

0.34, by 5.30 p.m. it is 0.48. Yet all his symptoms have disappeared and he is full of the joy of life—no thirst, no polyuria, no neuritis.

This case is typical. What has struck me most is the quick acquisition of a sense of mental and physical well-being. Of course in all these cases I correlate the dose of insulin and the diet with the blood sugar findings as well as one can with the limited amount of insulin available.

In view of these results one can only conclude that insulin is a practical proposition, and that every diabetic patient who is handicapped in the struggle for existence by the complaint should be urged to spend what money he can afford on insulin, resting assured that it shall be returned to him a hundredfold in strength, happiness, and increased earning capacity.—I am, etc.,

Hornsea, E. Yorks, July 11th.

ALEX. J. W. CALDER.

#### HISTORY OF MILITARY MEDICINE.

SIR,—In the BRITISH MEDICAL JOURNAL of March 10th (p. 419) General Sir W. G. Macpherson prints a long and courteous review of my *Notes on the History of Military Medicine*, in which, while pointing out several inexcusable blunders, he indulges in a little quibbling at my expense. At the end, where his chaffing culminates in "pyramidal pleasantries," my reviewer intimates that I do not even know the military significance of barrage fire or that daylight salvage of the wounded was possible before the barrage was invented. If one considers, however, the sad plight of wounded soldiers left for hours on the field in many great battles of the past (including some of the world war), I think it will be admitted that the barrage promoted efficiency in evacuation of the wounded, apart from its purely military aim.

My statements about the ultimate use of the evacuation hospitals and centres of *triage* referred, of course, to the American lines. In regard to the British casualty clearing stations, the parent of these devices, I stated on page 192 that they were invented in 1907—that is, five years after the conclusion of the South African war. That I date their origin in the period of the war itself seems an unwarranted inference.

My figures concerning Marlborough's march to Blenheim were not the result of mathematical calculation, but were taken on trust from Sir A. A. Gore's *Story of Our Service under the Crown* (1879). I regret that a book so interesting and informing should be misleading on this point. My figures of national battle losses in the world war are, for the most part, obviously rough estimates, but were the only thing of the kind available. They may be appraised in the light of Dr. Johnson's dictum: "Round numbers are always false." Those who expect too much from statistics of wars with heavy battle losses, particularly those in Eastern Europe from the earliest periods, may find consolation in the French proverb, *La plus jolie fille au monde ne peut donner que ce qu'elle a*. How such round numbers were ever evolved from the confusion and disorder following a pitched battle is one of those *problemas para solucionar* of which the Spanish-speaking physicians incessantly write. This is true of some of the statistics of the Napoleonic, Crimean, and Austro-Prussian wars. My failure to account for the work of the Austrian medical department in the pre-Napoleonic period is a serious error of omission, but that Brambilla and Sax are more important than Larrey and Letterman in the history of "collection, evacuation, and distribution of the wounded" will be news to American medical officers. I saw no mention of this view in the English, German, and French sources available (including Longmore). As for the assertion that the Brambilla-Sax system was subsequently adopted by the Continental and British armies, I am at this moment 10,000 miles away from any reference books, and beg that this momentous claim to perhaps the most important advance in modern military medicine be confirmed by textual verification.

The statement that my Notes were "largely culled from the writings of Frölich" will not, I think, be confirmed by anyone who is familiar with these writings or who has examined the pamphlet and its footnotes in relation thereto. Frölich's derivation of the Homeric *ἰηρός* from the concept "arrow" rather than from the verb "to heal," has, how-

ever, some justification, in that the Homeric "healer" was not a medicine-man but a wound-healer, as in the old High German epics in which surgery is *Heilkunst*. In this view, the verb *ἰάομαι* would connote wound-healing, and might have the same root-concept as *ἰός*.

In conclusion, I beg to say that my pamphlet was intended, not for the "instruction" of medical officers proper, but as a stimulus to further study among student officers at the Army Medical School, Washington, D.C., and that its title "Notes" conveys this fairly modest intention. I am glad to have perpetrated a few blunders in 206 pages, in view of their eventual correction by military authority so high.—I am, etc.,

F. H. GARRISON,

Manila, P.I., May 17th.

Lieut.-Colonel, Medical Corps, U.S.A.

SIR,—You have kindly given me an opportunity of replying to Lieut.-Colonel Garrison's letter in which he criticizes my review, in the BRITISH MEDICAL JOURNAL of March 10th, 1923, on his *Notes on the History of Military Medicine*.

I will be as brief as possible. But first of all I must disclaim any attempt at quibbling or pleasantries in that review, and, moreover, I venture to think that no unbiased reader of it could discover any indication of such in it. I certainly fail to do so myself, and, in fact, nothing could be further from my wishes or intention than to indulge in any frivolity at the expense of so distinguished a colleague as Colonel Garrison, for whom I have the greatest respect and admiration, and from whom I have learnt so much. It was a reviewer's duty, however, to point out the errors which Colonel Garrison himself handsomely acknowledges to have occurred in his *Notes*, more especially as historical errors are apt to be perpetuated by repetition.

As regards barrage fire, I can state definitely, from personal observations during almost all the battles on the British Western Front, that wounded were cleared as rapidly before as after the introduction of barrage methods of attack; barrage, in fact, had absolutely no influence one way or the other on the work of clearing the wounded.

Colonel Garrison has entirely misread my remark about the casualty clearing stations. I did not say that he dated their origin to the South African war. What I did say was that he is wrong in implying their origin to the faulty organization of the field medical units during that war instead of to their faulty organization after the war—that is to say, to the faulty organization of the field ambulances during the reorganization of the bearer companies and field hospitals which existed at the time of the war.

He has cleared up the statement about Marlborough's march to Blenheim, but in doing so he offers us an extremely interesting example of how historical truth becomes perverted. He now tells us that the authority for his statement "Marlborough's celebrated march to the battlefield of Blenheim covering 1,176 miles in 86 days" was Sir A. A. Gore's *Story of Our Service under the Crown*. This book was a reprint from an article by Surgeon-Major A. A. Gore in *Colburn's United Service Magazine*, and this is the statement in it:

"Marlborough's campaigns are also to be remembered for two marches of historical interest. His celebrated march commencing 19th May, 1704, and ending in the crowning victory of Blenheim on 13th August; and the march of the Prince of Hesse, rarely equalled for rapidity and execution. The first was commenced on the breaking up of winter quarters, the troops returning in the autumn during which they covered 1,176 miles (Miller)."

Now Gore bases his statement on Millner. Who, then, was Millner? He was a sergeant in the Royal Regiment of Foot of Ireland—in other words, the Royal Irish Regiment—who kept a journal and published it in 1733, and the following extract tells us of the origin of Gore's statement as modified in Colonel Garrison's notes:

"The tedious but ever glorious, memorable and victorious campaign of 1704 was in length 30 weeks and one day; commenced the 24th day of April and ended on the 20th November; of which our Corps with the Grand Army, and apart, to, in and back from Germany march'd and sail'd ninety-one days and therein three hundred and ninety-two leagues or eleven hundred and seventy-six miles English."

This is a very different affair, and Marlborough's march to Blenheim, it will be seen, forms only a part of the 1,176 miles of marching and sailing of the Royal Irish Regiment in 1704. I confess that I was very much puzzled to account for Colonel Garrison's statement, but now that it has been cleared up my mathematical calculation to explain the puzzle has, of course, no longer any significance.

The influence of Brambilla and Sax will be found recorded in the voluminous works of Myrdacz and other Austrian writers.

"Sax was the moving spirit in all matters connected with the Medical Service in the War of Liberation. The Austrian organization for dealing with masses of wounded and evacuating them to fixed hospitals was indeed studied and imitated in the armies of Prussia and Russia, which formed the Allied Army against Napoleon."—(*Medical Services of Foreign Armies*, Part III, Austria-Hungary, 1910, p. 18.)

This, I take it, was the origin of the extension of the Austrian system to Continental armies; but, however that may be, I can only say that the British medical organization, which was first introduced into Field Service Regulations and the Manual of Training for the R.A.M.C. by myself, was learnt in the Austrian school, and I also know that my revered master, Sir Thomas Longmore, was influenced just as much as I was by its teaching and historical importance.

My conjecture that Colonel Garrison's *Notes* were largely culled from Frölich should be taken with the context, in which I fully recognized how deeply he had explored the literature of his subject in other directions. His derivation of the Homeric *ἡτρός* is, of course, debatable but not convincing.—I am, etc.,

London, S.W., July 12th.

W. G. MACPHERSON.

#### ETHER VERSUS CHLOROFORM.

SIR,—To one who has been engaged in the administration of anaesthetics since the beginning of the century, this perennial controversy is a source of some wonder. One had thought the question settled long ago. But the old cry is revived that chloroform is quite safe if only the revivalist's technique is adopted; that deaths are not due to it, but to shock; that, from the operator's point of view, ether is an inefficient anaesthetic and equally dangerous owing to its remote effects.

Speaking generally, and I trust without offence, the experience of the advocates of these views is usually more peculiar than extensive. Dr. Samways speaks of hundreds of administrations to soldiers. Well, of these there were in the main two types—the shock-stricken or sepsis-ridden casualty near the front, and the healthy young man full of 'baccy and beer needing attention to a hammer-toe or tonsils at a remote base hospital. I venture to think that Dr. Samways's very satisfactory experience was not amongst the former. With the latter even an expert accustomed to getting every ounce out of ether will readily admit that in many cases the path of least resistance literally lies with chloroform. They required plenty of it, and it was indeed difficult to overdose them, and whatever anaesthetic was administered quick and good recoveries were the rule. In civil practice the vast majority of cases form a happy mean between these two extremes.

That chloroform is, on the table, more dangerous than ether has been proved by statistics; is, I believe, the considered view of all the most experienced anaesthetists; and is emphasized almost weekly in the press. Persuade yourself that many of these deaths are due to shock if you like, but you are still up against this—that chloroform is a depressant, promoting shock, ether a stimulant protecting from it. Moreover, should any contretemps arise during administration, if chloroform is the agent, grave, even if unnecessary, anxiety pervades the atmosphere; whereas if ether be the agent sublime confidence inspires all. Of three deaths I have seen before return of consciousness after administering ether, two were forlorn hopes before operation, and the third died of haemorrhage. All were kept alive by ether during severe and lengthy procedures and then slowly succumbed. Contrast this with two appallingly

sudden catastrophes under chloroform—one during induction and one in quite an early stage of operation.

That as profound a degree of narcosis can be produced with semi-open ether has been proved by Rood,<sup>1</sup> and his "enclosed" method so ably advocated by Tilley I have found of the greatest possible value in such surgical procedures as demand it. In the vast majority of operations the light mixed narcosis produced by the minimum of open ether following full doses of alkaloids serves our purpose. When this method is carefully carried out after-vomiting is rare and bronchitis unknown, assuming, of course, infectious catarrhs are not epidemic and the patient has not been unduly exposed to chills. Finally, there is practically no risk of acidosis.—I am, etc.,

London, W., July 14th.

G. A. H. BARTON.

SIR,—I should like to endorse all Mr. Tilley says in his admirable letter (June 30th, p. 1114), and it is strange indeed that chloroform is used when ether is so much safer.

It may be of interest to your readers to know that I looked up all the recorded deaths under anaesthetics in the United Kingdom in the *British Medical Journal* and the *Lancet* for three years—those preceding 1890—mentioning these in a letter to the *Lancet* (vol. ii, 1890, p. 44), there being 41 from chloroform and only 3 from ether, and 1 only from nitrous oxide.

I spoke very strongly in favour of ether in this letter, and from my own personal experience—it having been my duty to administer the anaesthetic for nearly ten years as resident medical officer in all major operations in three hospitals—I am happy to be able to state that I have had no death under any anaesthetic, either in hospital or private practice, before or since.

Ether has been chiefly my choice (or its mixtures) with Clover's inhaler, except in later years, when open ether has been used sometimes.

I have not seen any cases of "ether" bronchitis or pneumonia, and certainly no fatalities from this cause.

I will not repeat what I stated in the letter above quoted, except that ether always gives warning of its danger, whereas chloroform does not always, fatal cases occurring sometimes during induction—I am, etc.,

London, W.C., July 10th.

H. C. NANCE, F.R.C.S. Eng.

#### TREATMENT OF FEVER.

SIR,—I have made trial in twenty-seven cases of influenza, eight typical cases of lobar pneumonia, and in several other acute infections, of the treatment recommended by Sir Archdall Reid for acute toxæmia (*BRITISH MEDICAL JOURNAL*, June 4th, 1921, p. 835), and I have obtained such excellent results that I would be glad to inform the medical profession of them. I will mention only three cases of lobar pneumonia which represent three different classes under different circumstances.

*Case 1.*—A priest, aged 57, had a severe chill on the afternoon of June 24th, 1921, followed by fever (39° C.); he passed a restless night and called me the following morning. I found him with a fever of 39.50° C., he complained of a stitch on the right side of his chest and had an incessant cough. Physical examination revealed the presence of all the symptoms of lobar pneumonia. I at once prescribed three powders, recommended by Sir Archdall Reid, and ordered him to take one at once and another in the evening. He was living in a village far from where I was, and on June 26th his son came and reported that his father had no fever, the cough was softer but contained traces of blood, otherwise he felt in perfect health and wanted to get up. He got up the next day. On June 28th he came himself to see me in excellent health, which he still enjoys.

*Case 2.*—A girl, aged 13, had a cold for about a week. On January 17th, 1922, she returned from the school with a fever of about 40° C. and cough and stitch on both sides; as they were not in a position to call a physician at once, they waited two days, when they noticed the bloody sputum, which caused them to send to me in the evening of January 19th. I found all the symptoms of double pneumonia, and after some hesitation ordered the same powders, with the result that on January 21st she had no fever and did not have any discomfort except the blood-stained sputum, which continued for over ten days after the fall of temperature.

*Case 3.*—A cabman, aged 35, robust and sanguine, had a history of two attacks of lobar pneumonia in the course of the last three years. In the first attack he was ill for over twenty-five days and

<sup>1</sup> *Transactions Royal Society of Medicine.*