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ON THE TREATMENT OF A SEVERE CASE OF
LIGNITUDO CAPITIS.

BY DR. CARL BOTH.

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THROUGH my publisher, Mr. Moore, of Boston, I received a copy of the New York Medical Journal, for August, 1873, containing (page 201) an attack upon my book on Consumption, which is written in a style neither scientific nor professional, but personal; intentionally misrepresenting my book to mislead others, who have not seen it, in their judgment. I should hardly have noticed the article did it not give me occasion to *prove* a sweeping assertion made in my book, and to show the total incompetency of that very class of men whom I intend to hit in my book, and who, like the Tammany ring, pretend to rule medical opinion by combination, thus making it almost impossible to introduce anything tending to eradicate that superficial, farcical knowledge which is taught from the Cathedra as scientific medicine, and which in practice results in a legalized quackery, called allopathic or homœopathic, a disgrace to science and a scourge to the country. The style of this article permits me to set aside all courtesy and to use severe language, but knowing that the editor is used as a catspaw for others who are afraid of handling the chestnuts, I shall confine myself to show the superficiality and utter incompetency of the editor, Dr. Wm. T. Lusk, Professor at Bellevue College, at the same time giving him occasion to prove me mistaken by answering better than I have done, the difficult and unknown points in question.

The object of the article is to stigmatize my book as a quackish fabrication. That "the book abounds in new views" is admitted, but "they lack any particular support," and are presented in such a way that I, myself, should fail to recognize them as my work. If such was really the facit of my book, I would throw it into the fire at once; but Dr. Lusk not only fails to represent my views, but he utterly fails to show or point out a single mistake in them. The results of twenty-two years of toilsome work and costly study, partly under the best teachers the world affords, partly under the greatest difficulties imaginable, results which have run the gauntlet of private, public, professional and practical investigation for fifteen years, without a spot of damage, he condemns upon his authority by one glance; especially my views of disease and my treatment; he also charges me with "not being familiar with the views of Max Schultze, Stricker and Rolett, and the latest observation of German authors generally."

Let us see what justification my views lack: When I first announced cellular theories and their consequences in Boston, I was ridiculed and my views pronounced a humbug. The very men who then thought themselves justified in treating my views with contempt, and who refused even to investigate them, think differently now. I will subject my views, to-day, to the following men of Boston, to Prof. Henry I. Bowditch, D. H. Storer, Henry I. Bigelow, Winslow Lewis, Horatio R. Storer, Samuel Cabot, Prof. Wyman, in Cambridge, and other men of unquestionable reputation; who know how hard it is to produce any new fact, and if they think my views are quackish I will begin seriously to consider the matter.

What was quackery in Boston in 1858, is to-day the hard nut on which the younger professors at Harvard College nibble without results, in spite of importation and great advertisements. What was laughed at in Boston in '63 was published in the New York Medical Record in '68. Is the Medical Record a quackish paper? or, has anybody refuted an iota of my ideas?

My views have been published, with plates, in the Journal of the Gynaecological Society of Boston. Dr. Aufrecht, of the staff of Schmidt's Jahrbücher in Leipzig, has my work under consideration for scientific criticism. My views have appeared with original wood cuts in the Journal of the Imperial Faculty of Vienna. They have been presented before the Gynaecological Society in Boston; before the Society of Medical Improvement in Cambridge; before the Boston Society of Natural History. Prof. Gaillard Thomas, of New York, quotes one of my papers in his latest work. All my papers are mentioned in the regular list of Medical Authors in Leipzig. My name is known all the way from San Francisco to New York, and from London to Vienna, in scientific quarters. My views have been assailed by the Boston Medical and Surgical Journal; by Professor Horsford; by the Scientific American. Have they proved anything quackish or wrong? If my views do not stand in such general approbation as they deserve, is it my fault? or, is it the fault of those who ignorantly refused to test them, either from ill-gotten prejudice or from conceited ignorance? Can nonsense or quackery keep itself for fifteen years before scientific societies, in the very first medical journals, and before the whole world, without being once exposed and proved as such?

Let us see how little I know about Max. Schultze, Stricker and Rolett: The "Oestreichische Zeitung fuer praktische Heilkunde," the organ of the Vienna faculty, 1871, No. 9, begins with an article by Dr. Carl Both, of Boston, showing, with wood-cuts, the three existing opinions on the minute anatomy of the lungs. Dr. Lusk will find there that I am not only familiar with Schultze's anatomy, in Stricker's histology, but that I have shown good reason why I disagree with Schultze, and how he made a mistake in observation, and how the anatomy really is, and why it must be so.

Long before I ever heard of the existence of Dr. Lusk, I was consulted by a patient and pupil of Prof. Rolett, a tubercular medical student, who

came all the way from Austria to Boston, and who returned after seven months with cured lungs, much to the astonishment of his former attendants at Wuerzburg and Vienna.

If my views on cells and their growth "do not correspond with the latest German authors," they undoubtedly dissent from them, which to know is quite instructive for Dr. Lusk. But I will show instantly how much better it would be for Professor Lusk, instead of boasting of acquaintance with the latest observations of German authors, to study the fundamental knowledge laid down by older German authors, or the A, B, C of scientific medicine. He says: We are reminded, in reading the author's views of disease and its treatment, of the passage in Macbeth, "Fillet of a fenny snake," etc. Dr. Lusk wrote this thinking the view of disease, as given in my book, originated with me; but he will be much astonished to learn that what "reminds him of hellbroth," is a *verbal translation from Foerster's Pathological Anatomy, Jena, 1856, page 8*, and is the foundation which made Foerster the greatest pathologist of his time. And it is significant that the very paper which Dr. Lusk so bitterly condemns, earned, when first published, in 1869, a very flattering letter from Dr. Lewis A. Sayre, Professor at Bellevue, a man of whom America may justly be proud. Here the reality of the surgeon recognized at once what the unreality of Dr. Lusk so utterly failed to see.

He next ridicules my treatment, which I claim to be unailing in its application.

In 1863 I offered to show it in Boston, 1868 at the Bellevue College in New York. Without any reason, against the rules of American Ethics, against professional custom since Hippocrates, against all principles of science and humanity, it was refused in spite of my offer to pay for extra expenses! What entitles Dr. Lusk now to ridicule a treatment which he never saw applied, of which he knows actually nothing, and which does not rest upon drugs but upon physical, chemical and physiological *known* laws? Can Dr. Lusk state an instance where my treatment failed upon application? Is it likely that a man offers a humbug for *test* before his adversaries and at Bellevue Hospital? My treatment is a method, and as unailing as the methods of surgery or of analysis; that it is difficult in its application, that it is in my possession only, that it is not known by all, is it my fault? Because everything else has failed yet, is it justifiable to condemn my method without test and without knowing it? What I have tested in practice for fifteen years, and found without fail, is it to be insulted by men who, theoretically or practically, do not outweigh a Homœopathic dilution?

Dr. Lusk, as much as I know of him, was a student at Bellevue, was called to Boston as Professor of Physiology, where he was a favorite of a class of men of whom Dr. Storer in Boston told me "he considered it a disgrace not to be hated by them." In as much time as it takes the train to go from Boston to New York, the Professor of Physiology turned into a Professor of Midwifery and Diseases of Woman, at Bellevue. He next appears as editor of Appleton's Medical Journal, and now as the latest and

and conclusive authority on Consumption. If another mortal should perform such salto mortale, he would be considered an idiot or a second Cagliostro—but, of course, Dr. Lusk stands far above such insinuations, and he will prove what he is by answering the following questions, which to answer I have so badly failed:—

He will explain to the world: I. The minute anatomy of the lungs. II. The true theory of respiration. III. What consumption is. IV. Classify the lung diseases. V. He will show how to cure consumption. VI. What whooping cough is. VII. What scarlatina is. VIII. How to cure both of them. IX. He will demonstrate what disease is, and prove Foerster an ignorant quack. X. He will tell us all about cell-growth. XI. He will bring the cellular pathology into actual practice. XII. He will not fail to do all this, better, of course, than I have done.

As soon as Dr. Lusk has accomplished this, I promise to throw my book into the fire, become the pupil of this modern Hercules, and will spread with loud voice *his* ideas, *his* discoveries, and *his* results! Amen.

In anticipation of this great scientific revolution, and in conformation, with the great Dr. Lusk, by ending with poetry, I conclude with a concoction of my own, which is not so classical as Shakespeare, but much more to the point and the truth:

Heraus mit Eurem Flederwisch
 Ihr Cundurango Helden!
 Wie sie sich raeuspren und wie sie spucken
 habt Ihr gelernt schnell nachzudrucken.
 Aber — dummer noch als dumm —
 mit umgedrehten Paradoxen,
 Swapnia, Sporen, Clima, Noxen,
 zieht man Euch an der Nase 'rum.
 Mit Facten koennt Ihr nichts gestalten,
 Mit Faxen nur und leeren Worten
 Gutglaeubige zum Narren halten.
 Euer ganzes Wissen und Denkerthum
 heisst: Ego sum. — M. D. — silentium.

Rochester, N. Y., September, 1873.