

ON THE ARREST OF TYPHUS FEVER BY CINCHONISM.

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NOTWITHSTANDING the evidence adduced in support of my views on fever, and my proposition to arrest the course of typhus by Cinchonism, I regret to find that these views should still be so partially adopted by the profession: an evident example of the difficulties which beset all doctrines opposed to the bias of early education, and to the time-honoured theories of the schools; against which the most positive and direct evidence, on the present question, has been nearly disregarded by the profession, though referring to confessedly the most numerous and fatal section of human diseases.

For example, the power of bark to arrest the course of intermittent fever has long been an universal fact, admitted by all; and its power over continued fever has been almost equally insisted on, I might almost say proved, by distinguished and trustworthy practitioners, from Morton and Sydenham down to the present day. Within our own time, that is, within the last twenty or thirty years, the influence of large doses of quinine in controlling the remittent and congestive fevers of the West Indies, Africa, and America, has been repeatedly pointed out by different American, French, and English army physicians; and two or three years ago, at the suggestion of my relative Dr. Leslie, the effect of cinchonising doses of quinine, in continued fever, was tried in Ireland, with success in many cases, though with doubtful results, I believe, in some.

Neither ought the assumption of the essential identity of typhus fever and ague to be deemed so irrational as some able men have lately insisted. Did not Sydenham himself, and Lind and Fordyce, the elder Stokes, and numerous other distinguished physicians, admit that ague may lapse into typhus, especially when injudiciously treated, *id est*, by depressants? and do we not constantly witness this conversion in southern climates? Tommasini, and others, pretty clearly show that yellow fever is but a more severe variety of typhus. And Dr. Graves has long since pointed out to the profession, that the fevers of warm countries and those of Ireland, "differ in degree but not in pathology". Why, then, should it be deemed unreasonable to expect that the remedy which so effectually controls the fevers of the former countries, should prove equally efficacious in those of the latter?

Dr. Graves, with his usual acumen and sound judgment, has also pointed to the folly of adopting twenty-four hours as the unit of our computations in calculating the intervals of fever; and nothing can be more irrational. Authors note the quotidiana *duplex*: practitioners treat the quotidiana *triplex*; and what are these but continued fevers? Dr. Shewbridge Conner observes, "some cases of typhus had pretty regular hot and sweating stages, several in a day; perhaps the cold fit escaped observation." (p. 518.) All practitioners know that the cold stage in ague is often wanting. Again, have we not a nine days' ague, and a seven days' ague, as well as a quartan, tertian, and quo-

tidian? And, as regards the regular succession of the cold, hot, and sweating stages, every practitioner, how slightly soever familiar with intermittent fever, will admit that one or other of these stages is frequently absent, and sometimes, even, that the succession is reversed. I would finally observe, that in every case of genuine typhus, when undisturbed by treatment and free from local lesions, I have invariably found more or less distinctly marked intermissions or remissions, with longer or shorter intervals. Such also are the cases in which we so constantly observe the fever to terminate by what is termed well-marked crisis.

Guided by the foregoing facts, by the observations of others, and by my own experience, and especially supported by my profound conviction of the essential identity of tropical and European fever, I pursued my investigations on the present question, until they finally assumed the form in which I submitted them for judgment to the profession. In a late notice of my work in the *Medical Times*, additional evidence is, however, demanded, and this I feel in some measure bound to supply, as clearly and concisely as possible, in the following recent cases: one from the Northern Hospital, the others by able and experienced practitioners of Liverpool and the neighbourhood. The patients were all under different circumstances, in different localities, and in different ranks of life.

EXTRACT OF A LETTER FROM T. B. GILDERSLEEVES, Esq., Union Medical Officer of Liverpool.

“51, Great George Street, 23rd March, 1852.

“ Hugh Evans resisted the effects of quinine for some days; he took altogether sixteen doses of ten grains, and the only effect for some time was increased drowsiness. It was not until the last day, when I ventured on six consecutive doses of ten grains, that a favourable change occurred.”

CASES I AND II, by Mr. Gildersleeves. “Caroline Moore, aged 35, and Barbara Moore, aged 14, mother and daughter, living in a wretched court, No. 4, Jamaica Street, were both attacked with fever, and lying in the same bed. The mother had been ill seven, the daughter five days. Symptoms of typhus were well marked; the tongue was coated with brown fur and quite dry; sordes were present on the teeth; the skin was hot, dry, and covered with petechial spots; the pulse upwards of 100; the eyes were suffused; there was prostration; in the mother, some delirium. They had taken salts and senna of their own accord before I visited them. They refused to go to the Fever Hospital. I ordered for the mother ten grains, and for the daughter five grains of quinine every three hours.

“After the second dose, the mother became quite deaf; the daughter, after the third dose, complained of tingling in her ears. The medicine was discontinued with both, and sleep, for some hours, followed.

“On the *following morning*, both were *quite convalescent*. The countenance was cheerful; the tongue moist, and the brown fur had disappeared. The petechial spots remained for more than a week, and great debility, but there was no relapse.”

CASE III. “Hugh Evans, No. 3, Crump Street, had been ill two days. I prescribed a dose of calomel and antimony, under the supposition that the disease was simple febricula. Typhoid symptoms

supervened on the fourth day. The tongue was brown, with *sordes* on the teeth; pulse 110. He had severe headache, and low muttering delirium. I ordered three ten-grain doses of quinine, one to be taken every three hours, and six ounces of wine, to be taken at intervals; a blister to the nape of the neck.

“*Fifth day.* There was no improvement; the quinine and wine were continued as yesterday.

“*Sixth day.* There was slight improvement; less stupor. The quinine and wine were continued.

“*Seventh day.* The patient was in all respects worse; he was covered with petechial spots; greatly prostrated; and had subsultus and twitching of the limbs. I ordered six ten-grain doses of quinine, one to be taken every three hours; his wine to be continued, and a quart of ale to be given.

“*Eighth day.* There was a most remarkable change. The tongue was moist and clean; the brown fur gone. He was perfectly sensible; complained of great debility, but as regarded fever, was quite convalescent. He had no relapse.

T. B. GILDERSLEEVES.”

I had here intended to refer to cases observed by Mr. Bainbridge, Mr. Jones, Mr. Blackburn, Mr. Hensman, Mr. Pye, and Mr. Arnold, of Liverpool; Mr. Gorst, of Rock Ferry; Mr. Swinden, of Wavertree; Mr. Glazebrook, of West Derby; Mr. Longton, of Southport, etc., etc.; cases highly important from the character and talent of the observers. The case from the Northern Hospital was also very striking. Space, however, compels me to postpone them. Moreover, the introduction of further evidence on this point might, perhaps, be deemed an act of supererogation by the profession, after perusing the subjoined extract of a letter just received from Dr. Graves, of Dublin.

“Merrion Square, 22nd March, 1852.

“ At my desire, your quinine treatment of typhus has been tried in one of our hospitals, and with success. When I receive the notes of the cases, I shall not fail to let you know.”

The following letter, subsequently received, from Dr. Richard Kelly, physician to the Drogheda Fever Hospital—the gentleman selected by Professor Graves to test the value of “cinchonism in typhus fever”,—needs no comment. These names will thoroughly fix the attention of the profession.

“Drogheda, 3rd April, 1852.

“With regard to your original treatment of typhus fever, I must acknowledge myself a convert, as I have treated eight cases of the most severe description with the happiest results.

“I shall, however, enter into details of one of the most severe cases under my care.

“A poor farmer, named Pentony, aged 55, was admitted into hospital 16th February, had been ten days previously ill. The pulse was 120; the tongue dry and brown; he had constant muttering delirium. The respirations were forty; the skin covered with maculæ; the temperature 90°. He had involuntary discharges, and subsultus tendinum.

“In *two days* after the administration of quinine (according to your directions) he was *convalescent*, and left the hospital in excellent health ten days after.

"In my opinion, such a happy result could not have been procured by any other treatment that I am aware of.

"In three of the fore-mentioned cases, four members of the families died in the houses from which my patients were removed; and in one case, the patient had been afflicted with chronic bronchitis for years, but it did not militate against the treatment.

"Trusting that such an invaluable improvement may be adopted by the members of the profession, and that my humble testimony may be of service in the trial of it. I remain, etc.,

R. KELLY."

"To Dr. Dundas, Liverpool."

In the conviction that my late work, *Sketches of Brazil*, had placed the question of cinchonism in typhus fairly before the profession, I had determined there to leave it, satisfied that the universal experience of the profession must soon accomplish the rest. The following late occurrence has modified this expectation.

Within the last few days, a professional friend, by my request, brought the question of "cinchonism in typhus" before one of the most eminent physicians in the metropolis, a name second to none, distinguished as an author, and one of the most esteemed teachers of medicine in London, as well as physician to one of the largest London hospitals. Yet this gentleman (justly occupying the very highest rank in his profession) had not only not tried the system himself, but, on mere theoretical reasoning, he condemned it altogether as visionary and dangerous. "He did not like fever-curers."

Now such, I humbly submit, is not exactly the spirit in which new and important views should be entertained by leading members, and, more especially, by the teachers of our profession, even though such views should be supported by evidence less direct and positive than that which I was enabled to adduce in proof of mine. Surely, the testimony of a physician to St. Thomas's Hospital, of the physicians to the Liverpool Fever Hospital, the Birkenhead Fever Hospital, the West Derby Fever Hospital, the Liverpool Northern Hospital, the Drogheda Fever Hospital, with that of numerous private practitioners of eminence; surely, I repeat, such testimony ought to have obtained, for any system, a fair and impartial trial, putting altogether aside my own experience, on which I have, purposely, little insisted.

This question, however, having been now taken up, in the true spirit of investigation, by such men as Professor Graves of Dublin, Christison and Bennett of Edinburgh, Todd, Budd, etc. of London, the true value of "cinchonism in typhus fever" must, ere long, be established beyond the reach of a doubt or a sneer. But I would also earnestly entreat that the different practitioners throughout the kingdom should make known to the profession the results of their experience in their different localities, and in the different classes of society.

I would here beg to recal, briefly, a few of those principles on which I have elsewhere strongly insisted, namely, that the value of cinchonism in typhus will be in proportion to its early induction; that, adopted early, it arrests with certainty, in the vast majority of cases, the course of all continued fevers,¹ and thus prevents the complica-

¹ Dr. Billing in his valuable work, *Principles of Medicine*, lays down (p. 103) that "quinine has not much influence on any continued fever." Has Dr. Billing ever tried it?

tions which prolong the disease, and peril life; that we cannot arrest all cases of typhus fever by cinchonism, nor can we all cases of ague: serious visceral disease, in either case, will interrupt the specific power of the remedy; also, idiosyncrasy in some, and a broken-down state of the constitution in others, will prevent its success; that a vital organ already seriously damaged, or the vital fluids already seriously vitiated, will necessarily render the success of cinchonism doubtful; but that in none of the foregoing conditions, idiosyncrasy excepted, should the remedy be altogether suspended; for even in these, its administration will prove commonly useful, and *always* safe; that, after the first impression has been made on the disease by cinchonism, the patient should be constantly, and *well supported*; no slops. Wine will be often necessary, and, especially with hospital patients, brandy. To the purely medical measures I need not refer; but there is one other point on which I am anxious to fix the attention of the profession, namely, that in estimating the specific power of cinchonism over typhus fever, the practitioner must carefully distinguish those cases of visceral disease, attended with low inflammation and typhoid symptoms, which are constantly admitted into hospitals as "typhus" or "typhoid" fevers. In these cases, the failure of cinchonism attaches, not to the remedy, but to the physician.

I may, perhaps, be here permitted to offer a few words on a late notice of my work in the *Medical Times and Gazette*. My reviewer states, that he had observed certain shades of colour, but *never eruptions*, in tropical fevers. Surely there must be some mistake here: never witnessed eruptions in the severe and fatal forms of the congestive remittent and continued fevers of warm climates! The identical shade of colour may not, certainly, be always found in a London garret with the mercury at zero, as at the equator with the thermometer at 90°: who expects it? But eruptions most undoubtedly there are, and in great variety too. Amongst others, what practitioner has not witnessed, in warm climates, the mulberry-coloured spots passing into the petechial? The climate of Charlestown is thoroughly tropical. Must Dr. Wragg's late account of the Charlestown fever, then, be deemed a myth? My reviewer must, of course, have observed the fevers of hot climates, because he tells us so; but this I boldly aver—His has been a limited experience; for I will not insult *his* "diagnostic skill" by comparison with that of "old women" of the London Fever Hospital—a standard which he, however, has not hesitated to apply (under the safe security of an anonyme) to the most distinguished physicians of Dublin and Edinburgh, France and Germany; though he has not indeed favoured us with the precise evidence on which he has arrived at conclusions so deeply humiliating to the "diagnostic tact" of his brethren. One might here naturally exclaim, if such be the skill of the humble servitors of the above institution, what may we not look for from the "Dii Majores" of the temple!

As regards such humble observers as myself, it is somewhat consolatory to know, that more competent authorities have found equally "inappreciable" those distinctions which I have declared, and must still declare, to be practically visionary.

Amongst others, the *British and Foreign Medico-Chirurgical Review* states (No. XV., p. 27), "Typhus and typhoid fevers have been con-

stantly before our eyes, and we have failed to find out their differences, striking as they appear to be in the pages of Jenner and Stewart." Whilst the *Dublin Quarterly Journal of Medical Science* (No. XXIV., pp. 440-447) observes, in reference to the same subject, "We believe that these two forms of disease are . . . blood relations, and that they cannot be separated. . . . they are one."

Now, at the risk of being deemed ungallant to the ladies, truth compels me to avow that, in this part of the world, we are disposed to admit the above evidence, malgré the "diagnostic tact" of all the "old women," or young, of the London Fever Hospital.

As my reviewer has not stated my theory of intermittent fever, nor the evidence on which it is based, "I must content myself with a protest against his opinions."

I would here beg to express to Dr. Jenner my regret that, in my brief allusion to his valuable cases, I should have placed the words "typhoid fever" between inverted commas. I must at the same time confess that I am still unable to assign case 35 to a category different from that of its immediate predecessor.¹

Liverpool, April 1852.

BIBLIOGRAPHICAL RECORD.

1. ON THE DISEASES OF THE BLADDER AND PROSTATE GLAND. By WILLIAM COULSON, Esq., Surgeon to St. Mary's Hospital, &c.
2. A PRACTICAL TREATISE ON THE DISEASES AND INJURIES OF THE URINARY BLADDER, THE PROSTATE GLAND, AND THE URETHRA. By S. D. GROSS, M.D., Professor of Surgery in the University of Louisville, etc.
3. THE ANATOMY AND DISEASES OF THE PROSTATE GLAND. By JOHN ADAMS, Esq., F.R.C.S., Surgeon to the London Hospital, etc.
4. THE PATHOLOGY AND TREATMENT OF STRICTURE OF THE URETHRA. By JOHN HARRISON, Esq., F.R.C.S., formerly Lecturer on Surgical Anatomy.

It has been a subject of complaint, that while every other organ of the body has been the subject of separate treatises and monographs, no full and comprehensive account has appeared of the DISEASES AND INJURIES OF THE URINARY ORGANS. The works, whose titles are above quoted, will effectually remove this charge against the literature of our profession. It must be admitted, that up to the present time the student has been at a loss where to seek for a full and comprehensive account of the various diseases and lesions of the urinary apparatus. The work of Sir B. Brodie, having the advantage of containing nothing which he had not himself observed, is on this very account far from being a complete treatise; and although it holds a de-

¹ Discussing yesterday the question of cinchonism in typhus with one of the most eminent local physicians attached to our largest hospital, I altogether failed to convince him of the *safety*, even, of the treatment. I urged the fact of its *universal* adoption at our Fever Hospital here; and, what I thought would prove conclusive, its perfect success at one of the Irish Fever Hospitals, where it had been tested at the suggestion of Dr. Graves. On this evidence, I begged that he would give the system even a single trial: "*I dare not try it*", was his sole, and evidently conscientious, response. R. D.