

Both (61)

# QUESTIONS

ASKED IN "THE BOSTON MEDICAL AND SURGICAL JOURNAL,"  
UNDER THE HEADING, "IRREGULAR AND QUACKISH  
ADVERTISEMENTS,"

# ANSWERED

BY

*DR. CARL BOTH.*

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To contend against stupidity, the gods themselves would fail. — SCHILLER.

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BOSTON:  
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## R E V I E W.

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In the "Boston Medical and Surgical Journal" of Nov. 17, 1864, I am accused of irregular and quackish advertisements, of dispensing secret nostrums, of proclaiming alleged superior claims to the confidence of the community, of injuring the honor of the "Medical Society," &c. On these accusations, the editors of that paper, and a nameless "friend," propose to expel me summarily from an association called the "Suffolk Medical District Society."

When I entered this society six years ago, I had the impression that it was not the place for me; and, ever since, I have been rather an evil spirit and troublesome member of it. Had I been a member, at the time, of the Roman clergy, I surely would have received the bull of excommunication, if not burnt as Michael Servetus, or sawed through as Gabriel de Zerbis. I am one of those incorrigible, restless subjects, who not only find fault with many things, but who undertake to correct them. I have even gone so far as to produce new ideas, and dare to stand up for them against all "regular" custom and authorities; and, what is worse, I am not afraid to say, and even print, the truth. Must I not become, therefore, "regularly" and summarily expelled from a "regular" society? This has been always customary: why should it not be so now?

We shall see who these men are who have nothing to say to my face, but afterwards assert that my pamphlet "*is utterly unworthy of the notice of a scientific physician.*" They are, first, the two editors of the "Boston Medical and Surgical Journal." I could easily make a few remarks which would ridicule and upset these men at once, and make them the joke of Boston. But I shall not do so; for it is not my object to appear witty, nor will I degrade myself by being personal. I will only criticise their Journal *a little*; and, as I have not another of their products, I will take only the Journal of Nov. 17, 1864, in which I am accused of making nostrums and secret medicines. There is, first, "An Introductory Lecture delivered at the Massachusetts Medical College at the

opening of the winter course of instruction in Harvard University, by Edward H. Clarke, M. P., professor of materia medica." This lecture, intended for young students, is a very good one; but I thought that the medical paper was printed for *scientific* physicians only. It is very strange, that, for scientific physicians, a lecture for young students is printed. The editors of that paper should be aware that scientific physicians must already know every thing which that lecture contains; that this lecture cannot be headed "Recent Progress of Materia Medica," but that this progress began already with Hufeland. But the editors show by this that they are not acquainted with foreign literature, and therefore think this something new, which really is something old. Dr. Clarke, when he sent in his lecture for printing, knew very well that the editors and readers of the Journal were not quite so "scientific" but that this lecture would be something new for them, and do them good. Speaking of Dr. Murray, Dr. Clarke says he does not give a hint of mental influence, or diet or exercise or occupation, as agents or forces entitled to be called remedies. He further says, — mentioning only foreign authors, none of the Boston "Improvement" Society, — "These observers have demonstrated that the most rational and effectual method of treating certain forms of disease is to prescribe an appropriate diet, oleaginous, farinaceous, saccharine, or otherwise, and thus have brought food within the strictest definitions of the materia medica. Their observations show, moreover, what I fear is often neglected, or forgotten or *disbelieved*; namely, that as much knowledge, experience, and skill, are requisite to prescribe and regulate an appropriate diet, as to administer quinine in a quotidian, or opium in colic, or iron in anæmia." Perhaps, if the editors of the Journal had read this before, they might not have written, "a paper utterly unworthy of the notice of a scientific physician"! I have never called my paper a scientific paper, but only a sketch; but I must freely say, it was, nevertheless, too scientific for the editors, because *they never understood it!* After they have printed and read a few more of Dr. Clarke's lectures, perhaps, if they read my sketch again, they will better understand it. Dr. Clarke says, further, "Give me the methods of a science, says Cousin, and I will give you its results. If its methods are true, the

results will be true ; if its methods are false, its results will be false." If the editors of the Journal, or any readers of it, are able to produce a better and more correct method of treating disease than I have given, in short, in my sketch, I will freely pronounce them more scientific and better physicians than I am. There is the best chance to overthrow me, and to show how much more scientific men they are than myself. So much about the lecture of Dr. Clarke. May he live long, and write many such lectures ; for nothing is more needed in Boston. The proof of these words we shall give in the following "Extracts from the Records of the Boston Society for Medical Improvement, by Francis Minot, M.D., Secretary." There is no society in the world which bears such a high-heeled and arrogant title ; and there is likewise no society which has produced so much less than nothing towards any improvement in medical science.

But to their report. The first case is suspension of labor-pains after dilatation of the os uteri. Dr. Storer reports a case in which he found labor-pains for two or three days already, and the os dilated to the size of a dollar ; and he thought the lady would be delivered in a few hours. The pain ceased, however, and it was decided to remove the patient to Boston (twelve or fourteen miles' distance) in a carriage. During the drive she had some severe pains, so that he feared she might be delivered in the carriage ; but she reached town safely, and the pains again stopped. There having been no advance in two days, Dr. Storer ruptured the membranes, and twins were born. Dr. Storer had met with a similar case six or seven years ago : the os was dilated, the membranes protruding ; and he thought delivery would take place in a few hours ; but the patient was not confined for ten days. Prof. Siebold and Prof. Scanzoni, my teachers, would have acted differently. Their rules are to break the membranes immediately after the os is dilated so much ; for they know that pains must cease when the membrane (as it was in this case) is too thick to break by itself. They would not have exposed a lady under such circumstances to a carriage-ride of fourteen miles, because it was supposed, that, through the shaking occasioned by riding, the membranes would break, and the poor lady be delivered in a carriage, where she might very likely have found her sure death

in consequence of a bleeding or some other beforehand unknown accident. They would under no circumstances allow a poor woman to suffer for nothing five and even ten days. What do the editors of the medical paper say to that? What do the physicians of the whole world call that?

Then comes the report of a live birth at the beginning of the sixth month, by Dr. Minot. A respectable married woman, who had no children previously, had an abortus, and the child weighed one pound and three-quarters, and lived forty-eight hours.

Then a case of necrosis of the astragalus, by Dr. Cabot. A boy had the right ankle and foot swollen and painful. A history of the case could not be obtained, except that the disease began four weeks previously. Fetid pus from an opening below the external malleolus. Traces of scrofulous abscesses were seen below the ramous of the jaw. The boy's foot was poulticed for ten days, then operated upon. The report says, "On making an incision, several large pieces of the astragalus, which were perfectly loose, were removed with the forceps. They comprised the entire astragalus, denuded of its periosteum." The boy got well in a considerable short time; the motion of the foot was good, &c. Operation, treatment, and cure were not alone very good, but uncommonly effectual. Cases of extraction of the astragalus with a healing process of such a short time are seldom, and of great interest to any physician. At first, it seems strange that this case was not headed "Caries of the Astragalus:" for, from the report, it does not appear as a case of necrosis, because the pieces were perfectly loose, and without any periosteum or capsula sequestralis; and necrosis is very rare in spongy bones. However, by close examination, it seems also not to have been a case of caries; for a process of caries does not generally divide a whole bone into pieces, at least not large pieces. It began only four weeks previously. I should think that this boy's foot must have been struck; and, in consequence, the astragalus became broken, or the boy must have fallen, or broken the bone in some way. The understanding of such points is a most necessary thing in criminal cases; and the more interesting, the more difficult the questions might be. In any other medical society in the world, I believe this question would have been brought up.



I believe the society must have been asleep while Dr. Cabot reported the case: but he showed the casts of the foot; they could not have been asleep. Perhaps they did not know that this case was a rare and extraordinary one.

The next case is "Diseased Bladder, by Dr. Hodges." A very interesting case, a very interesting specimen. No etiology of the case, no diagnosis, no prognosis, of course, no treatment. The patient died. Post-mortem examination very interesting, but again no explanation; no consequences from the case, no questions asked, nothing.

But the last case shows how much the lectures of Therapeutics are needed,—"Typhlo Enteritis, by Dr. Fiefield." "A young gentleman had been ill with typhoid fever for six weeks, and was not recovering satisfactorily: nineteen years old; exceedingly emaciated; with a bright eye; some flushing countenance; skin slightly discolored in spots about the knees and ankles, with echymosis. He had some sores upon the lower limbs, one of which had yielded some pus; pulse eighty; abdomen moderately full, and not tender at the present moment, but had appeared to be so to the attending physician some time previously. The bowels were and had been somewhat loose; three or four evacuations daily; sometimes restrained by opiates; appetite ravenous; tongue clean. Ordered a pill composed of acet. plumb. gr. ii and opii. gr. i three times a day until diarrhoea ceased; the bowels then to be allowed to move once in two or three days. Quinine gr. ii with dilute sulphuric acid, to be given three times a day before meals. Diet, beef-tea frequently, in small quantities; boiled rice or blanc-mange as patient wished; sherry-wine, a teaspoonful once in two hours during the day, occasionally through the night. Prognosis favorable." (This was September 28.) Saturday, October 1, a note was received that every thing had been done as prescribed; and the report further says, "Soon he began to suffer pain in the bowels; a little difficulty in urinating; fomentations applied, and began to give him something to move the bowels; but without success. Last night, had a shaking turn; pulse 80. I visited him in the afternoon: found him in great pain; abdomen universally sensitive; but, upon pressure over the right iliac region, he complained of excessive pain. Pulse 130; tongue dry and brown. He had

taken a large dose of castor oil. Had had repeated enemata, and another was preparing at the time of my visit. I expressed a decided opinion that perforation of the intestine had taken place; deprecated any further attempt to move the bowels; gave a teaspoonful of laudanum, and ordered pulv. op. gr. ii, calomel gr. i, every six hours, to keep him easy. Death took place the following day." In the post-mortem examinations appear the following passages: "The intestines were seen to be *universally inflamed*, but particularly so over the region of the cœcum." Then, seven lines farther, the following: "The intestines, being cut through in their length, appeared *perfectly healthy*, with the exception of a shallow gray ulceration found at the lower part of the ileum, *supposed* to be in the situation of Peyer's patches." Then there was a concretion found, "*supposed* by myself and others to be a cherry-stone, but may be a phosphatic concretion," &c. This concretion is in the society's possession. Such a report the Boston Society of Medical Improvement prints in the year 1864, and with a coolness as if that was perfectly scientific, and not otherwise. In the first place, there is again no diagnosis made. Because a man has a slight diarrhœa, to stop it is the practice of old women; but a scientific physician asks first what causes the diarrhœa. But to prescribe six grains of acetate of lead with three grains of opium per day in this case, without a diagnosis, is very bad. There is no physician living that would dare to take such a dose when he was strong and healthy; and this patient was weak and in a very feeble condition. Acetate of lead is *to-day* no internal remedy at all, in no disease whatever, but a filthy, dangerous, unmanageable poison. And to prescribe acetate of lead, together with sulphuric acid, shows an entire ignorance of chemistry and of the material prescribed; for acetate of lead is a chemical re-agent for sulphuric acid, and forms, under all circumstances, sulphate of lead. After the lead and opium had contracted the intestines so that it was impossible to effect any movement, something to move the bowels, and a large dose of castor oil, were given. Sugar of lead, opium, quinine, sulphuric acid, wine, something to move the bowels, large dose of castor oil, repeated enemata, opium by teaspoons and by grains with calomel, all in three days, in a reconvalescent of typhoid fever, where ulcers in the intestinal canal must be expected, and therefore any thing irritating

must be carefully avoided! In what pathology or in what region of the world is such practice taught? \* Yet the Boston Society of Medical Improvement listen to that, and say nothing; the secretary reports it, and the paper prints it, and the "scientific" editors can't see it!

Their report begins with a case where any physician who understands midwifery would have known immediately how to deliver that woman, but where the professor of midwifery in Boston gives her a ride of fourteen miles. When a so-called backwoods practitioner or a very unexperienced man does not know when it becomes his duty to break the membranes, it might be excusable; not so for a professor of midwifery. But to make a lady under such circumstances leave her home, expose her to the dangers of a ride through the country, without reason, that appears to me positive malpractice. That accidentally not a misfortune happened is no excuse, but only a fortunate accident: "during the drive he feared that she might be delivered, but reached town safely." A scientific man, if he ever could have made such a blunder, would take a lesson from that for the future; but the professor of midwifery publishes such doings boastingly, as if there were no other man that knew any thing about it, and as if he were the responsible manager of life and death among women in trouble. Do the physicians of Boston all agree with such practice, such example? Can they admire such a society, and such a paper, and such a party, which not only allow such nuisance, but who print and boast of it as medical improvements? I have long enough had mercy on them, and swallowed their snubs and their ungentlemanly behavior, because I did not want to strike the first blow. They may snub and sneer at me personally as much as they please; but they shall not drabble my papers. No matter how insufficient and sketched they are, none of them can produce any thing better, and but very few of them are educated enough to understand them,—ideas which I can defend before the whole world as correct. And, if I am mistaken, can I not become corrected? Have I not done every thing to be heard,

\* That concretion, "*supposed* to be a cherry-stone, but *may* be a phosphatic concretion," may rather be a medicinal concretion, I believe.—*Remark of John Smith.*

to be corrected, to be contradicted when wrong? I never pretended to cure gangrenous lungs, as Prof. Shattuck, but only degenerated ones. I cannot cure *mortification*; but I can positively cure a *degeneration*, as tubercles.

Two questions come into consideration: 1. Can consumption be cured? 2. How can it be cured? To answer the first question, I shall translate a passage from a work written lately by Dr. Buechner, a well-known German physician. He says as follows: "*The tubercular disease is curable*, provided that it is diagnosed at the proper time, which, in most cases, is only possible through those physical methods of examination founded by Auenbrugger and Laennec, or through percussion and auscultation of the chest. That this thesis was doubted formerly, was owing to the want of that experience acquired now through the post-mortem examinations of persons in whom, not seldom, the indubitable signs of former existing and afterwards healed tubercular processes can be found. *To-day, the most eminent writers on tuberculosis—in France, Piorry and Thiercelin; in England, Alison and Churchill; in Germany, Cannstadt, Niemeyer, and many others—pronounce the curability with the greatest positiveness: Thiercelin, even the curability in all stages, &c.*"

Should these men, none of whom I ever saw, be members of the Medical Society in Boston, under the name of Smith or Both, then they would be all quack doctors and nostrum makers. I am proud to be such a quack doctor. I am proud to be, to my knowledge, the *first* physician in America, who, by his own investigations, pronounces tuberculosis as "*unfailingly*" curable. I have been taught to look at this disease as incurable: to-day I pronounce any physician that doubts or does not know any thing about the curability of tuberculosis, a physician who is not up to the level of medical science; a man who does not know what happens in the world; in one word, an *unscientific physician*. I have read my original theory before the physicians of Boston; I have offered to show my theory on patients given to me; I have read it before men, before the Medical Society; I have shown and explained it to all physicians I am acquainted with; and I have published it: so far I have not found any thing incorrect in it, nor *can anybody* else find any thing incorrect in it.

The second question is, How can it be cured? Neither in the book of Dr. Buechner, nor in any other book, do I see mentioned a method for cure, nor a certain principle about it. Every thing is more generally recommended, and the so-called "cures" consist in medicines which I pronounce insufficient and helpless, without exception. There is but one remedy in the world which will stop immediately the process in the lungs, and heal it, if the general circumstances allow this. This is *common pure air*, — this is the only remedy which I use to cure the lungs. And I claim as my own original investigation the following fact, which I have proved and can prove by experiment: the tubercles, inflamed lung-vesicles, inflamed lung-tissues, cease immediately their process of further degeneration as soon as air divides and extends their volumes. The air not alone washes out all cavities, all pus and secretions, from the lung-cells, but acts first, through its oxygen, on inflamed parts, as irritants, similar to cauterization with nitrate of silver; and, second, through its nitrogen and produced carbonic gas, as extending power and as pressure on the enlarged capillaries, like a bandage (*spica*) on the leg or arm. The difficulty about it consists, first, in the (sometimes vain) efforts to get the air between the sick parts; secondly, in the danger of producing emphysema in the healthy parts by too forced treatment. The effect is always the same, in all stages, in cases where patients cannot live if the further process is ceased, or in those which get better immediately. Once more I repeat, for the benefit of the members of the Medical Society, that I cure lungs, *unfailingly*, with common air, as I have read it before them. This is one of my "*nostrums*" and one of my claims towards my improvements in medical science.

If I find but little difficulty in arresting the process in the lungs, so lies the great important point in the digestion of food and the regulation of circulation and digestion in the blood. I must say here, that all my studies have concentrated themselves on one point: that is, to find out the anatomical and chemico-physiological cause of diseases, and to treat and cure them through dietetics; to avoid, if possible, all drugs. If I am proud of any thing in the world, I am proud of this object, and the results I have achieved thereby. No matter how imperfect these results are yet, I have succeeded in finding a method

with strict laws to it; and in this method and these laws lies one of my other "nostrums" in the success of the treatment of phthisis.

As I have already declared in my pamphlet, I have no peculiar remedy at all: I prescribe all my remedies; and they are made in the apothecary store of Mr. Babo. It is true that I use several extracts which are imported from Germany, and not commonly used: perhaps these, to my accusers unknown names, and the fact that I write my prescriptions in Latin, and not in English, may be the cause of my being accused of using "secret" remedies. To satisfy the curiosity of those persons, I will declare here, that, if necessary, I can cure tuberculosis without any medicine at all; for I have learned to find all the ingredients I need in a certain diet: only it is more convenient for me and for my patients to get in a few ounces of extract which I otherwise could get only through a very strict and sometimes inconvenient diet. And I declare to the members of the Medical Society, that this my method of treatment is not only answering to all progresses of medical science, but that it is the only method by which consumption ever can be cured; and that, no matter how much they ridicule and abuse it, no one of them *can* scientifically contradict it; and that, in a short time, they will be obliged to adopt it, with or against their will; for the public mind, however ignorant it is in medical matters, is already sick of cod-liver oil and whiskey, of hypophosphates and fusel-oil, because nobody was ever cured by it.

For the present, I believe, this will answer the questions in the *Journal*. To the editors I shall tell a little "secret" story, which I think will do well for the *Journal*. "There was once an old fox whose teeth were rotten, and whose legs very weak, so that a bunch of grapes hung entirely too high for him. The cunning animal started off, and said, 'Those grapes are miserably sour, and are utterly unworthy of the notice of a scientific fox.'" I am much more polite, very polite indeed; for I have criticised *only one* of their papers, and find it "very worthy of the reading of everybody," for it is very important to know where malpractice and fearful quackery are practised, supported, printed, and defended.

CARL BOTH.

BOSTON, November, 1864.



