-Psychological Association quarterly meeting at York on Feb. 23rd last I authorised him to send a report to your columns. He now writes to me expressing his regret that the substitution of the word "two" for "three" should have occurred as a purely accidental error in transcribing. I desire to be associated with his regret.

I am, Sirs, yours faithfully,

ROBERT JONES,

Honorary General Secretary of the Medico-Psychological Association of Great Britain and Ireland.

March 14th, 1905.

## SHIFTING DULNESS IN PLEURAL EFFUSION.

*To the Editors of THE LANCET.*

SIRS,—Mr. Rickman J. Godlee's paper on Shifting Dulness, which appeared in THE LANCET of Feb. 25th, p. 480, though written primarily from the surgical point of view, invites comment from a physician. Mr. Godlee implies, and in your leading article upon his paper in the issue of March 4th you definitely assert, that "it is generally taught that the dulness due to a pleuritic effusion or to an empyema shifts with position under the influence of gravity." I should not have thought this to be the case but, on the contrary, I should have supposed it to be now generally accepted that the level of dulness in simple pleural effusion, whether serous or purulent, did not readily shift with position. I certainly have taught this for many years and in my book on "Diseases of the Respiratory Organs," p. 659, I write as follows:—

*Alteration of percussion lines with position.*—In cases of hydro-pneumothorax, i.e., where air and fluid are present together in the pleura, it is very easy to make out the change of level, for this takes place quickly as the patient changes his position from the recumbent to the sitting posture; but it is not by any means so easy to demonstrate this change when fluid alone exists in the pleura. A change indeed does take place, but in most cases only slowly. The explanation is to be found in the fact already mentioned—viz., that as fluid forms, although the lung no doubt undergoes general shrinkage from its own elasticity, still the collapse is most marked and may be complete in those portions of the lung which are immediately adjacent to the fluid, while the rest seems to be *pro tanto* dilated. In this way the fluid is to some extent held up, as it were, against the action of gravity, or, at any rate, the lung will not float as readily or change its position as easily as we might expect.

I would add that if the dulness does not shift in front or in the axilla it is still less likely to do so behind, for it is at the base posteriorly that the collapse of the lungs is most marked. If these facts are not yet generally recognised they have, at any rate, been long enough known, as the following quotation from Fagge<sup>1</sup> shows:

A circumstance which has long attracted the attention of clinical physicians is that temporary changes of posture of the patient often fail to alter the position of the liquid within the chest so far as one can test by percussion.

The answers sometimes given by students in examination, if they may be taken as any indication of the views of their teachers, seem to justify the conclusion drawn by Mr. Godlee that the teaching on this point is in some quarters not sound. The fact is of so much importance and ignorance of it may so easily lead to errors in diagnosis that Mr. Godlee has done good service in drawing attention afresh to it.

The upper borders of the liver and the heart as determined

<sup>1</sup> Principles of Medicine, 1889, p. 916.