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LAENNEC,

HIS LIFE, LABOURS, AND INFLUENCE IN MEDICINE:

BEING THE

Address in Medicine

DELIVERED AT THE

ANNUAL MEETING OF THE BRITISH MEDICAL ASSOCIATION
IN CORK, AUGUST, 1879.

BY

ALFRED HUDSON, M.D.,

REGIUS PROFESSOR OF PHYSIC IN THE UNIVERSITY OF DUBLIN.

REPRINTED FROM THE 'BRITISH MEDICAL JOURNAL,'

With Notes and Additions.

DUBLIN:

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BY PONSONBY AND MURPHY.

1879.

LAMBERT

THE LAMBERT AND LUTHERAN IN MEDICINE

Address in Medicine

ANNUAL MEETING OF THE BRITISH MEDICAL ASSOCIATION
IN 1881, AUGUST 1881

ALFRED HUBSON, M.D.

LECTURE IN MEDICINE IN THE UNIVERSITY OF DUBLIN

Printed by J. & J. Gifford

PRINTED AT THE UNIVERSITY PRESS
BY HENRY AND SONS

1881

A D D R E S S ,

& c. & c.

WHEN the Council of the British Medical Association did me the honour to nominate me to deliver the Address in Medicine on the present occasion, my great difficulty was the choice of a suitable subject; what to select from the wide field of the past, present, and future of Medicine?

To attempt a review of the triumphs of the present and recent past would be to follow, by unequal steps, in the path of those who have so ably and exhaustively treated the subject, on many previous occasions, and to repeat to-day what you have read in the retrospects, records, and journals of yesterday; while to pourtray the glories of the future of Medicine would require a prescience which I do not possess.

Besides having arrived at an age when a man naturally becomes a "*laudator temporis acti*," I felt attracted to the consideration of the more remote past, and I resolved to offer a few observations on the history of its several epochs, and of their representative men, and more especially

of that great revolution which was in progress in my student days, when the era of nosology was superseded by that of pathology; when the theoretical deductions of Cullen and his predecessors were replaced by the inductive method of the present day, as inaugurated by Laennec.

In a comprehensive review of the past, three things attract our attention—its history, its progress, and the lives of the men who have marked its successive epochs.

Such a task differs from that of the student, or of the worker of the present day, in that, while the one necessarily seeks for advanced knowledge, in the most recent sources, and the other founds his conceptions on those of his more immediate predecessors, a retrospect of the kind involves some notice of men whose works are now obsolete, and whose names have become mere traditions in the history of the science.

Its history has not always been one of progress, but rather of a succession of cycles; too often a record of barren hypotheses and fanciful systems; of the lives and labours of men, of marvellous ability and industry, whose aim was to attain that which was unattainable:

“Crying for the light,
And with no language but a cry.”

These men have been sometimes compared to giants who, taking us in their arms and lifting us above their heads, have disclosed to us countries

and prospects which they themselves could never discover. Living before their age, and being unable to explain observed phenomena by the aid of the imperfect science of the time, they resorted to hypotheses evolved out of their own consciousness, which were really not explanatory of diseased processes, while their ideas of therapeutics were made to correspond with the imaginary conditions. Not that their labours were altogether useless, for, as has been well said, "As in the growth and development of the body the daily death of the tissues is in strict relation with the activity of life, so, in the organic growth of thought through the ages, there is a corresponding decay or consumption of erroneous doctrines—a death of the false in strict relation with the growth of the true."

The comparison of the growth and development of science to that of the human body is by no means a novel one, and it is employed by Lord Bacon in terms peculiarly applicable to Clinical Medicine, and a suitable motto for my discourse: — "As young men," says Bacon, "when they knit in shape and perfectly, do seldom grow to a further stature, so knowledge, while it is in aphorisms and observations, is in growth; but when once it is comprehended in exact methods, it may perchance be farther polished and illustrated, and accommodated for use and practice, but it increaseth no more in bulk and substance."

The different stages of this comparison of progress correspond with the lives and labours of the great men who have marked the successive epochs of the science. While those of Harvey and of Haller are associated with the birth and progress of physiology, and Hunter is commemorated periodically as the founder of scientific surgery, the great names of Hippocrates, Morgagni, and Laennec correspond to the eras of aphorism, observation, and method, which each of them in turn illustrated by his life and works—their labours with those of their followers having accumulated a body of practical truth which has been not unaptly compared to a mighty river moving for ever onwards, widening, deepening, strengthening as it flows—each individual life, moreover, affording practical lessons of value. Thus the life of Hippocrates teaches the value of that early acquaintance with the objects of his future study now too much overlooked. We are told that “amid the sports of childhood he received from the mouths of his parents the elementary notions of medical science. By viewing diseases, he learnt to distinguish them.”¹ Himself convinced of the superiority of this method of practical instruction, Hippocrates wrote: “In order to improve to a certain degree the knowledge and practical skill of the physician, it is

¹ Cabanis, *Revolutions of Medical Science*.

necessary (independent of natural genius, the place of which nothing indeed can supply) that he be placed from early infancy amid all the objects of his researches, and that every means of instruction be employed with unremitting assiduity."

Of the character of his writings Cabanis remarks—"He brought the science back again to its natural channel—that of rational experience. He freed Medicine from false theories, and formed for it sure and solid systems. His books of aphorisms have in all ages been regarded as models of grandeur of conception and precision of style. Through the whole of them we may remark that truly universal method, the only one which is adapted to the mode in which our intellectual faculties are exercised, and which in every art and in every science, by making the principles flow naturally from the observations that have been collected, transform the deductions from facts into general rules." This writer adds:—"If the disciples of Hippocrates had understood his lessons well, they might have laid the foundation of that analytical philosophy, by the aid of which the human mind will be henceforth enabled to create to itself, as it were daily, some new and improved instruments of advancement."—It is scarcely necessary to remark how exactly this prediction has been realized in the history of physical diagnosis and the life of Laennec.

The traditions of Hippocrates and his natural method, having outlived the centuries of dogmatism and of successive systems, have survived to the present day, transmitted through the dark ages of medicine in the aphorisms of Lommius, Baglivi, Boerhaave, Stoll, and others—aphorisms which, while comparatively free from the dogmatism of their age, contain matter of the highest practical value. One of the most illustrious of these writers (Baglivi) devotes two chapters to treating of the hindrances (*impedimenta*) of progress arising from neglect of ancient aphorisms and observations, and love of new systems, maintaining that “the entire value of the history of diseases depends on the careful and patient description of that which the observer, skilled in the mode of invasion, the progress, and issue of diseases, has perceived.”² The writings of these men, like those of their great master, are especially distinguished by the fidelity of their descriptions of symptoms and the sagacity with which they anticipated sequences. They read the prognostic signs of disease with an accuracy and an insight

“Whose old experience doth attain
To something of prophetic strain,”

and are for this reason, if for no other, worthy of the careful study of the practitioner. By the side of these we place the English Hippocrates,

² *Praxeos Medicæ*, lib. i., cap. 9. See Note A.

Sydenham, the great observer, who, says the editor of his works, now stands out at nearly the end of a second century as the great representative of the practical medicine of practical England.

“It would not be easy,” observes the author of an admirable essay on ‘Locke and Sydenham,’ “to over-estimate the permanent impression for good which the writings, the character, and the practice of Sydenham have made on the art of healing in England and on the Continent generally. In the writings of Boerhaave, Stahl, Gaubius, Pinel, Bordeu, Haller, and many others, he is always spoken of as the father of rational medicine; as the first man who applied to his profession the Baconian principles of interpreting and serving nature, and who never forgot the master’s rule ‘non fingendum aut excogitandum, sed inveniendum quid natura faciat aut ferat.’ He was what Plato would have called an ‘*artisan*,’ as distinguished from a doctor of abstract science. But he was by no means deficient in either the capacity or the relish for speculative truth. Like all men of a large practical nature, he could not have been what he was, or done what he did, without possessing and often exercising the true philosophizing faculty. He was a man of the same quality of mind in this respect with Watt, Franklin, and John Hunter, in whom speculation was not the less genuine that it was with them a means rather than an end.” — *North British Review*, vol. xii.

The influence of Sydenham is especially seen in the writings of the succeeding century. Of Grant and Huxham, of Pringle, and others whom a distinguished American writer on Fever terms, “those glorious old British observers — the types and ornaments of a school never since sur-

passed by their countrymen." And I may here remark that the study of these and the above-mentioned older writers would greatly tend to moderate extravagance of doctrine, and exclusive devotion to special methods of treatment in our own day. These British writers, as Dr. Bartlett observes, clearly recognised the distinction between the two great forms of continued fever which the great majority of the British physicians in his day "refused to admit, or to endeavour to ascertain";³ while, of the wisdom and breadth of their views of treatment, no higher testimony need be adduced than the candid acknowledgment of Dr. Stokes, who says in his memoir on the state of the heart and use of wine in typhus fever,⁴ "I confess that it was not until several years after I commenced practice that I became fully aware of the erroneousness of what is termed the anatomical theory of disease; and I feel certain, humiliating though the confession may be, that the fear of stimulants, with which I was imbued, was the means of my losing many patients, whose lives would have been saved had I trusted less to the doctrine of inflammation and more to the lessons of experience, given to us by men who observed and wrote before the times of Bichat or of Hunter."

³ *Philosophy of Medical Science.*

⁴ *Dublin Medical Journal*, vol. x., First Series.

I would add one remark to the above. It is, that a careful perusal of the excellent, albeit somewhat antiquated, chapter of Lommius on the diet of a fever patient might help to guard the young practitioner from the too prevalent perversion and abuse of Graves's famous saying, "He fed fevers."

In England these great observers were succeeded by men scarcely less noteworthy; by Fothergill, Fordyce, Heberden, and Currie, whose practical writings will live as long as the art is cultivated. Contemporary with these were others of great ability, who aimed at generalization rather than observation, and who strove to construct systems to which the medicine of the future should conform. I need not tell you that the systems of Cullen, Brown, and Darwin have become things of the past and are forgotten.

But we pass on to the epoch of Pathology and Clinical Medicine inaugurated by Morgagni and his contemporaries and successors. In an admirable address, delivered before the Glasgow Pathological and Clinical Society in 1874,⁵ Professor Gairdner thus tersely describes the character of Morgagni's work:—

"In investigating the *seats* of diseases, Morgagni is not content to record the *coincidence* of a lesion in an organ with the symptoms apparently due to disordered function in that organ.

⁵ *British Medical Journal*, 1874, vol. ii., p. 15.

“For the first time almost in the history of medical inquiry, he insists on examining every organ, as well as the one suspected to be chiefly implicated; not only so, he marshals with the utmost care from his own experience and that of his predecessors, all the instances in which the symptoms have existed apart from the lesion, or the lesion apart from the symptoms. He discusses each of these instances with severe exactness in the interest of truth, and only after an exhaustive investigation will he allow the inference either that the organ referred to is, or is not, the seat of the disease.

“And in like manner in dealing with causes: a group of symptoms *may* be caused by certain organic changes—it may be even probable that it is so—but, according to Morgagni’s method, we must first inquire into *all* the lesions of organs which occur in connexion with such symptoms: in the second place, we must know if such lesions ever occur, or occur without the symptoms; and, again, if such symptoms can be attributed in any cases to other causes in the absence of such lesion.”

Did time permit, I would fain add to Dr. Gairdner’s illustrations some examples of the glimpses of facts pointing to future observations and discoveries which are scattered through the portion of Morgagni’s work relating to Thoracic diseases. Such are his remarks on the connexion between disordered states of the nervous sys-

tem and deranged respiration (Letter 15):—His notice of the sound heard on applying the ear to the præcordial region in a case of pericardial effusion (Letter 16); of the decubitus in empyema and the displacement of the liver in that disease; of the signs of dilatation of the right ventricle, and the clear explanation of the phenomenon of jugular pulsation, and the wonder he expresses that the increased bulk of the heart is not attended to in the histories of asthma; and of the curious case (quoted by Wardrop in his work on the heart, p. 215,) of alteration of the radial pulse, caused by a fall on the spine, and inexplicable until the discovery of the vaso-motor system of nerves; to lesions of which similar cases have been referred by Dr. Russell Reynolds and Dr. J. W. Ogle.—*British Medical Journal*, 1868, vol. ii.

To those who, like Dr. Gairdner, look to what Morgagni has done for pathology and clinical medicine “in the light not only of his own researches, but of those of his successors,” it would seem difficult to over-estimate the value of his work and influence. But that the work itself was regarded by his more immediate successors rather as a record of symptoms and morbid appearances, than an exposition of morbid processes in the living body; as needing the explanatory *why* which advancing science has since supplied and is daily supplying, and consequently of little assistance to the practitioner at the bedside, appears from the language

of Corvisart, who, after stating that it will immortalize its author, adds the remark that this great work "neither helped nor hastened the art of diagnosis," adding, in a foot-note, that "he himself had formerly read a memoir to the Institute, expressing the idea of a work analogous, but in an inverse sense from that of Morgagni, with the title 'De sedibus et causis morborum per signa diagnostica investigatis, et per anatomen confirmatis,' but that for such a work, at least another Morgagni would be required."⁶

The hour and the man were both at hand ; and, as it has been said of Harvey, "that on the doctrine of the circulation the dawn had long been visible.—Harvey came, and the sun arose ;—so Laennec appeared, and realized the prevision of Corvisart by substituting a living for a dead pathology, and revolutionizing clinical medicine by a method of diagnosis which, by revealing the seat, nature, and *progress* of disease at the bedside, anticipated the disclosures of the *post mortem* theatre, and endued the dry and unexplained facts of both symptoms and morbid appearances with meaning and with power.

"Time," says Dr. Stokes, "has shown that the introduction of auscultation and its subsidiary

⁶ *Sur les maladies du coeur*, Discours preliminaire, p. ix. "Observation," says Mill, "without experiment (supposing no aid from deduction) can ascertain sequences and coexistences, but cannot prove causation" (*Logic*, vol. i., p. 423).

physical signs has been one of the greatest boons ever conferred by the genius of man on the world.

“A new era in Medicine has been marked by a new science, depending on the immutable laws of physical phenomena, and, like the discoveries founded on such a basis, simple in its application and easily understood—a gift of science to a favoured son, one by which the ear is converted into the eye, the hidden recesses of visceral disease opened to view; a new guide to the treatment, and a new help to the early detection, prevention, and cure of the most widely spread diseases which affect mankind.”⁷

I have alluded to men who lived before their time, who were in advance of their age; Laennec was fortunate in living at the exact period at which he did, when Bichat had just created the science of general anatomy, and Haller and Hunter that of physiology; when Morgagni had grouped such a mass of his own observations and those of others on the results of disease as entitled him to be called the founder of morbid anatomy; and when the first essay in physical diagnosis by Auenbrugger, long neglected, had been taken up and translated by Corvisart, who employed percussion as an aid to the diagnosis of cardiac diseases with much success. Laennec had thus in the

⁷ *On Diseases of the Chest*, p. 40.

works of his immediate predecessors the materials necessary for his system, which needed only the spark of his genius to burst into life and beauty.

I cannot but think it also fortunate that he did not live at a later period. He might perhaps have then anticipated some of the improvements which have followed from the modern advances in the correlated sciences; but I can imagine that had the pathological anatomy of the period ripened into the histology of the present day; a science which, says Dr. Gairdner, "can only be sounded through the absolute devotion of a lifetime to its new methods of research," his active and inquiring mind might have been altogether diverted from the study of the diagnosis of disease at the bed-side to the researches of the dead-room and the laboratory, which, however important, practical physicians must hold to be subordinate and secondary to the other.

Laennec would seem to have so regarded it when he wrote that "if the causes of severe diseases are sought for in mere microscopical alterations of structure, it is impossible to avoid running into consequences the most absurd, and if once cultivated in this spirit, pathological anatomy, as well as that of the body in a sound state, will soon fall from the rank which it holds among the physical sciences, and become a mere tissue of hypotheses, founded in optical illusions and fanciful speculations, without any real benefit to Medi-

cine.”⁸—An unfortunate anticipation, as time and progress have proved.

The life of Laennec, like the lives of other illustrious men, is not without its lessons. Although of feeble constitution and delicate health, it was marked by mental activity and incessant work. Like his great prototype Hippocrates, he became early familiar with disease, and, while a youth, showed a decided predilection for morbid anatomy and clinical observation, the future studies of his life. At the early age of eighteen he served as assistant surgeon in the military hospitals, and in the following year he became a pupil of Corvisart at La Charité. Here he proved his diligence in the study of clinical medicine by drawing up a minute history of nearly four hundred cases of disease, which, it is known, furnished the groundwork of all his future researches and discoveries.

On taking his degree, he wrote a thesis on the doctrine of Hippocrates, as applied to practical medicine, which, says M. Bayle, proved him to be no less skilled in the knowledge of the Greek language, than deeply read in the writings of the father of physic. In the previous year he had commenced a course of lectures on pathological anatomy, which he continued for three years, when ill health obliged him to discontinue them. From this time until 1816 he continued to contribute articles on general and morbid anatomy to

⁸ See Note B.

the *Dictionary of Medical Sciences*, and other publications, but on his appointment as Physician to the Necker Hospital they ceased, and having made the discovery of mediate auscultation, he devoted himself solely to the perfecting of the new system of diagnosis which he founded on it, and to the clinical teaching of his method to the numerous students who resorted to him.⁹

The history of this discovery and Laennec's observations on it are too important to be passed over without notice.

After stating that palpation and percussion give but imperfect information in cases of heart disease, he says:—"Within these few years some physicians have in those cases attempted to gain further information by the application of the ear to the præcordial region. In this way the pulsation of the heart, perceived at once by the ear and touch, becomes no doubt more distinct. But even this method comes far short of what might be expected from it. Bayle was the first who, to my knowledge, had recourse to it at the time when we were attending the lectures of Corvisart. This great man himself never used it; he says only that he had several times heard the pulsation of the heart in *listening very close* to the chest But neither Bayle nor any other of our fellow-students, who with myself might, in imitation of him, employ this immediate auscultation (of which, by the way, the first notion is derived from Hippocrates), obtained any other result from it than that of perceiving more distinctly the action of the heart in the cases where this was not very perceptible to the touch. Nevertheless I had been in the habit of using this method in obscure cases and where it

⁹ See Forbes's Translation, Life of the Author. See also Note B a.

was practicable ; and it was the employment of it which led me to the discovery of one much better. In 1816 I was consulted by a young woman labouring under general symptoms of diseased heart, and in whose case percussion and the application of the hand were of little avail on account of the great degree of fatness. The other method just mentioned being rendered inadmissible by the age and sex of the patient, I happened to recollect a simple and well-known fact in acoustics, and fancied at the same time that it might be turned to some use on the present occasion. The fact I allude to is the augmented impression of sound when conveyed through certain solid bodies—as when we hear the scratch of a pin at one end of a piece of wood on applying our ear to the other. Immediately, on this suggestion, I rolled a quire of paper into a kind of cylinder, and applied one end of it to the region of the heart and the other to my ear, and was not a little surprised and pleased to find that I could thereby perceive the action of the heart in a manner much more clear and distinct than I had ever been able to do by the immediate application of the ear. From this moment I imagined that the circumstance might furnish means for enabling us to ascertain the character, not only of the action of the heart, but of every species of sound produced by the motion of the thoracic viscera, and, consequently, for the exploration of the respiration, the voice, the rattle, or rhonchus, and perhaps even the fluctuation of fluid extravasated in the pleura or pericardium. With this conviction I forthwith commenced at the Hospital Necker a series of observations, which have been continued to the present time. The consequence is, that I have been enabled to discover a set of new signs of diseases of the chest, for the most part certain, simple, and prominent, and calculated, perhaps, to render the diagnosis of the diseases of the lungs, heart, and pleura, as decided and circumstantial as the indications furnished to the surgeon by the introduction of the finger, or sound, in the complaints where these are used.”

I have quoted this passage at length, as showing the strictly inductive character of the process of this discovery. First the observed fact, followed immediately by the anticipation based on previous knowledge of the pathological conditions of other diseases; then the repeated experimental trials, and the confirmation of the anticipation by repeated *post mortem* examinations.

The narrative illustrates several important principles common to the histories of scientific discovery.

First it was apparently, but only apparently, accidental. "Men," says Whewell, "are fond of repeating that such discoveries are most commonly the result of accident; and we have seen reason to regret this opinion, since that preparation of thought by which the accident produces the discovery is the most important of the conditions on which the successful result depends."¹⁰ Then, it conformed to the usual conditions, viz., distinct general notions, careful observation of many facts, and the mental act of bringing together these elements of truth.

Regarded as an epoch in scientific discovery, this, like other inductive epochs, had a *prelude* in the preceding period, which led immediately up to it, and to which I have already referred. It has also had a *sequel* of verification and correc-

¹⁰ *History of Inductive Sciences.*

tion, during which the discovery has acquired a more perfect certainty and a more complete development among the more advanced thinkers and improvers, has been diffused to the wider throng of the secondary cultivators, and traced into its distant consequences in its influence on the practice and teaching of Medicine and the status of the profession.

This discovery, nevertheless, did not at first excite much interest in the profession generally. It was announced in the *Edinburgh Medical Journal* of the day in these terms: "M. Laennec has discovered that by interposing a tube of paper or wood between the ear of the observer and the chest of the patient much information may be acquired concerning the diseases of the chest. The pulsations of the heart are thus rendered more audible, and in phthisical patients the voice seems to proceed from the chest when one end of the tube is placed over those places where there are tubercles, and, according as the sound is clear or rattling we may judge whether the cavity is clear, or contains pus."

After such a notice it is not surprising that the practitioners of the day did not much concern themselves about the new method of diagnosis, or that Sir John Forbes should write, three years after the appearance of his translation, that up to that time not even a single case of the use of auscultation had appeared in any British

medical journal.¹¹ Some of us can remember how the stethoscope and its inventor were sneered at and ridiculed by a few of the teachers of our own day. The new light was too strong for older eyes, and it is possible that, had not Laennec survived his discovery for some years, and continued to demonstrate its employment and results, to the younger generation, it might have left as slight an impression on the minds of the profession as had the treatise on percussion by Auenbrugger, which preceded it.

Sir James Paget tells us that "although Hunter was in high repute as the chief anatomist and naturalist of the time, yet in his great work of comparative anatomy and physiology not one of the young men of science imitated him—in the highest sciences he had not one true disciple." With Laennec it was different: unlike Hunter, he was "apt to teach," and had the opportunity afforded by his connexion with the Hospital Necker of training numerous disciples from this and other countries, one of the most distinguished of whom (Dr. Williams) still survives. Others who, like our own Stokes and Corrigan, had not the advantage of his personal teaching, drank the knowledge from his published writings, and drew therefrom an inspiration and an impulse which carried them far into the field of discovery.

From Laennec's first observation till his death

¹¹ *On the Stethoscope and Percussion*, p. 8.

only ten years elapsed, two years of which were spent in the country, while suffering from disease which afterwards proved fatal. Few men have contributed an equal amount of such valuable work within the same space of time. Of its appreciation by men of kindred genius and pursuits I have already given an example in the eloquent words of Dr. Stokes; let me add another in those of Dr. Addison:¹²—"Were I to affirm that Laennec contributed more towards the advancement of the Medical art than any other single individual, either of ancient or of modern times, I should probably be advancing a proposition which, in the estimation of many, is neither extravagant nor unjust. His work *De l'Auscultation Médiate* will ever remain a monument of genius, industry, modesty, and truth. It is a work in perusing which every succeeding page only tends to increase our admiration of the man, to captivate our attention, and to command our confidence. We are led insensibly to the bedside of the patients; we are startled by the originality of his system; we can hardly persuade ourselves that any means so simple can accomplish so much, can overcome and reduce to order the chaotic confusion of thoracic pathology; and hesitate not, in the end, to acknowledge our unqualified wonder at the triumphant confirmation of all he professed to accomplish." But is he still appre-

¹² *Works*, Syd. Soc. Edit., p. 65.

ciated as he deserves? Is he still the living oracle, or rather is not his name now rarely mentioned—fast becoming a mere tradition among practitioners and students? We might apply to him what a recent biographer of Locke says of his great work on the Human Understanding:—“By it modern philosophy has been revolutionised, and if many rival sects of thinkers have built upon the broad foundations that he laid, and some of them ignore their debt to him, that debt is none the less for their ingratitude.”¹³ His work has been long out of print in both languages, but neither the old nor new Sydenham Societies have thought it worthy of a re-issue. For the one it may have been too new—not yet fossilized; and it may be supposed that the council of the new society have passed it by, while translating and re-editing works of inferior merit, because it is too old. I am glad, however, to see an announcement in a recent number of the *Lancet* that, “Thanks to the late Minister of Public Instruction, who granted a subsidy for this purpose, the Faculty of Medicine have been enabled to publish a new edition of Laennec’s work upon Auscultation, which has long been out of print. This is intended as a national monument to the illustrious pioneer in physical diagnosis, and it is to be sold at a low price, in order to bring it within the reach of all.”

¹³ *Life*, by Fox Bourne, vol. ii., p. 524.

The old saying seems still to be true, "They manage these things better in France."

Having touched on the *prelude* and the *epoch* of Laennec's discovery, it only remains to glance at the immediate *sequel*, or at what he did and what he failed to do, since to follow up the paths of investigation to which his method has led, to the present, would indeed be a task equally illimitable and interminable.

Time would fail me were I even to attempt an analysis of his work. I can merely notice a few salient observations in the more important chapters, occasionally referring to the corrections and additions of succeeding writers.

Of his chapter on Exploration of the Chest it is impossible to speak too highly. Unlike most authors of new systems, he did not commence with destroying the work of his predecessors; while no mere improver, he yet was not a revolutionist. Not only does he repeatedly urge the value and necessity of the study of rational symptoms, but he avails himself of those physical signs which had been observed from the time of Hippocrates by physicians and surgeons, and more especially of the discovery of percussion by Auenbrugger, whose method, he says, acquires a fresh degree of value when combined with auscultation. It must be confessed, however, that in some instances he undervalues the more ancient methods, as in the case of measurement and of palpation, and he

evidently was unaware of the diagnostic value of vocal fremitus as perceived by the hand. His observations on percussion are minutely accurate; take for example the distinction he draws between the sound elicited, and the tactile sense of elasticity or the contrary. The same may be said of his account of the stethoscopic signs, of the felicitous comparisons by which he describes them, and his explanation of the physical conditions indicated by them.

One serious oversight he made which had important results, namely, the non-recognition of the friction sound produced by the exudation of lymph on the serous membranes of the pleura and pericardium. Had he recognised this, he would probably not have left the diagnosis of pericarditis unwritten; but he seems to have considered friction murmur to be caused by interlobular emphysema exclusively.

His chapters on the different forms of Catarrh are excellent—that on dry catarrh especially so. His original description of dilatation of the bronchi deserves the eulogy of Trousseau, who pronounced it to be “complete, although probably thrown off at the first dash.” He first gave a complete account of emphysema of the lungs, but erred in his explanation of the mode of genesis, as demonstrated by Dr. Gairdner,¹⁴ and perhaps

¹⁴ “On the Pathological Anatomy of Bronchitis and the Diseases of the Lung connected with Bronchial Obstruction;”

also in regarding *dry crepitous rattle* as the pathognomonic sign of the interlobular form.

His chapter on Pulmonary Apoplexy is, as he himself states, original, and contains some valuable observations, *e. g.* the occasional occurrence of bellows murmur in the heart and larger arteries; the rise of temperature, diagnostic of the super-vention of intercurrent pneumonia, and the fact that in this, as in some other cases, the abstraction of blood by leeches seems sometimes to excite hæmorrhage. He does not notice the accentuation of the second sound of the pulmonary artery present in this disease and in phthisis, as well as in mitral sthenosis. Dr. Balfour is the only writer who, so far as I am aware, attaches its due importance to this valuable sign in pulmonary as well as cardiac cases. It is also noticed by Dr. Da Costa in his article on "Blowing Sound in the Pulmonary Artery" (Latham's murmur)—*American Journal of Medical Sciences*, January 1859. Of course, Laennec had no knowledge of the connexion between hæmoptysis and the hæmorrhagic infarction caused by thrombus in the branches of the pulmonary artery, nor does he appear to have recognised the source of hæmorrhage in aneurisms of these arteries in the latter stages of phthisis, so fully described by Rasmussen and also by the late Dr. Cotton, and Dr. Peacock, also papers in *Medico-Chirurgical Review* for April and July, 1853, and *Clinical Medicine*, pp. 438-53.

but first observed and recorded in the *Lancet*, of February 6th, 1841, by the late Mr. Fearn, who sagaciously anticipated that it would be found to be a not unfrequent cause of fatal hæmoptysis.

In his chapter on Pneumonia he gives what may be considered a fairly complete anatomical description of the ordinary sthenic, croupous form of the disease.

To his account of the physical signs I would take one exception, namely, the attributing to the crepitating râle the character of a pathognomonic sign of the first stage. His enumeration of symptoms needs only the addition of the altered pulse ratio (the importance of which was, I believe, first insisted on by Dr. Walshe), and more thorough information regarding the temperature, the peculiar *sensible* character (the pungent heat) of which was first noticed by Dr. Addison, while Wunderlich and others have shown that the value of the thermometer is second only to that of the stethoscope in the study of this disease.

I question if he over-estimates the good effect of tartar emetic in the sthenic form of the disease, and I do not know a more plausible theory of its mode of action than his own, namely, by "promoting interstitial absorption." He bled freely, as was the fashion of the day, and may be perhaps the fashion of the future — only that we know better *why* we bleed, not to cure pneumonia, but to relieve congestion and avert im-

pending suffocation; urgent dyspnœa being, as Dr. Wilson Fox observes, the only positive indication for this remedy, with the exception of a very high amount of pyrexia in the early stages.¹⁵

Laennec recognises, though briefly, some exceptional forms which succeeding writers have described more fully, such as that prevailing during an epidemic of influenza in which hepatization did not occur, or was long delayed—a form corresponding apparently to the blue pneumonia described by Sir D. Corrigan and by Dr. Gordon. He also refers to an epidemic form which corresponds to the pythogenic pneumonia described by Drs. Grimshaw and J. W. Moore; and to an outbreak of it, described by Dr. Bryson, which occurred on board some of the ships of the Royal Navy in 1860, due to ochlesis generated by over-crowding, and becoming communicable by contagion.¹⁶

His opinion, that abscess is a comparatively rare termination of pneumonia, is generally received, and the same remark applies to gangrene, of which he says: “It can scarcely be ranged among the terminations of the pulmonary inflam-

¹⁵ Art. Pneumonia in Reynolds' *System of Medicine*, vol. iii., p. 702. Dr. Walshe, however, goes farther when he says:—“Clinical observation has more than once led me to at least strongly surmise that active congestion may be prevented from reaching the exudation stage by a well-timed abstraction of blood.”—On *Diseases of the Lungs*, 4th ed., p. 369.

¹⁶ See Note C.

mation, and still less can it be considered as the consequence of its intensity It would, on the contrary, seem in most cases to approach the nature of the idiopathic gangrenes," &c. His division into the two forms of uncircumscribed and circumscribed gangrene has been adopted by subsequent writers as being well founded.

I do not think he mentions that occasional result of pneumonia described by Rokitansky under the name of indurated hepatization.¹⁷

Of complications Laennec refers especially to the cerebral and biliary. The former he ascribes to determination of blood to the head; but as he remarks that it occurred in old persons, and ran into the third stage in a few hours, one would be inclined to think the "coma" was probably due to uremia, in which complication Dr. M'Dowel has shown that pneumonia possesses the tendency to run rapidly into purulent infiltration.¹⁸

Of the biliary complication I may remark, that the acute yellow softening of Rokitansky is an occasional and invariably fatal complication of pneumonia.

His chapter on the different forms of Pleuritis is full of valuable observations. I can notice but a few :—

¹⁷ *Pathological Anatomy*, vol. iii., p. 81, Syd. Soc. Edit.

¹⁸ On the Connexion between certain Forms of Pneumonia and Renal Disease.—*Dublin Quarterly Journal*, vol. xxi., p. 322.

Dry pleurisy he regards as usually a mere complication of some more serious disease, as pneumonia or phthisis. He remarks on the mistake of supposing that pleuritis terminates by effusion, whereas it (the effusion) may occur in the course of a few hours from the attack. He anticipates the late Dr. Greene's observations on vicarious purulent expectoration in empyema.¹⁹ He notices the frequent confusion of chronic pleuritis with phthisis. He lays down judicious rules for paracentesis, and advocates an early operation, on the ground that compression of the lung against the spine renders the success of the operation at a later period doubtful. He observes on the sanguinolent nature of the effused fluid in cancerous pleuritis—a fact often noticed in Dublin, and fully confirmed by Dr. Bowditch, who states that, in his extensive experience, sanguinolent fluid, when following the first puncture, was almost certainly fatal, and a consequence of some malignant disease of the lung or pleura.

No chapter in Laennec's work displays more patient and original research than that on Pulmonary Phthisis; but none has been more severely criticised by two classes of pathologists, viz., by eminent clinical physicians, and histologists. For a statement of the views of the former I may refer to the second chapter of Dr. Williams'

¹⁹ *Dublin Medical Journal*, vol. xvii. (first Series).

work on Consumption, and to the writings of Drs. Addison, Graves, Andrew Clarke, and Niemeyer.²⁰

It is in the researches of the histologists, however, that Laennec has met his Nemesis for the depreciation of microscopic investigations—one of the many instances proving how unwise are those vaticinations of future failure and of finality in science which seem to say to the ever-advancing tide of discovery, “Thus far shalt thou come and no farther, and here shall thy proud waves be stayed.”

Virchow and Reinhardt in Germany, followed by many others in England, have initiated a controversy which, like its subject, might, to use the expression of Dr. Wilson Fox, be termed *illimitable*: which has led to much original microscopical research, and many doubtful experiments on those quasi-scrofulous animals, guinea-pigs and rabbits, and also to a most interesting and valuable discussion in the Pathological Society of London,²¹ in which Laennec's views, with slight modification, were advocated by Drs. Wilson Fox and Moxon—namely, that all phthisis is tuberculosis—W. Fox holding that caseous pneumonia is pneumonia running a particular course in the presence of tubercles, and Moxon maintaining that the “anatomical characters of phthisis, as seen

²⁰ See Note D.

²¹ *Transactions*, vol. xxiv.

by the unaided eye, are positively sufficient to separate it from all other pulmonary diseases, and to gather all phthisical cases into one natural group, practically coinciding with the tuberculous phthisis of Laennec and Louis." Dr. Bastian, on the other hand, considered these views so erroneous, that he would expunge the word tubercle from the medical vocabulary; while Dr. Williams maintained the views published in his valuable work on Consumption, namely, that in inflammation of the adenoid tissue, the general result and future history of the inflammatory product are determined by its composition and vital endowments; and that it is by a scrofulous type of inflammation of the adenoid tissue that miliary indurations are developed, tending to caseation, softening and spreading, and the formation of cavities.

To those who would wish to see an able résumé of the opinions of the numerous British and foreign writers on this vexed question, I would recommend a perusal of Dr. Foote's Lectures on Tuberculosis.* I would also direct attention to an exhaustive and argumentative essay by Dr. Finny, on "Pulmonary Hæmorrhage a Cause of Consumption" in the same journal for May, 1873, in which several cases are detailed in support of the doctrine

* *Dublin Medical Journal* for May and July, 1877.

of Phthisis ab Hæmoptoe. All, however, occurred in patients of a strumous diathesis.

A review of the controversy shows the balance of opinion to be in favour of Laennec's views, with little modification, since not only the eminent pathologists to whom I have referred, but others so distinguished as M. Charcot and Rindfleisch, maintain their substantial accuracy—the former stating, that to his mind, “Nothing is better established than the existence of infiltrated or discrete tubercle, as a fundamental element in the different forms of pulmonary phthisis; on the other hand, nothing is more doubtful than the existence of caseous pneumonia independent of tuberculosis, and constituting the prime agent in the phthisical process”; while the other, in his article on Tuberculosis, in Ziemmsen's *Cyclopædia*, asserts the similarity and close connexion of scrofulous infiltration and tubercle, and the mutual relations between scrofula, scrofulous inflammation, and tubercle, maintaining that, “like certain animals—guinea-pigs, rabbits, and monkeys—scrofulous persons only need to have some local inflammation set up, in order to become tubercular. The tuberculosis depends on the resorption of the individual's own inflammatory products.” Rindfleisch thus approaches to the views so strongly expressed by Graves in the second edition of his *Clinical Lectures*, as well as to the opinion of Laennec, that tubercle

and the matter of scrofulous glands are absolutely identical. (Forbes' translation, p. 328).

But, after all, the wonder is not that Laennec's pathology should be questioned, but rather that he should have grasped so much of the truth—if not the whole truth—for, as Mr. Buckle rightly observes, “the philosophic pathologist is as different from a physician as a jurist from an advocate, or an agricultural chemist from a farmer. The two sets of functions may be united, and occasionally, though very rarely, they are, but there is no necessity for their being so.”

They were so united in Laennec, however, but in different degrees. He was a pathologist before his discovery; but ever after, his pathology was made subservient to the great object of his life—the improvement of clinical medicine by the application of physical diagnosis. To justly estimate his merits and the influence of his work, we should realize the truth, that “the progress of every science is affected more by the scheme according to which it is cultivated, than by the actual ability of the cultivators themselves”; and as the writer I have quoted justly remarks, “whoever has reflected much on the different stages through which our knowledge of biology has successively passed, must be led to the conclusion that, while fully recognising the great merit of the (microscopic?) investigators of the animal frame, our highest admiration ought to be re-

served not for those who make the discoveries, but rather for those who point out how the discoveries are to be made.”²²

The second part of Laennec's work, though marked by great ability and originality, is confessedly inferior to the former, as also to the mass of the observations of subsequent writers. And first we observe that while his explanations of the physical signs of the lungs may be said to have received few corrections, his theory of the sounds of the heart in the healthy and diseased states has been proved to be erroneous. Starting from a sound principle, that the ear judges more correctly of the intervals of sound than the eye of the intervals of motions corresponding to these, he yet vitiated his conclusions by ascribing to the contraction of the auricles the sound produced by the sudden tension of the semilunar valves. To estimate the important consequences of this error, which could scarcely have been committed had Laennec borne in mind the hydrostatic law of the equal pressure of fluids, we have but to recall to mind the phenomena of a case of aortic patency which Laennec seems not to have observed, but which a very few years after was so thoroughly investigated by Sir D. Corrigan, in a masterly memoir, which threw much light on the pathology and treatment, not only of this, but also of other

²² *History of Civilization*, vol. 1, p. 819. The same idea is expressed differently by Bacon. See Note E.

diseases of the heart, showing more especially the true conditions indicating the use of digitalis in cardiac affections.²³

Other disciples of Laennec examined and corrected his theory by experiments on living animals, and by clinical observation; notably Drs. Hope and Williams, and I would add Dr. Billing and Mr. Bryan, whose researches have scarcely been appreciated as they deserve.²⁴

That the subject of the heart's motions and sounds continues to occupy the minds of eminent physiologists and pathologists in this and other countries, we have proofs in the most recent works on physiology,²⁵ in the monographs of Walshe, Balfour, and Hayden, and in the contributions of clinical observers and experimenters in the periodicals of the day.

Laennec's erroneous explanation of arterial murmurs has been amply corrected by the experimental researches of Williams and Corrigan, while the mistake he made of locating the continuous murmur heard in the neck in the carotid arteries was long since corrected by Dr. Ogier Ward, who conclusively proved that it is seated in the veins. Another erroneous explanation is that of the genesis of a loud musical murmur audible at some distance from the patient, since

²³ *Edinburgh Medical and Surgical Journal*, April, 1832.

²⁴ See Note F.

²⁵ *Vide* M. Foster's *Text Book*, 2nd ed., chap. 4.

proved by the clinical observations of Drs. Banks, Stokes, and Corrigan to be due to the presence of a vibrating tongue in the heart or aorta.²⁶

With regard to the application of physical diagnosis to the diseases of the heart, Laennec has been accused of an amount of confidence which is not justified by its results in his own hands. But such an accusation does not appear to be warranted by his language, for he not only rather over-estimates the difficulty "of the study of the heart's actions in health," but while maintaining the insufficiency of the general symptoms—which he has sketched with great terseness and power, but without any attempt at differentiation—to characterise or indicate disease of the heart, and the consequent necessity of recourse to mediate auscultation, he adds, that "even it more frequently fails in this case than in any of the other diseases which it is calculated to discover." And again, that in these diseases "we shall be most liable to commit grievous errors in diagnosis, more especially if we restrict our exploration to a few minutes, and fail to take into account the general symptoms and the diseases which may complicate those of the heart."²⁷

Of the additions needed to be made to Laennec's description of diseases of the heart, the

²⁶ *Dublin Hospital Gazette*, February 1st and 15th, 1857, and Stokes, *On Diseases of Heart and Aorta*, p. 139.

²⁷ See Note G.

most remarkable is the case of pericarditis. We cannot realize his admission that the stethoscope scarcely furnishes us with any certain signs of this disease, now, when the contrary is so generally acknowledged. But for many years after Laennec the general belief, as enunciated by Andral and others, was that the diagnosis of pericarditis could only be arrived at indirectly and by a process of exclusion. It was reserved for Dr. Stokes to expound so fully the physical diagnosis of this disease, as to leave "what had been the most difficult the most easy of detection in medicine" (Sibson, *Med. Chir. Rev.*, July, 1854). True, the friction murmur had been noticed by Collin, and Watson, but these were scattered rays converging to, and concentrated in a burning focus in the exhaustive memoir of Stokes.

Perhaps no chapter in Laennec's work is more worthy of admiration than that on softening of the heart, as occurring in fever.²⁸ His description of the colour and consistence of the organ; of the analogous condition of other muscles; of the diminution of the heart's impulse and occasional loss of its sounds; of the slowness of its action and occasional fits of excitement; of its non-inflammatory nature, and of the indications for wine, which, he says, "is indicated in case the affection supervenes to a severe fever, *and if the patient bears it well*"; and of its curability in con-

²⁸ See Note H.

valescence under tonic treatment, are excellent; and I may add that he evidently recognised the co-existence of softening and hypertrophy (the failing hypertrophy of Fothergill), although ignorant of the fact, since demonstrated by Rindfleisch, that this febrile softening is a true fatty degeneration. I need not remind my hearers how these observations, together with those of Louis, suggested the admirable researches of Dr. Stokes, and led to the placing the indications for the exhibition of wine in typhus upon a sound and certain foundation.

On the great subject of fatty degeneration of the heart, Laennec's claims to originality have been ignored by some writers, while others, as the author of the article in Ziemssen's *Cyclopædia*, have fully recognised them. He was, in fact, familiar with both forms,—fatty infiltration and fatty degeneration,—the latter of which he defines to be an actual transformation of the muscular substance, into a substance possessing all the chemical and physical properties of fat, thus anticipating the idea which Dr. Quain afterwards developed and proved to be a fact by the original and elaborate experiments detailed in his memorable essay in the *Medico-Chirurgical Transactions* (vol. xxxiii.), researches of which it has been said: — “Dr. Quain has candidly referred to many previous observers by whom similar changes were recognised, but the honour of the

full proof and of the right use of it belongs to himself alone."²⁹

Perhaps no subject in Medicine has since occupied a larger share of the attention of the profession than this, as is proved by the contributions of numerous English and Continental writers; the recorded cases in the Transactions of the Pathological Societies of London and Dublin; the monographs of Walshe, Balfour, Fothergill (who gives a masterly description of the general symptoms);³⁰ of Hayden, who has carefully collected and tabulated the cases reported since Dr. Quain's memoir, with the addition of many of his own; and of Stokes, who first, I believe, expounded the differential diagnosis of degeneration of the right and left cavities, based upon the preponderance of respiratory or cerebral secondary affections.

Not to follow this review farther, I may remark that a careful perusal of these chapters will disclose some observations and suggestions worthy of notice; such, for example, as his account of the signs of contraction of the mitral opening (pp. 647-48), as his observation, that "the severest diseases of the heart consist in defects of proportion, and yet a slight disproportion between it and other organs, or between some of its own constituent parts, is compatible with a

²⁹ Paget, *Surgical Pathology*, vol. i. p. 107 (1st edit.)

³⁰ *The Heart and its Diseases*, 2nd edit.

state of health,"—that vegetations on the valves are not solely due to inflammation, but partly to accretion of fibrine—that dilatation of the right ventricle without hypertrophy is of rare occurrence—that digitalis is of no benefit in hypertrophy—that simple hypertrophy without dilatation is much more rare in the right than in the left ventricle—that intermission of the pulse is often due to the alternate feeble contraction of the ventricle,—that the capillary circulation is in some sort independent of the general, a fact now recognised by physiologists;³¹ and—not to multiply instances—his theory of angina pectoris, of which Dr. L. Clarke says, "Laennec's view of the proximate cause has been materially supported by recent enquiries."³²

What has been Laennec's influence on practice, on clinical teaching, and on the public estimate of Medicine? By his introduction of differential diagnosis—the essential pre-requisite of scientific therapeutics—the treatment of diseases of the chest, previously directed to a name, a group of symptoms, or often to a single symptom supposed to be pathognomonic of a certain affection, has become differentiated: and a case of pneumonia is no longer treated as one of pleuritis; nor one of diseased heart,

³¹ See Pettigrew, "On the Physiology of the Circulation," *Edinburgh Medical Journal*, vol. xvii., 1872.

³² See Note I., *Pathological Society's Transactions*, vol. i.

or of emphysema as one of hydrothorax, the universal practice of the preceding era. Auscultation also, by enabling us to recognise the tendency of diseases to recovery, or the contrary, becomes a powerful adjuvant in their treatment. Moreover, we have in the recognition of the vital condition and innervation of the heart a most valuable guide to the treatment of various diseases, notably with regard to the exhibition of wine in fever, and the treatment of the cerebral anæmia of fatty heart and of other cerebral affections; while in the field of preventive medicine, auscultation co-operates by detecting the germs and vestiges of disease, as for example the early signs of phthisis, or the organic sequelæ of acute disease.

With regard to Laennec's influence on clinical instruction, we might well ask what would such instruction be, if physical diagnosis were omitted? Without it, the knowledge gained at the bedside would be mere cramming. "*Teaching*," says Dr. Moxon, "is the storing of knowledge—it may be done quickly. *Training* is the creation of an organ for use of knowledge—it needs much time, it is a slow process. The trainer has to convert the pupil's knowledge into motive, his desire into patience, his will into skill. Every good trainer aims to raise up in the pupil's mind a *self-training faculty*, which shall itself continue to train, more and more, knowledge into motive. By such train-

ing knowledge becomes power.”³³ This is exactly what the study of physical diagnosis does. Following the method of Laennec, the clinical teacher trains the pupil to the use of his senses in the observation of sensible phenomena, then to the associating these with the physical conditions from which they arise, and finally to their relations with principles of pathology and of treatment. No greater improvement has ever been effected in medical education than by the introduction of this method into hospital teaching by the late Dr. Graves, as described in his work on Clinical Medicine.

It were more easy to say what should be than what is the influence—indirect, of course—of Laennec on the public estimate of Medicine. This, we know, has never been high, while physicians have been the objects of the satire of wits for centuries. A hundred years ago Zimmerman wrote:³⁴—“I sometimes hear pretended wits affecting, with a tone of raillery, that Physic is still such as it was in the time of Hippocrates, and that the best informed physicians know only that which he knew.”³⁵ Nor is our art better

³³ *Hunterian Oration*, 1877.

³⁴ *On Experience*, vol. i. p. 174.

³⁵ Molière's caricature of the physicians of his day is familiar to all, and Montaigne declares that he would choose a physician who should first have passed through all the diseases he pretends to cure, adding :—“For the others only

appreciated by certain philosophers, for we have the question gravely propounded by Sir William Hamilton—"Has the practice of Medicine made a single step since Hippocrates?"³⁶ and the late Dr. Symonds, in a Presidential Address on this subject—since published in his "Miscellanies"—referred to passages in two leading reviews¹ of the day, too long for quotation, both representing Medicine as uncertain, always changing, resting on arbitrary assumption, &c. One oracle asks:—"But can any one at this moment seriously declare that there is such a thing as a science of Medicine?" "What there is is this: there are a few facts—a very few—distinctly known and beyond the reach of controversy, and the number of these increases but slowly, if it increases sensibly at all."

Our answer to such nonsense is to point to what Laennec has done. Is the influence of Medicine in directly saving life called in question, we may refer to the physical diagnosis of cases which, without medical or surgical inter-

guide us like him who paints the sea, rocks, and ports, and draws the model of a ship as he sits at his table; but send him to sea, he knows not what course to steer. They make such a description of our maladies as a town-crier does of a lost horse or dog—of such a colour, such a height, such an ear, but bring the animal to him and he knows him not for all that."—*Essays*, vol. iii., p. 390.

³⁶ "Review of Thompson's Life of Cullen."—*Edinburgh Review*. See Note K.

ference, run on inevitably to a fatal termination, *e.g.*, of empyema, or of a foreign body in the bronchus; to the advantage of the early detection and treatment of phthisis, or of certain diseases of the heart, or aneurism of the aorta.

In regard of that power of prognosis which Hippocrates recommended physicians to cultivate, with a view to securing the confidence of those who consulted them, I need not remind you of what we owe to Laennec; and in the detection of latent disease in the numerous cases, in which the aid of the physician is sought by insurance companies or governmental departments, from whom do we obtain help so largely as from Laennec, either alone or in conjunction with Bright? If, then, we challenge the public estimate for our profession as a science, it is to Laennec we owe much of the grounds of our claim; and if it be said that the art is conjectural, we reply that to a portion, at least, he has contributed a degree of certainty almost amounting to law.

“As soon as any department of knowledge,” says Mr. Buckle, “has been generalized into laws, it contains either in itself or in its applications three distinct branches: namely, inventions, discoveries, and method.”

Laennec combined all these. His invention was as perfect as simple, when tried by the test for such—“will it work?”—his discoveries have led to others, only second in importance to his

own, by those who have adopted his method. Of his great predecessors in the history of Medicine he, perhaps, most resembled Sydenham, who, says Dr. J. Brown,

“Did for his art what Locke did for the philosophy of mind, he made it in the main observational They were among the first in their respective departments to show their faith in the inductive method by their works They pointed out a way and walked in it; they taught a method and used it, rather than announced a system or a discovery; they collected and arranged their *visa* before settling their *cogitata*—a mean-spirited proceeding, doubtless, in the eyes of the prevailing dealers in hypotheses, being in reality the exact inverse of their philosophy.”

He was not a generaliser like Cullen, and scarcely deserves the eulogy of Sir Wm. Hamilton on that great man, whom he seems to praise for that “he had not made the discovery of a single phenomenon.” He did not undervalue theory, but justly estimates its value when he says:—

“No doubt, it would be better if we could dispense with all theory, but this is impossible: the numerous and diverse facts which constitute the science of physic can only be classed in the memory by the aid of some systematic bond. It is indeed much to be desired that less importance were attributed to views which after all can only be considered as the scaffolding of the science; and more especially is it to be wished that the attachment to theory would not lead many persons (as it does) to reject the very facts on which other theories, whether ancient or modern, hostile to their own, are founded.”

While no mere collector of facts, he reasoned,—

not downwards from general principles, but in the contrary and safer direction; and having seen and seized on the “enlightening fact,” and pursued what Bacon calls the “*experimenta lucifera*” through a lengthened series of observations on the living and on the dead, he developed and has bequeathed to us a method “comprehensive and exact” in its induction, which, however it may hereafter “be further polished and illustrated, and accommodated for use and practice,” can never be dispensed with in the present, or superseded in a future age of Medicine. To him we may fitly apply Cowley’s well-known lines on Bacon:—

“From these and all long errors of the way,
 In which our wandering predecessors went;
 And like the old Hebrews many years did stray,
 In deserts but of small extent;
 Bacon, like Moses, led us forth at last,
 The barren wilderness he past,
 Did on the very border stand
 Of the blest promised land:
 And from the mountain top of his exalted wit,
 Saw it himself and showed us it.”

Allow me to trespass on your patience for a moment longer, while I express my personal regret for one whose recent death I may truly say is deplored by the entire profession; and whose loss to this Association may be judged of by this, that his last public act, so far as I am aware,

was in reference to the arrangements for the present meeting.

On reading the short biography accompanying the announcement of his death in the *British Medical Journal*, I was struck by the resemblance of his career to that which I have feebly attempted to sketch.

Like Laennec, Dr. Murchison was a diligent and highly distinguished student; he, also, early devoted himself to the cultivation of Pathology and Clinical Medicine, and became a great Clinical teacher; like Laennec, he compressed the labours of a long life into a comparatively short period, and like him, his name will be ever associated with a great work—a work which must live, inasmuch as it is not only the most comprehensive and exhaustive which has ever appeared on the subject, but also marks the epoch of that sound and scientific classification of fevers which the late Dr. Parkes, in his admirable Address in 1873, pronounced to be probably “our greatest advance in practical Medicine.” That advance, mainly due to the researches of Stewart and Jenner, has been secured, and rendered permanent, and illustrated, by the great work of Charles Murchison.

APPENDIX.

NOTE A, PAGE 8.

BAGLIVI would seem to have borrowed the idea, if not the language, of Bacon when, after arguing that to limit medicine by systems, and to elaborate and ornament it by certain commentaries entirely abstract and useless, is to be deprecated as retarding its progress, "since this methodical treatment may make the art appear, as it were, complete and reduced to limits beyond which it could not progress, and extending moreover to the rejection and contempt of new and profitable truths because irreconcilable with the prevailing system," he adds :

"Quamobrem prudentiùs se gessisse mihi videntur majores nostri. Hi non ignari Medicinam methodis et partitionibus conclusam illustrari et nobilitari posse crescere tamen et confirmari non posse ; quicquid diuturnâ observatione in morbis detegebant describebant illud aphorismis, sive per sententias quasdam breves, et contractas nullis methodi et scholasticæ subtilitatis legibus devinctas, sed liberè expositas notatasque : quo pacto indicarent Posteris nuda simulacra rerum inventarum, et indicarent pariter spatia vacua ad novas et perpetuò multiplicabiles naturæ voces inferendas." (Cap. IX. Impedimentum VI.)

Baglivi was not singular in his condemnation of the prevailing tendency to dogmatism, or of the elaborate descriptions of disease by systematic writers ;

(of men, "who," said Locke, "find it more easy and more natural to *build castles in the air of their own than to survey well those that are on the ground*"); Sydenham also ridicules those descriptions of disease "which the manufacturers of 'Bodies of Medicine' make up in their studies, and which are oftener compositions than portraits, or at the best bad copies."

But it must be confessed that reverence for the ancients had an injurious effect on the practice of these men. The writings of Sydenham alone furnish proof that much of the therapeutics of the day was founded on the false and fanciful theories of Galen rather than on the true physiology inaugurated by Harvey. On this subject Morton, a distinguished contemporary of Sydenham, wrote:

"Non possum satis mirari ipsam etiam medicinæ praxin non pari passu cum ejus theoriâ gradum suum promovisse; præsertim verò priscorum indicationes, methodumque curandi falsis antiquorum theorematis innixam, etiam hodie inter plurimos medicos obtinere. Quo factum est ut *Erasistratus* in *Fontanelli* quinto Dialogo mortuorum priscorum cum recentibus non minùs verè quam sarcastice *Harveio* magno reponeret: 'Vos magis estis physici, at peritiores indè medicos evasisse inficior, ægros enim eodem ac vos successu sanavimus.'"

That Morton was himself by no means free from the false theory and polypharmacy of the day appears from the numerous formulæ of "*Medicamina incras-santia et lubricantia*" scattered throughout his *Phthisiologia*, while, on the other hand, his hygienic or preventive treatment is most judicious. Take for example his moral prophylaxis:

"Ab iracundia, mæstitia, intensa cogitatione, atque omnibus aliis gravioribus animi pathematis summo opere divertatur æger, idque non tantùm monitis et consiliis, verum etiam societate et jucundissimorum amicorum consuetudine. A studiis etiam et vigiliis intempestivis diligenter sibi caveat, somnum semper (quantum possibile est) primâ noctis parte capessendo."—*Phthisiologia*, chap. 8.

NOTE B, PAGE 17.

That there is some foundation for such a fear as was expressed by Laennec would seem to be the opinion of not a few thinkers. Mr. Buckle remarks on the tendency of the physical philosophers of the present day to "display an inordinate respect for experiments, an undue love of minute detail, and a disposition to overrate the inventors of new instruments, and the discoverers of new, but often insignificant facts." . . . "The magnificent generalizations of Newton and Harvey could never have been completed in an age absorbed in one unvarying round of experiments and observations. We are in that predicament that our facts have outstripped our knowledge and are now encumbering its march. The publications of our scientific institutions and of our scientific authors overflow with minute and countless details which perplex the judgment and which no memory can retain. In vain do we demand that they should be generalized and reduced into order. Instead of that, the heap continues to swell. We want ideas and we get more facts. We hear constantly of what nature is doing, but we rarely hear of what man is thinking."—*History of Civilization in England*, vol. ii. p. 503.

"It is high time," says Brudenell Carter, "that the intelligence of mankind should assert itself in opposition to the pretensions of sham science. The tendency of the day is to exalt what is technically called research as opposed to ratiocination, and one consequence of this tendency is that a number of otherwise unemployed and unappreciated persons set themselves to work with microscopes and test tubes, and fancy

that they are making discoveries. The laborious trifling of six months is then described as a 'research.'"
—*Contemporary Review*, January, 1879.

Of such persons Ruehle sarcastically remarks, "It seems to me that the tone which our younger writers assume, as if by their arduous labours they had attained a position which entitles them to ignore the merits of such men as Bayle, Laennec, and Louis, and to look down compassionately on those of us who take any notice of such antiquated greatness, may be explained by the simple fact that they have never read the works of these authors."—Ziemssen's *Cyclopaedia*, vol. v. p. 596.

NOTE B A., Page 18.

My friend Dr. Duffey has kindly drawn my attention to a memoir of our author by Dr. A. Chereau in the *Archives Générales de Médecine*, July, 1879. From this we learn that Laennec inscribed his name as a student of medicine at La Charité in November, 1801. "At that epoch," says Dr. Chereau, "the School of Paris was divided into two great divisions, which formed two separate camps There was the School of Pinel, or of the Salpêtrière, and the School of Corvisart, or of La Charité The first called itself the School of Philosophical Medicine, almost ridiculed inspection of dead bodies, did not rest its diagnosis on any physical means, while its therapeutics was almost insignificant. Its favourite method was analysis. It divided and sub-divided diseases, ranged them in classes, orders, genera, various species, and according to it true medicine was founded on the

principles which govern the study of natural history. Its axiom was this : 'being given the disease, determine its true characteristic and the class it should occupy in the nosological chart.'*

"The other School professed the worship of the Hippocratic traditions. Its great method was observation ; it was humorist within certain limits, and believed in crises and critical days, but it acknowledged the progress of science, and promptly accepted new facts if well observed, and new and well-tried measures. Faithful to the magnificent general plan of medical instruction elaborated by Fourcroy and Thouret, the School of La Charité has the imperishable honour of having created clinical medicine, and of inspiring those men of great talent who have given such a lustre to that memorable epoch in our scientific history.

"Laennec did not hesitate between these two programmes. He chose La Charité. His genius led him there, the affection with which he regarded Corvisart kept him there."

The opposing Schools of Pinel and Corvisart were subsequently replaced by those of Broussais and Laennec, whose personal and mental characters and doctrines are graphically sketched in a passage too long for quotation.

The spirit of Laennec's teaching is thus epitomized :—

"Doué d'un esprit éminemment observateur et d'un jugement aussi prompt que sûr, versé dans l'étude de l'anatomie proprement dite, qui était à cette époque cultivée avec tant d'ardeur à l'école de Paris, Laennec

* "Pinel's definition of medicine," says Claude Bernard, "was : For a given disease find its place in a nosological classification."—"Lectures," *Med. Times and Gazette*, vol. i., 1860.

regarde l'anatomie pathologique comme le flambeau le plus sûr qui puisse guider le médecin soit pour reconnaître les maladies, soit pour guérir celles qui en sont susceptibles : pour lui l'anatomie pathologique est une science beaucoup plus certaine, et présente des objets d'étude plus distincts que la nosologie symptomatique ; l'altération des organes est ce qu'il y a de plus fixe, de plus positif et de moins variable dans les maladies locales ; c'est de la nature et de l'étendue de ces altérations que dépend toujours le danger ou la curabilité de ces maladies ; c'est, par conséquent, ce qui doit les caractériser ou les spécifier. Le trouble des fonctions qui accompagne ces altérations est au contraire extrêmement variable ; il est le même sous l'influence de causes tout à fait dissemblables et, par conséquent, il peut rarement servir à faire distinguer des objets même très-différents En un mot, presque toute la doctrine de Laennec repose sur la pathologie *organique*, tandis que celle de Broussais se baptise du nom de pathologie *physiologique*. Broussais est un penseur profond s'élevant jusqu'à la recherche des causes finales, s'occupant peu des détails minutieux de l'observation. Laennec est un profond observateur, pour lequel tout est dans cet axiome de Bacon : *Ars medica tota in observationibus*. Tels furent les deux hommes si bien faits pour se compléter l'un par l'autre, qui se trouvèrent en présence. Un colosse et un pygmée. . . . Eh bien ! dans cette lutte, en apparence, si inégale, ce fut le colosse qui tomba, non pas tant dans le présent, que dans la postérité."

Dr. Chereau gives the following account of the accident which led up to the subsequent discovery of the stethoscope.

"Un jour qu'il traversait la cour du Louvre, il aperçut des enfants qui, l'oreille appliquée aux deux extrémités d'une longue poutre, s'amusaient à se transmettre réciproquement le léger son provenant de choc du doigt contre le bout opposé. Dans l'espace intermédiaire aucun bruit n'était perceptible. L'habile observateur réfléchit, et bientôt, comme Archimède, il put s'écrier : *j'ai trouvé*."

How this new aid to diagnosis was received, Dr. Cherrau tells us :—

"Laennec avait au reste quelques raisons pour attacher une certaine importance au nom à donner à son enfant chéri. A peine sa découverte fut elle connue qu'elle excita l'attention générale, et que la majorité des médecins s'empressa d'accueillir un moyen qui promettait d'éclairer le diagnostic d'un ordre important de maladies. Mais dans notre vieille Gaule on rit

de tout ; l'instrument explorateur fut ridiculisé, caricaturisé, burlesquement versifié. Les rieurs avaient beau jeu avec *pectoriloque*, *thoraciloque* ; *stethoscope* leur ferme la bouche."

How it was received by the older physicians of other countries, some of us remember, and how its author was misrepresented and undervalued, let a very few examples show.

Armstrong, the most popular lecturer on medicine of his day, says :—

"It very often may be noticed that when any individual makes an important discovery, he is very apt to abuse it, and disregard every other guide. This is the case with Laennec, who holds the common modes of distinguishing affections of the chest from each other in absolute contempt. He is too sceptic as to the symptoms, and uses the stethoscope with all the enthusiasm of a man who has made a discovery, &c."—*Lectures by Rix*, p. 278.

Probably no work on the practice of medicine was more popular in Germany in its day than Hufeland's *Enchiridion*, translated into English in 1844. The following is Hufeland's estimate of auscultation and percussion :—

"The signs afforded through hearing by means of the stethoscope or percussion have recently been highly recommended for the diagnosis of pulmonary diseases. But these signs are very illusory, nor will the existence of an inflammation be ever discovered by them alone, while the other signs are sufficient of themselves for the purpose of diagnosis."—P. 149.

On this subject Dr. Gairdner wisely remarks :—

"It is evident that the invention of physical diagnosis has a tendency to diminish the *apparent* value of symptoms, not by circumscribing the field of their application in diagnosis, but by extending the field of diagnosis taken as a whole. It is also unquestionably true that an undue estimate of the relative importance of physical signs has caused in some minds a neglect of the diagnostic value of symptoms ; which is the more to be regretted as it is far removed from the spirit of Laennec himself, and the best of his successors."—"On modern Practice in Diseases of the Chest," *Brit. & For. Med. Ch. Rev.*, Jan. 1854.

Laennec had his detractors as well as those who disparaged his discoveries. One of them (Bouillaud) seems to have gone so far as to impugn his veracity, alleging, on the authority of a former pupil, that Laennec gives an inaccurate return of the results of his treatment of pneumonia by antimony, that in fact he lost by this method two-thirds of his patients.

This statement is refuted by Grisolle (on the authority of Dr. Meriadec Laennec), who accuses Bouillaud of thus "finding out a new opportunity to exalt his own method and depreciate that of Rasori," and concludes by indignantly declaiming against such an aspersion on the fame of Laennec:—

"J'ai cru devoir réfuter un document que M. Bouillaud a eu le très grand tort d'accepter avec trop de confiance ; car, s'il eût pu être authentique, il eût porté certainement une grave atteinte à la veracité de Laennec. Mais heureusement de pareilles imputations ne sauraient ternir la gloire imperissable d'un des plus grand génies, et disons mieux d'un des hommes les plus utiles que honora jamais l'école française." — *Traité Pratique de la Pneumonia*, p. 623.

I may add that Laennec's statement is confirmed by the experience of Dr. Blakiston, who "ventures to assert that with certain modifications in individual cases it (tartar emetic) is suitable to every form and stage of primary pneumonia."

"Of sixty-one cases thus treated, in which the disease was simple or only complicated with a moderate amount of pleuritis, three died and fifty-eight recovered." — *On Diseases of the Chest*, p. 280.

NOTE C. PAGE 29.

To the accuracy of his description subsequent writers give uniform testimony. Even Skoda, hypercritical as he is, says that "as regards the anatomical conditions of the lungs in pneumonia, they seem to be, at the present time, exactly such as described by Laennec."—*On Auscultation*, translated by Markham, p. 281.

"With Laennec," says Juergensen, "begins a new era in the theory of inflammation of the lungs; his anatomical and clinical descriptions are clear and true to nature. . . . In Laennec's descriptions there may be here and there a statement that is not quite satisfactory, yet, upon the whole, it may be said that he laid the scientific foundation for future investigation in the theory of chest diseases in general, and particularly of pneumonia."—Ziemssen's *Cyclopedia*, vol. v., art. "Pneumonia."

"It is to Laennec," says Dr. Sturges, "that we owe the earliest picture of pneumonia in its anatomical stages; improved means of research have since made evident much that was then obscure, and enabled later observers to define the pathology of lung inflammation more strictly, but the clinical account of pneumonia, although it has been since amplified and refined upon, remains substantially as Laennec wrote it."—*On Pneumonia*, p. 16.

Of the successive stages, as described by our author, that of resolution seems to have been viewed differently by different observers. Laennec states that in resolution of the first stage "occasionally a serous succeeds to the sanguineous infiltration" . . . while in the stage of hepatization "the texture of the part

becomes softer, more humid, and, when cut, exudes more serum than blood," &c. Rokitansky considers this serous exudation an essential element in resolution in every stage. In the second, he says, "the granulations, together with the tissue, gradually become paler, and a serous fluid which is secreted in the cells seems by degrees to cause a fusion of the granulations layer by layer."—*Path. Anat.*, vol. iv., Syd. Soc.'s ed. Buhl, quoted by Juergensen, is of opinion that "the whole of the hepatized portion of the lung is restored to its normal condition almost entirely by means of the absorption of the softened alveolar plugs."

Zehetmeyer's account of the process is similar, but more elaborate ;—

"When we say that a pneumonia is in course of resolution we understand thereby the infiltration of the hepatized part with albuminous serosity, through the agency of which the breaking up of the crude febrinous material is induced, and the formation of pus cells is rendered possible. Without such infiltration there can be no softening and no perfect cure. This imbibing of resolving serosity is effected by means of a new congestion, which occurs the more readily the more the affected parts are surrounded by sanguiferous vessels. This is the explanation of pneumonia always softening first at the periphery, and of the resolution proceeding thence towards the centre. It is the reason why pneumonia so often undergoes purulent softening at its exterior, while the centre remains dry and ill-coloured, and is subject to the necessary transition of the fibrine into casein, as well as the reason why pneumonia of the lower lobes usually runs a quicker and more favourable course than hepatizations of the upper lobes, which have long been a source of dread on account of their known tendency to pass into tuberculosis."¹

On the other hand, Grisolles, while admitting the accuracy of Laennec's description of the resolution in

¹ "Researches on Pneumonia," translated by Dr. F. Battersby, *Dubl. Med. Jour.*, July, 1845.

the first and second stages, expresses his doubt of the generally received opinion that œdema supervenes on pneumonia in these cases. This opinion he founds upon the *post mortem* appearances in a case of death from pericarditis, after resolution of pneumonia, in which no part presented any trace of œdema.

Dr. Wilson Fox, however, states that he "once found, three weeks after the physical signs had disappeared, a considerable amount of œdema remaining in the affected parts, together with a marked loss of elasticity of the tissue." Lastly, Dr. Da Costa says: "If the pneumonia ends by resolution, we have the organ becoming of a paler hue, the granulations lighter in colour, mixed with a freshly-exuded serous fluid which gradually dissolves them, leaving the lung itself more infiltrated with serum, &c."—*Amer. Jour. of Med. Science*, Oct., 1855.

Several considerations, upon which I cannot now dwell, lead me to the belief that antimony "increases the activity of the interstitial absorption" of the concrete fibrine in the hepatized lung (as Laennec supposed) by its power of promoting the exudation of serosity in and around the affected part. I do not know how we can otherwise explain its action in resolving indolent hepatizations of many weeks' duration, after other measures have failed. I have known this to occur in cases in which consolidation had existed for periods of a month, six weeks, and three months. In two of these the patients had become dropsical—a consequence of the hepatization noticed by Rokitansky;—but the anasarca rapidly disappeared along with its cause, under the antimonial treatment.

The form of asthenic pneumonia here referred to

was first described by Sir D. Corrigan in a communication to the Pathological Society of Dublin; several cases being afterwards brought forward by Dr. Stokes. It prevailed epidemically in Dublin in 1841, and again in the years 1855-56-57. In December, 1857, Sir D. Corrigan thus summed up the results of his experience of the disease :—

“Asthenic pneumonia attacks all ages. It proves fatal either directly in the first stage of congestion—in which state it is, indeed, a very fatal disease, the patient dying while the lung is gorged and dark, and from which, on a former occasion of noticing it, I called it blue pneumonia—or it passes from the first into the third stage, scarcely showing at all the second or hepatized stage.

“Instead of being seated, like ordinary pneumonia, in the lower lobe, it is frequently found in the upper lobe. Its symptoms are as peculiar as its pathology. It is not accompanied with the peculiar calor mordax of skin which is so characteristic of inflammatory or sthenic pneumonia, but, on the contrary, the skin is of its natural temperature, or cooler than natural, and the face rather sallow than otherwise. Pain is variable, and appears to depend altogether on the degree of inflammation of the pleura; most generally, however, it is not much complained of, the patient describing shortness of breath as his prominent symptom.

“One of the most remarkable features connected with it is the absence, for several days, of any expectoration, and, even when this does appear, its being very small in amount compared with the extent of local disease, as revealed by percussion and auscultation.

“Bronchial respiration, and very fine crepitating rattle, are the auscultatory symptoms in the disease. I have again, as two years ago, to recommend strongly its main treatment by quinine. The general dose which you see administered for an adult is five grains every third hour; and under its exhibition the pulse becomes slow and steady, the respiration free, and rapidly so. If the patient be young, with evidence of capillary congestion generally over the system, the exhibition is preceded by local depletion; but this is rather the exception. The patient in this treacherous disease often does not seek admission into hospital, nor advice in private practice, until too late, deceived by the absence of pain, of fever, and of expectoration, and feeling merely debility and shortness of breathing, &c.”—*Dublin Hospital Gazette*, Dec. 15, 1857.

NOTE D, PAGE 32.

Although the chief opponents of Laennec are to be found among the pathological anatomists of Germany, the doctrine of the inflammatory origin of phthisis was previously upheld by a series of eminent British physicians, commencing with Dr. Alison and Dr. Abercrombie in 1822-23-24, Dr. Williams (a disciple of Alison) in 1828, and continued through Addison and others enumerated by Dr. Andrew Clark, who well observes "that the light which Germany boasted to have thrown upon the changes undergone by the lung in phthisis was a light borrowed from England, and transmitted back to us through a different and less transparent medium."—*Croonian Lectures*, 1867.

It is due to the memory of Dr. Graves to state that he anticipated Dr. Addison and others in maintaining the frequent independence of phthisis on tubercle, as also in regarding it as a form of scrofula, in which latter doctrine he himself had been anticipated by Morton. In his clinical lectures, published in Ryan's *London Medical and Surgical Journal* in 1832-3, this great physician says :—

"The occurrence and development of tubercles in phthisis, constituting the most remarkable phenomena of the disease, have engrossed almost exclusively the attention of medical men, and consequently they have attached an undue degree of importance to them as the cause of phthisis. . . .

"I look on tubercular development and consumption as the consequence of that particular state of constitution which occasions what is falsely termed tubercular inflammation—a state of constitution in which we have three distinct processes, attended by corresponding morbid changes, each different in itself, but depending on one common cause. Every form of consumption which has hitherto come under our notice is referrible to one common origin, and this is that debilitated state of constitution which has been termed the scrofulous habit. . . .

"Many cases come under our observation, in which most of the symp-

toms of phthisis, and its attendant hectic, are manifest and striking, and, when the injury done to the lung is very great, still no tubercles can be detected. That the mere presence of tubercular matter does not occasion inflammation of any kind may be inferred not only from the lungs, in which this fact is of every-day occurrence, and a matter of every-day observation, but also from finding them frequently in the spleen, liver, kidney, and muscles, where they must have existed for some time, and yet we cannot perceive any inflammation of the surrounding tissues. On the other hand, as we may have tubercles without any phthisical pneumonia or suppuration of the lung, so we may have also the latter without the former. Thus in a man of middle age, who died lately in this hospital, the lungs were extensively solidified, black and ulcerated, containing several sinuous cavities, filled with pus of a scrofulous character, but not a single distinct tubercle. There was not the slightest vestige of the chief kind of tubercle—the yellow one—nor could we find any of the small miliary transparent kind; the whole mass was solid, except where it was suppurating, evidently the result of phthisical pneumonia of a chronic nature. Occurrences such as this have been frequently observed (and particularly in the phthisis of persons advanced in life), by Professor Alison and others; but the preconceived opinion, that the solidification of the lung was the consequence of tubercular disposition, made them overlook its real nature. The most important thing for the student to impress on his mind, with regard to all these cases of phthisis, is, that the pectoral symptoms, of whatever nature they may be, are caused by scrofulous inflammation.

“You will find in the works of Laennec, that he states that bronchitis never hastens the development of tubercles. I must, in the most positive manner, deny the truth of this statement. It is a very dangerous thing for a person of a scrofulous habit to get an attack of cold, producing catarrh, or inflammation of the lungs, as it has a direct tendency to bring on tubercular development and suppuration; if persons be weakly, unhealthy, and of a scrofulous constitution, and get cold and inflammation of the lungs, they are more liable to have consumptive suppuration of the congested than of the other portion of the lung; for the same reason that a simple injury, producing inflammation of the hip or knee joint in a scrofulous habit, may degenerate into true scrofulous ulceration of these parts. Hence common bronchitis in a scrofulous habit may become true scrofulous bronchitis, and common pneumonia may end in the scrofulous consolidation and burrowing ulceration of the lung peculiar to phthisis.”

Probably no subject in the history of medicine better illustrates its tendency to advance in cycles than this; the glimpses of a past age being confirmed

—even if corrected—by the discoveries of the present, and similarly the treatment advocated at one period repudiated in another, to be again revived upon new and sounder principles. Thus the reader of Morton's chapters—"De phthisi scrophulosa," "De phthisi ab-hæmoptoe," "De phthisi à peripneumonia et pleuritide ortâ"—will trace resemblances to the views of some of our most recent authorities. Take for example a passage in his chapter on "Scrofulous phthisis," which he asserts to be far the most frequent form of the disease. After attributing to the spiral distribution of the vessels in glands a tendency to stagnation and to consequent swelling and hardening of these structures, he says:—

"Quod in cæteris glandulosis partibus accidit, in ipsis etiam pulmonibus accidit, qui glandulis undique innumeris verum juxta trachæam atque ejus ramulos magis conspicuis referciuntur, etsi in naturali statu non ità facile percipiuntur, quoprimum non est si scrophulosi, qui tumoribus glandulosi in aliis partibus frequenter obnoxii sunt; non raro etiam ejusmodi tuberculis: vel in ipsis pulmonibus afficiuntur, quorum parenchyma est naturaliter spongiosum et serosas sanguinis partes continuâ eorum agitatione copiosi hic excretas recipere aptum natum. Et quidem ab istis tumoribus in pulmonibus fixis orire solet phthisis ista scrophulosa."

To me it would appear that Morton anticipates the views of Graves, and also of Portal, quoted by Dr. C. J. B. Williams as anticipating Dr. Burdon Sander-son:—

"Pour raisonir maintenant aux tubercles qui constituent la phthisie originaire, je pense, d'après l'examen le plus attentif, qu'ils sont formés et par des engorgemens des glandes lymphatiques, répandues dans presque toutes les parties du poumon, ou loin des bronches, ainsi que par des engorgemens lymphatiques du tissu cellulaire des poumons, lesquelles, après avoir pris une consistance plus ou moins grande, terminent fréquemment par tourner en une mauvaise suppuration."—*Obs. sur la nature et le traitement de la Phthisis pulmonaire.*

The recognition of the part which inflammation plays in the origin as well as in the progress of phthisis has an important influence on its treatment. By ignoring this connexion Laennec was led to repudiate the notion of cure in the early stage, regarding it as out of the power of nature, or of art, to reverse the gradual increase and softening of crude tubercle, and maintaining that cure is only possible after the softening of the tubercles and the formation of an ulcerous excavation.—*Forbes' Translation*, p. 305.

Dr. Stokes, on the contrary, teaches that "the hopes of cure in consumption rest on the recognition of the early stages and the prevention of incurable disorganization." He holds that "the most important part of the successful treatment of phthisis is to have clear notions as to its connexion with irritation," which "we find, in the great majority of cases, to precede, accompany, and accelerate the disease, and further that, within certain limits, it is by removing irritation that we best succeed in effecting a cure;" and the two first conditions of a successful attempt to cure rather than merely palliate are, in his opinion, "1st, the absence of the strumous diathesis," and "2nd, the fact of the disease being recent, for *where physical signs of tubercle exist* the chance of recovery is inversely as the duration of the symptoms."—*On Diseases of the Chest*, 1837.

Niermeyer's views are similar to the above. After denouncing the *laissez aller* practice which neglects the early stage, he says :—

"Let anyone make the experiment of having patients with the supposed signs of a commencing pulmonary tuberculosis strictly confined to their beds for a time, forbidding them all unnecessary talking and coughing, covering their chest with poultices, ordering a local abstraction of blood

by leeches or cupping on its first appearance, and on every recurrence of pleuritic pains, and he will soon satisfy himself that many a patient gets well who would formerly have been assumed to be the subject of tuberculosis, and therefore incurable.”—*Lectures on Phthisis*.

I may state that in many such cases I have known the application of a few leeches to the axilla, followed by the use of counter-irritation, or the persistent application of tincture of iodine to the affected part, with the internal exhibition of iodide of potassium and bi-carbonate of potass with hydrocyanic acid or digitalis, to be followed by rapid disappearance of all signs of consolidation and constitutional symptoms. Usually, however, this treatment has been succeeded by the exhibition of cod liver oil, and of the chlorides of barium and calcium, with full doses of hydrochloric acid and conium—medicines which seem to have a special influence on the lymphatic system, and to act as specifics in other affections of a scrofulous nature.

My experience is thus confirmatory of that of the late Dr. Parkes, who, while admitting the great benefit which has arisen from the introduction of cod liver oil, says :—

“The local counter-irritation and local blood-letting employed in former times, as well as the iodine and mercury used with the idea of removing exuded inflammatory products, seemed to be very useful, and there are numerous cases of phthisis which appear to be largely benefited by measures of this kind, or by a union of the two plans of treatment.”—*Address in Medicine*, August, 1873.

NOTE E, PAGE 36.

The following is the passage to which I allude in the foot-note :—

“Neither is the method or the nature of the tradition material only to the use of knowledge, but likewise to the progression of knowledge; for since the labour and life of one man cannot attain to perfection of know-

ledge, the wisdom of the tradition is that which inspireth the felicity of continuance and proceeding. And therefore the most real diversity of method is of method referred to use, and method referred to progression : whereof the one may be magistral and the other of probation," &c.—*Advancement of Learning*, B. ii., c. xvii.

NOTE F, PAGE 37.

See Mr. Bryan's papers in *The Lancet*, January 12, 1833, and February 8, 1834. If we compare his conclusions with those of M. Foster (*Physiology*, 2nd ed., p. 113), with which they seem to be almost identical, and compare also the experiment by which he proves the change of form of the ventricle during systole with the similar experiments of Chauveau and Halford, the statement in the text will appear fully justified.

NOTE G, PAGE 38.

To cite one example of this misrepresentation and of groundless fear of the influence of auscultation :—Wardrop takes exception to Laennec's assertion that by the aid of auscultation and percussion "*some physiological symptoms caused by disturbed functions, and, taken by themselves, extremely vague*, can sometimes acquire a degree of certainty which they had not before possessed." Upon this passage (the italics are his own) Wardrop founds his charge that Laennec, "while attempting to give an undue importance to the physical signs of diseases of the heart, altogether disregarded and endeavoured to undervalue the importance and utility of the physiological symptoms," adding that "the influence of this opinion is the more to be regretted as it has had the effect not

only of impressing on the minds of other inquirers that the physiological symptoms are of little value, but his views seem to have created an insurmountable barrier to all future investigations on this subject."—*On Diseases of the Heart*, p. 144.

NOTE H, PAGE 39.

Laennec's statement that the voluntary muscles, in cases of fever of a putrid type, are also softened, was questioned by Louis, and, on his authority, by Dr. Stokes, who accordingly assumes "that softening of the heart exists in typhus fever as a local disease, and without any analogous condition of the muscles of voluntary life." It has, however, been fully confirmed by Zenker, who says Dr. Hayden "examined over one hundred bodies of those who died of typhus at Dresden in the epidemic of 1858-62, and found the voluntary muscles chiefly the adductors and the recti abdominis in a state of degeneration in different degrees." The observations of Zenker, confirmed by others, are adopted by Dr. Murchison in the second edition of his great work on fever.

NOTE I, PAGE 42.

Dr. L. Clarke refers particularly to a case reported by Lancereaux, in which he found after death a diseased condition of the cardiac plexus of nerves, and a calcareous tubercle seated close to the point of recurrence of the inferior laryngeal nerve.

Laennec, differing in opinion from those who considered angina to be the effect of structural change in the heart or its arteries, believed this to be merely a

coincidence, and not a causal connexion. He held "that the seat of the affection was sometimes principally in the pneumogastric, sometimes in the nervous filaments which the heart receives from the grand sympathetic, other nerves being also simultaneously affected, either by sympathy or from direct anastomosis." Laennec's theory—of uncomplicated angina—was adopted by Romberg, and recently by Eulenburg and Guttman as follows :—

"To sum up, we would state that the symptoms in angina pectoris may be referred to various causes, some of which may be external to the heart, that probably all the cardiac nerves and the ganglionic apparatus of the heart co-operate to a greater or less degree, and that the variable character of the phenomena observed in different cases may be traced to the greater or less share taken by the different nerves forming the cardiac plexus. *The sympathetic is probably most considerably involved*, as it takes the principal part in the formation of the cardiac plexus."—*On the Physiology and Pathology of the Sympathetic*, p. 107, 1879.

Laennec thus describes his treatment of neuralgia of the heart by magnetism :—

"I apply two strongly-magnetised steel plates of a line in thickness, of an oval shape, and bent so as to fit the part, one to the left precordial region and the other exactly opposite on the back, in such a manner that the magnetic current shall traverse the affected part. This method is not infallible, any more than others employed in nervous cases, but I must say that it has succeeded better in my hands in the case of angina than any other, as well in relieving the paroxysm as in keeping it off. . . . When the magnet gives but little relief simply, this is sometimes found to be increased on applying a small blister under the anterior plate," &c. (p. 705).

This statement was ridiculed by the critics, one of whom indulges in the following "solemn sneer" :—

"The treatment of this disease (angina) is so novel that we must make it known, at the risk of a smile at the credulity of its author. . . . The most singular circumstance in this morsel of therapeutic manipulation is to see a physician—the first in Europe, perhaps, for anatomical research, pathological inquiry, and habits of close reasoning—reviving the solemn

mystery of Mesmer (!), and the mockeries of animal magnetism. If a second Perkins shall arise, it is to be hoped that a second Haygarth will not be wanting."—*Edinb. Med. and Surg. Journ.*, October, 1826.

NOTE K, PAGE 45.

Sir Wm. Hamilton says:—

"We are not aware that Cullen made the discovery of a single phenomenon, nor do we think the less of him that he did not. Individual appearances are of interest only as they represent a general law. In physical science the discovery of new facts is open to every blockhead with patience, manual dexterity, and acute senses. It is less effectually promoted by genius than by co-operation, and is more frequently the result of accident than of design."

Our science was more justly estimated by Hamilton's eminent predecessor, Dugald Stewart, who says:—

"No science could have been chosen more happily calculated than medicine to prepare such a mind as that of Locke for the prosecution of those speculations which have immortalised his name; the complicated, and fugitive, and often equivocal phenomena of disease, requiring in the observer a far greater portion of discriminating sagacity than those of physics, strictly so called."—Preliminary Dissertation, *Encyc. Britan.*, vol. i., p. 701.

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NOTE K, PAGE 43.

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