

Pain was our teacher, speaking to the heart,
 Mother of pity, nurse of pitying art;
 Our lesson learned, we reached the peaceful shore
 Where the pale sufferer asks our aid no more;
 These gracious words our welcome, our reward,
 Ye served your brothers; ye have served your
 Lord."

THE PRACTICAL STUDY OF BIOLOGY.¹

BY MARY PUTNAM JACOBI, M.D.

MR. CHAIRMAN,

In accepting the very great honor of an invitation to be a guest at the annual meeting of the Massachusetts State Medical Society, I did not expect to be called upon to speak before it. But since, Sir, you have chosen to still further honor me by calling upon me, it would be churlish to refuse.

In a meeting like this, I presume it is intended that each person present shall bring forward the thought or thoughts that may have especially preoccupied him or her during the year that has gone by. Now, one of the subjects which has especially preoccupied me is one that I think must interest everybody who is either a student or a physician or a director of medical education. I refer to the question of the practical study of biology, in its threefold aspect of normal physiology, of pathology, and of elementary therapeutics.

There is a strange idea current among the laity, and even among many physicians, that the study of physiology by means of practical experiments and demonstrations on the living subject is only necessary, and indeed only permissible, for purposes of occasional original research. You know that this is the view taken by so august, yet in the premises so incompetent a body as the English Parliament, which has assumed the right and duty of actually forbidding physiological experimentation for any other purpose. But in reality the necessity is not occasional, but permanent, and coextensive with the education of every medical student. For what are the facts of the case? We undertake to prepare ourselves and others for the most profound and subtle and difficult of all sciences, the Science of Life; we undertake this, not for the purpose of irresponsible contemplation, but with the avowed intention of practically intervening among the phenomena of life, of regulating disturbances in the mechanisms of living organisms, of bringing normal order out of what may have become the wildest disorder and confusion. Now, how is it possible to do this unless the mind has first become thoroughly and personally acquainted with the normal order? Students in other natural sciences than animal physiology know perfectly well that listening to a didactic lecture or coming the pages of a text-book are entirely insufficient means to bring the mind into fertile contact with nature. Such contact cannot be obtained second-hand, but only by those who, as Claude Bernard says, have, in the hospital, the amphitheatre, and laboratory, with their own hands stirred the soil, fetid and palpitating with life. The difference is enormous between the person whose knowledge of physiological phenomena is summed up in a list of verbal statements, and the other whose mind has become saturated with vivid conceptions of vital facts, based

upon multiple experience of them. No one who has not tried it, knows how indefinitely both intellect and senses gain in delicacy and subtlety when they have become habituated by practical intercourse with the endless intricacies of nature. And it is the peculiarity of medical work that the necessity for such mental subtlety is not confined to the few, to the *élite*, to the college professors who, very likely, have withdrawn from the practice of medicine. The poorest sick person in the hospital, the most tedious invalid of the private *clientèle*, serve to illustrate all the mysteries of the science, and demand on the part of their doctor the ability to cope with its deepest and most difficult problems. The physician whose thought is sufficiently elevated, and whose imagination is sufficiently keen, confronts this commonplace sick human body as an antique priest may have stood before the veiled mysteries of Isis. It is his privilege, and to an increasing extent, to draw aside the veil of the surface, to plunge his eye into the depths of the organs and tissues as they are actually at work, to follow in imagination the innumerable streams of vital actions which are eddying and swirling in every direction, and to try, out of the dizzying maze, to construct a truthful chart of the vital conditions and tendencies of the organism. If he cannot do this, or attempt to do it, if he has not been previously trained to a profound feeling for the complexity and intricacy of vital processes, he is liable to lay heavy and clumsy hands upon them, and to estimate them according to theories both coarse and crude. When he gives a medicine he simply "exhibits" it. He has no distinct conception of a train of vital events, among which he is to insert some new and deliberately contrived conditions, by means of which the direction of the whole series may be modified. This is the ideal formula for physiological therapeutics. It is far enough from being realized or realizable to-day; but it seems to me no one can doubt that it is ultimately destined to supplant the rule of traditional "*Secundum artem*," with which at present we must too often be content.

It would be indeed the merest pedantry to attempt to base existing therapeutics exclusively upon existing physiological knowledge. No practising physician can or would follow Herman's classification of remedies, when he places first the utterly useless oxide of carbon because we know all about it, and consigns to a limbo of doubt opium and quinine as drugs of which we know little or nothing. Whether we know or not, we are compelled to use them. But it is absurd to say that we know nothing because we do not know analytically. We do possess an accumulated amount of experience in regard to these priceless remedies which is of immense and scientific value, although the ultimate reasons for their action have not yet been demonstrated. But on this very account, and because an exact and quantitative formula cannot be given for the action of drugs, we are obliged to intrust their handling to the acquired tact of long practice. Now tact, mental or physical, can only be acquired by incessant and varied experience in the practical operations requiring the exercise of tact. How can be acquired the mental tact needed to unravel the complexity of vital phenomena as they unravel before us in the history of

¹ Remarks addressed to the Massachusetts Medical Society at its Annual Dinner, June 12, 1889. See page 635 of this Journal.

every sick person? how can be learned the tact needed to undertake the direction of these phenomena, except by previously prolonged study of vital phenomena in the simpler animal body free from disease?

If it be admitted that the dead body must be dissected, as was still practically denied less than a hundred years ago; if it be allowed that the lesions left by disease must be studied in autopsies, which are still often refused in obedience to the grossest superstitions; if it be conceded that medical students must have some clinical experience with the sick before they are allowed to graduate, concession which still often remains a paper right, — it equally follows that the same persons must study living organisms, in the only way in which they can be studied, by analyzing, dissecting, and handling them in the process of life. There is no need now of wading through the horrors of physical pain amidst which Magendie laid the foundations of modern science. The use of anaesthetics obviates the objections on the score of cruelty for this as for other forms of surgical operation; and it is certainly one of the most extraordinary demonstrations of modern science that life can be so dissected and dismembered and yet persist.

The kernel of the question lies just here. Knowledge of the living organism, for practical purposes, must be obtained practically. Indeed, no knowledge is ever obtained in any other way, but only a verbal imitation of knowledge. The physician who has not learned to adjust himself to the intricate delicacies and fragilities of living organisms by laboratory study, is condemned to take his first lessons in dealing with life upon human beings. He is compelled, therefore, to experiment upon subjects who are often, if not always, far more valuable than the frogs and rabbits of the laboratory.

Sooner or later, if he is to be successful, his whole mind must have become modified in that mysterious manner in which the minds of students of nature do become changed, as they seem insensibly to blend with the phenomena they can profoundly contemplate. But the question is, shall this necessary training come sooner, or later? Shall it come economically, or with tremendous though carefully concealed expenditure of human life? Whether we will it or no, all practising physicians are constantly and professionally engaged in physiological experimentation, are trying the most audacious of experiments, — trying, namely, to deliberately modify the course of human life. Is it not evident, therefore, that we should strive to the utmost to obtain the most real and profound visions of life before we venture on our attempts at interference?

I should like, Mr. Chairman, to mention an incident that occurred to myself in the course of a very simple laboratory experiment. I was examining the circulation of a frog's lung by means of the Holmgren apparatus. I happened to so focus my lens that all the outlines of the capillaries and blood corpuscles disappeared, leaving visible only the spaces between the epithelial cells. Nevertheless there remained a vision of the streaming movement of the invisible blood through the ramified spaces. The streaming was so rapid, so energetic, so ceaseless, it seemed as if it were pure motion or force divorced from the accidents of

matter. The microscopic shred of tissue from the insignificant animal seemed for the moment to give a glimpse of a mighty vision of endless life, streaming with infinite energy into the minutest particles of an infinite universe. The impression was indescribably powerful. Since then I have confronted new students with this same impression, for the purpose of throwing open at once the horizons towards which they were henceforth to keep their eyes directed. And this is what it seems to me all students in medicine should learn to do.

Original Articles.

ON THE DISTRIBUTION OF DISEASE.

A REPORT AND AN APPEAL FROM THE COMMITTEE OF THE MASSACHUSETTS MEDICAL SOCIETY ON THE DISTRIBUTION OF DISEASE.¹

BY W. EVERETT SMITH, M.D.

YOUR committee, although by no means ready to present even a preliminary report upon the distribution of disease within the State, would at the same time, in order more effectively to prosecute their studies, make the following brief statement, outlining to some extent the *importance* and the *nature* of the work in which they solicit the active co-operation of the society at large.

Diphtheria, Scarlatina, and Phthisis. — Recent studies by Kelly² in England upon the distribution of diphtheria in the West Sussex District, and by Barnes³ upon the distribution of diphtheria and scarlatina, seem decidedly opposed to the conclusions of Maier⁴ that "it is more than doubtful whether the geological position of the place influences the epidemic appearance of diphtheria." Dr. Barnes, although the number of the cases which he observed is small, finds that the relative proportion of outbreaks of diphtheria to the inhabitants is 1 to 1800 on gravel and 1 to 300 on clay soil.

In his graphic outline maps of Great Britain he shows also "that there are certain counties in which the death-rate from diphtheria is invariably high and certain others in which it is invariably below the average; that there are some counties in which it is usually though not invariably high, and others in which it is usually though not invariably low." In comparing these maps, it is clear that "not only are the favorite habitats of diphtheria different from those of scarlet fever, but it will be observed that scarlet fever, like other infectious diseases, is most prevalent and most fatal in the districts containing the most dense population. Diphtheria, on the contrary, is most prevalent in the comparatively thinly populated districts." It seems therefore as though diphtheria, although undoubtedly infectious and spread from person to person more easily in populous districts, yet finds in rural districts an agency which favors a more prolific development; and a reasonable explanation seems to be dampness of soil, combined, if you will, with filth.

In this connection a study of the epidemic of malignant diphtheria which occurred in Vermont in

¹ Read at Annual Meeting of the Massachusetts Medical Society, June 12, 1889.

² Sixth Annual Report of the Condition of the Combined Sanitary District of West Sussex. By Chas. Kelly, M.D., F.R.C.P., 1889; also Fourteenth Annual Report, 1888; also personal correspondence.

³ An Address on the Etiology of Diphtheria. By Edgar G. Barnes, M.D., Brit. Med. Journal, July 28, 1888.

⁴ Vide Annual of the Universal Medical Sciences, 1888, vol. iv. p. 303.