1831: a retrospect : being the President's address to the Harveian Society, January 19, 1893 / by W.B. Cheadle.

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BEING THE

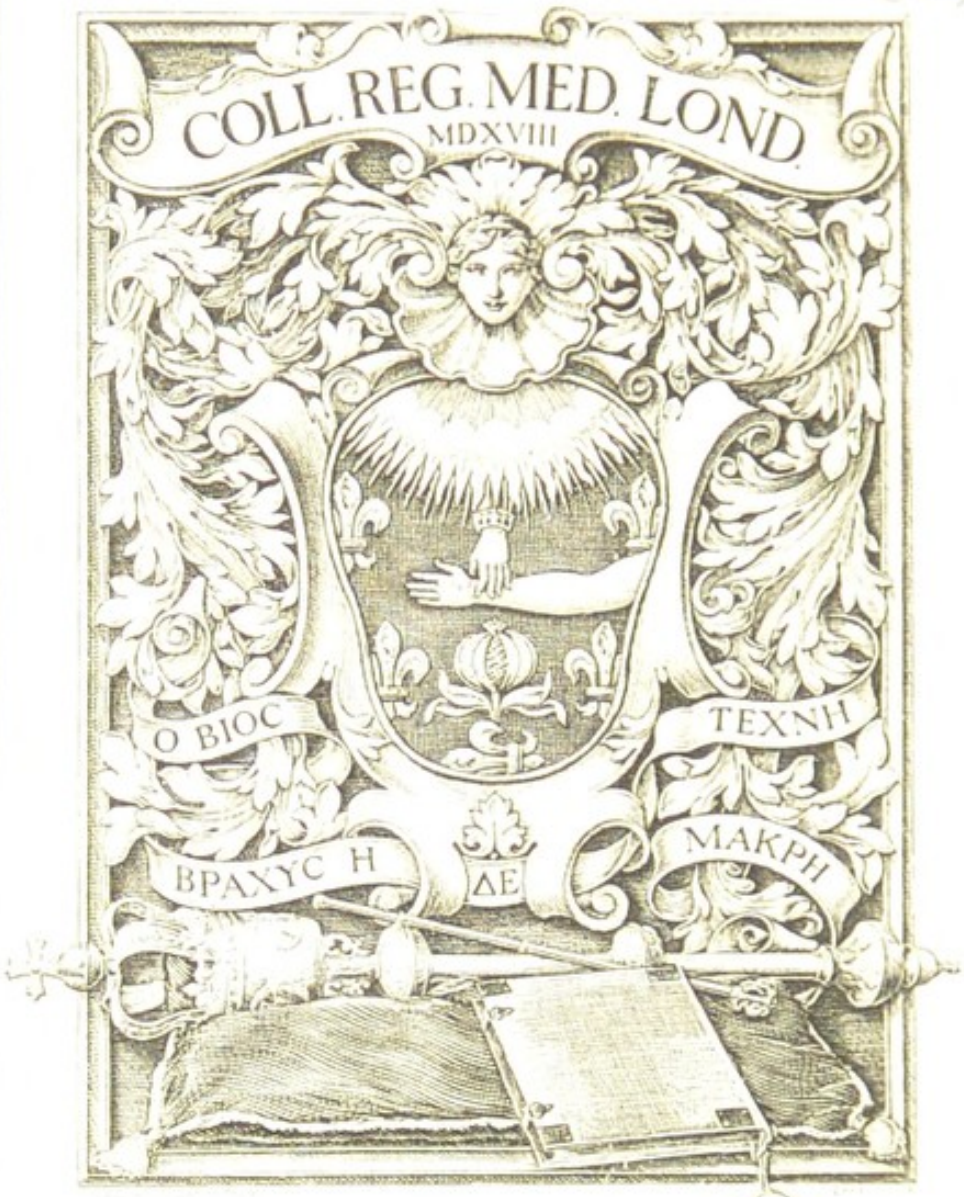
PRESIDENT'S ADDRESS TO THE

HARVEIAN SOCIETY

JANUARY 19, 1893

BY

W. B. CHEADLE, M.D., F.R.C.P.



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1893.

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A RETROSPECT.

SIXTY-ONE YEARS have elapsed since the Harveian Society was established. It was founded by ten medical men, who met for the purpose at the Western General Dispensary in Lisson Grove on September 15, 1831. Their names were:

Mr. Alexander Anderson.
Mr. Edgar Barker.
Dr. Anthony Todd Thompson.
Dr. Marshall Hall.
Mr. Mollison.
Mr. Giuseppi.
Mr. Henning.
Mr. Chesterman.
Mr. Cox.
Mr. Hodding.

Two of these original founders, Mr. Anderson and Mr. Barker, connected with the Western General

Dispensary, survived within my recollection, and I knew them well when I was physician to that institution more than twenty-five years ago. They form a personal link connecting us with that time.

The period in which the society was established—viz. the first half of the present century—was one fruitful beyond all others in the development of medical science. A great change had come over the spirit of the time. New methods had been adopted in the search for truth in the investigation of disease; these had been rewarded by great discoveries made in rapid succession. Under the new system light was gradually breaking in upon many difficult and apparently insoluble problems.

If we study the state of knowledge, of thought, and of practice at this time, we are able to realise the intellectual position of the members of the society in its early years, and the conditions under which they worked in the new connection opened out to them.

The preceding century (the eighteenth) had been the age of theory, of speculation, of hypothesis.

The nineteenth century ushered in the era of practical work, of the use of physical signs in diagnosis, of accurate clinical observation, corrected by examination of the body after death.

In the eighteenth century men of brilliant genius and great learning wasted their powers upon futile attempts to construct complete systems of medicine, and assumed various figments—imaginary bodies with imaginary properties—necessary to support them. It is curious to read the speculations of the leaders of thought at that time—men of great learning and intellectual power—and reflect that they formed the staple of medical inquiry and discussion little more than one hundred years ago.

One (Dr. Mead) hypothecated a 'nervous liquor' which he supposed to be a quantity of the universal elastic matter, the luminiferous ether of Newton.

Another (Boerhaave) had a system, of which the three main doctrines were inflammation, obstruction, and plethora.

Another (Hoffman of Halle) held that life depended upon an universally diffused ether, breathed in from the atmosphere, and contained in all parts of the body. It accumulated in the brain, and there generated the nervous fluid, or 'pneuma.' Health depended upon a proper 'tone'—some diseases being produced by excess of tone, or spasm, others by want of tone, or atony—a verbal figment which has survived to our day.

A fourth great teacher (Stahl of Halle)

developed the theory that the symptoms of disease were due to the efforts of the soul to rid itself of morbid influences.

A fifth (Cullen) based his system chiefly upon the doctrine of irritability, and regarded almost every disease, even gout, as a 'neurosis'—a fallacy which has been partially revived in our day.

Another notable system, constructed in this age of hypothesis—that of Dr. John Brown—met with widespread approval for a time. He referred all the processes of life and disease to one simple principle —viz. the property of excitability. Some diseases were sthenic, others asthenic, according to the amount of stimulus—terms which now survive. Cure was conducted on the same lines: sthenic diseases required depressant, asthenic stimulating, treatment. It is interesting to observe, however, that he estimated that 97 per cent. of all diseases required stimulation.

The construction of purely theoretical systems, framed upon baseless conceptions and arbitrary assumptions, reached its most extreme and fantastic form somewhat later, at the close of the century, in the homœopathic system of Hahnemann.

With all this waste of power from the misdirected efforts of the most original and brilliant thinkers of

the age in the field of medical discovery, the accumulation of positive knowledge went on. Additions were being steadily made, especially towards the close, by men such as Haller, Morgagni, Hunter, Matthew Baillie, Jenner.

Such was the condition of medicine at the beginning of the nineteenth century, the first half of which must be regarded as the great era, when the foundations of modern scientific medicine were surely laid by the adoption of the various methods of physical research, and the study of morbid anatomy. It was an age of marvellous advance—a time when giants arose in medicine. Rich beyond comparison is the roll of names of great men who adorned this period of medical history. Laennec, Bayle, Chomel, Louis, Cruveilhier, Andral, in France; in Germany, Romberg and Rokitansky; in Great Britain, Bell, Stokes, Bright, Latham, Hope, Marshall Hall, Williams; whilst, just beginning to rise to the height of their fame almost simultaneously in the last decade, the brilliant trio—the greatest of clinical teachers, Graves, and the most perfect of medical writers, Watson and Trousseau.

At the time when this society came into existence two great discoveries had recently been made, and were just beginning to bear fruit—discoveries

which more than any other, perhaps, have helped to an accurate knowledge of diseased conditions, and have effected the greatest revolution in medicine. The first was the discovery of auscultation by Laennec in 1818. The idea of ascertaining the condition of the lungs and heart—the faults in the machinery of respiration and circulation—by listening to the chest was almost entirely new. Hitherto auscultation had been limited to vague and trifling notions of sounds heard in certain diseases. It is remarkable that, of the very few references to be found of sounds heard in the chest before the time of Laennec, no less than three of them are met with in Hippocrates, who lived more than two thousand years ago, and who appears to have recognised the creaking of pleuritic adhesions and the splash of succussion.[1]

It is true that the way had been prepared for Laennec, and attention drawn to the physical examination of the chest, by the publication by Arenbrugger of Vienna of his 'Novum Inventum,' describing the method of direct percussion, by tapping the parietes with the tips of the fingers; the mediate percussion with the intervention of a plexi-meter, now in use, was an improvement introduced

[1] Dr. Gee, *Auscultation and Percussion*, 1877, p. 99, foot-note.

by Piorry two generations later. This simple mechanical device, apparently unimportant, proved the first great step in physical diagnosis. It effected more than all the systems devised by the most acute intellects of the century. At first, as might be expected, it was received with ridicule and contempt, and it did not obtain anything like general acceptance until the 'Novum Inventum' was translated into French by Corvisart in 1808. This led indirectly to the discovery of auscultation.[1]

Corvisart laid great stress upon the character of the heart's impulse; and Bayle, his pupil, was in the habit of applying his ear to the cardiac region, finding that a heaving impulse is more readily detected in that way than by the hand.

Laennec, Bayle's fellow-student, adopted the same method. You will pardon me if I repeat his graphic account of his discovery, although it may already be familiar to you. One day in the year 1816, as he relates, he was consulted by a young person who presented the general symptoms of disease of the heart, and in whom palpation and percussion gave no information on account of the patient's fatness. Her age and sex forbade an examination in the usual method. In this difficulty he remembered a well-

[1] Dr. Gee, *Auscultation and Percussion*, 1877, p. 100.

known fact in acoustics—that, if the ear be applied to one end of a plank, it is easy to hear the scratching of a pin at the other. An idea occurred to him. He seized a quire of paper, rolled it up tight, and applied one end of the roll to the præcordial region, his ear to the other. He was surprised and delighted to find that he could hear the beating of the heart much more clearly than by the ear applied directly to the chest; he had discovered auscultation. It is surprising that, seeing how much can be heard by the application of the naked ear to the chest, auscultation by this direct method had not been adopted hundreds of years before; but it was not so. Auscultation dates from the discovery of the stethoscope.

It was by the combination of auscultation and percussion with morbid anatomy, by the careful comparison of the physical signs noted during life with the changes found after death, and their verification by this ultimate proof, that an accurate interpretation of the signs of diseases of the chest was reached. This was the method followed by Laennec. He was not a mere stethoscopist. It has been said of him that, had he not discovered auscultation, his researches in morbid anatomy alone would have made him famous.

Although Laennec's work was published in France in 1818, translated into English by Sir John Forbes in 1821, while Stokes wrote an introduction to the use of the stethoscope in 1825, six years before this society was founded, auscultation had not been yet generally adopted in actual practice.

It was viewed with mistrust by the great mass of practitioners, and its use was at this time limited to a few experts who had acquired sufficient skill to trust the evidence which it afforded.

It is difficult for us in these days, when auscultation and percussion form the A B C of education in clinical medicine, to realise how scanty were the resources of the physician in dealing with diseases of the chest sixty years ago.

Not only was the art of physical examination confined to a few experts, but it was still very imperfect. In diseases of the lung Laennec worked out the physical signs of pneumonia, of effusion into the pleural cavity, of emphysema, and most of the signs met with in phthisis. He discovered the existence and significance of metallic tinkling. But he laid more stress upon the sounds of the voice than upon those of respiration, and knew little or nothing of bronchitis.

Laennec, however, had but a brief period in which

to complete his work, for he died of phthisis in 1826, eight years after the publication of his discovery of auscultation. Many of the more refined details are due to the later masters, amongst whom our countrymen, Dr. Williams and Dr. Walshe, occupy a high place.

As with diseases of the lungs, so with diseases of the heart, many morbid sounds were yet unrecognised ; and, of those which were heard, their mode of production was in many instances misunderstood ; they were vaguely ascribed to endocarditis ; their significance in disease was imperfectly apprehended or misinterpreted.

Yet Laennec accomplished wonders in the short time at his disposal.

He recognised most of the cardiac murmurs, even the noted præsystolic, but failed in the interpretation of them. He attributed their production to the sound of muscular spasm, forcing the blood through narrowed orifices, and regarded them all as obstructive.

Regurgitant murmurs were first recognised, and their mechanism first explained, by Dr. Hope about this time. Laennec had not grasped the principle of distinguishing the exact valve affected, and the mechanical fault which resulted, by the locality in

which the bruit was heard, and the period in the heart's action in which it occurred.

Laennec observed and described the præsystolic thrill, but he was unable to give any satisfactory explanation of its production, although he observed that it was liable to be associated with a narrowed mitral orifice, and that the first sound was prolonged. This thrill was by some connected with mitral regurgitation, or with pericarditis, and the final recognition of the præsystolic bruit as a sign of mitral stenosis was not made until 1843 by Fauvel, although Hope had previously described the diastolic murmur of mitral stenosis, and attributed it to obstruction to the passage of blood from the auricle to the ventricle.

Laennec appears also to have recognised the pericardial rub and its resemblance to that of pleuritic friction, without realising its real meaning. He concluded that the diagnosis of pericarditis was so difficult and obscure that it could only be guessed, not certainly detected. The typical double rub was not established as a sign of pericarditis until it was observed, and its meaning accurately appreciated, by Stokes and Watson some years later. The first experiments and investigations of Hope on the sounds of the heart were published in 1830, but they

were not completed until some years later. They were repeated and extended at this time by Dr. C. J. Williams, and the foundations of accurate pathology of valvular disease thus substantially completed.

The physical signs of diseases of the lungs and heart and great vessels were therefore, it will be seen, the subjects of eager inquiry and active controversy at this very time, and the great work of clearing up the difficulties connected with them was in full process.

A curious light is thrown upon the position of this branch of medical art, and the estimation in which it was held, by two passages in the writings of physicians of the day, which I venture to quote. Dr. Hope, in the introduction to the first edition of his work on 'Diseases of the Heart,' published about this time—in 1832—referring to M. Bertin's statement that Laennec's discovery had 'in a few years more completely illumined the diagnosis of diseases of the heart than all other modes of exploration had done for two centuries,' goes on to apologise for publishing his book, on the ground that 'the great body of the profession still deny that the piercing ray has reached its destination, and doubt the utility of auscultation in reference to the primary organs of

the circulation; still complain that the obscurity which involves the diseases of which we speak is scarcely less profound than ever.' 'I do not believe,' said a distinguished French physician, speaking of diseases of the heart to Hope, 'that, to tell the truth, it is possible to make the diagnosis except upon the table of the post-mortem room.'

Sir James Alderson, whom I knew as physician to St. Mary's, and subsequently as President of the College of Physicians, published his well-known paper describing the morbid appearances of collapse of the lung in whooping-cough, and the distinction between this condition and the consolidation of pneumonia, in the 'Medico-Chirurgical Transactions' for 1830. In the beginning of this paper he speaks of the great difficulty which exists in discriminating correctly between different diseases of the chest, on account of the obstacle which the unyielding walls of the thorax present to the examination of the organs within; contrasting with this the greater power which the physician possesses in the investigation of diseases of the abdominal organs. In these days the position is, I think, reversed. Diseases of the chest are diagnosed with a readiness and accuracy which contrasts painfully with the confusion,

difficulty, and uncertainty which surround diseases of the abdominal organs—the despair of diagnosticians.

Nothing can show more vividly the progress of auscultation, and the small reliance placed upon it sixty years ago, than this lament of Sir James Alderson and the apology of Dr. Hope.

From this time the study of auscultation and percussion engrossed the minds of both teachers and students. In place of being despised and neglected, it became the chief object of clinical study, and so occupied the field to the exclusion of almost all others, that a casual onlooker in a medical ward would have supposed that it represented the whole science of medicine—until the discoveries of Bright in renal disease began to attract attention, and turned it to another branch of physical research; then the examination of the urine shared the ground with the physical examination of the chest.

The discovery of Richard Bright was the second great event of this epoch. It rivalled that of Laennec in importance and in the magnitude of its results.

Up to the year 1827, when Bright published his celebrated 'Reports of Medical Cases,' showing that in certain dropsies the urine was albuminous

and the kidneys diseased, renal dropsy was quite unrecognised. The facts were indeed known, but imperfectly and apart; their dependence upon one another was not suspected. The presence of serum in the urine, for example, had been observed in the previous century. Fordyce, in his 'Elements of the Practice of Physic,' published in 1768, says that 'if the kidneys be relaxed or stimulated, chyle, serum, coagulated lymph, or even the red part of the blood may be thrown out.'

Dr. Darwin, in the first volume of his 'Zoonomia,' states, upon the authority of Dr. Cotunnius of Vienna (1770), that there is a species of mucilaginous diabetes which frequently precedes a dropsy, the mucilaginous fluid acquiring by stagnation the property of coagulation by heat.

In 1795 Dr. Latham had a patient in St. Bartholomew's Hospital, whom he mentions in his work on Diabetes as affected with a remarkable and copious discharge of serum from the kidneys.

Cruikshank, in 1798, observed milkiness or coagulation with nitrous acid in some diseases, especially in general dropsy or anasarca.

Shortly before Dr. Bright's observations, the coincidence of albuminous urine and dropsy had been pointed out by Dr. Wells of St. Thomas's Hospital,

in 1792, and by Dr. Blackall of Exeter, in his work on Dropsies, published in 1813.

Wells and Blackall, moreover, found the kidneys to be remarkably hard in these cases, but they failed to see any pathological connection between the facts.

Blackall explained the presence of serum in the urine on the hypothesis that it had become vitiated and possibly reabsorbed from the dropsical fluid into the blood, and then excreted by the kidneys as waste or effete matter.

Bright's genius was the first to connect the clinical symptoms of dropsy and albuminous urine with the morbid changes co-existing in the kidneys, and to interpret their real relation to each other. To him belongs the credit and glory of the discovery.

It was during this period of the early years of the Harveian Society that Bright was engaged in developing the discovery he announced in 1826, his last work on the subject being published ten years later.

As in the case of auscultation and diseases of the chest, so with regard to dropsy and diseases of the kidney, we can hardly realise how little was known about the matter at this time. As we have seen, the dependence of dropsy upon renal disease had only just been discovered. The pathology of dropsy

generally was most imperfectly understood. The connection of this condition with diseases of the heart and of the liver was known, but the exact nature of the relation between the serous effusion and the lesion of the organs was not clearly discerned—was still the subject of debate.

Bright, indeed, appears to have fully grasped the central truth, that passive dropsy is due to effusion consequent upon obstruction to the return of blood through the veins.

Yet we learn from Sir Thomas Watson in his 'Lectures,' published in 1837, that dropsy was commonly referred to deficient action or want of 'tone' in the absorbents; fluid accumulated because it was not reabsorbed, not because it was poured out in excess. All the efforts of the physician were directed to the stimulation of the faulty absorbents. The lymphatics were regarded as the agents in reabsorption of fluid; and Watson enters into an elaborate argument to refute this view, and prove that the veins play the chief part in the process.

Similarly, the knowledge of diseases of the kidney was extremely limited until Bright threw light upon them. In Baillie's 'Practice of Medicine,' published in 1825, nephritis is not mentioned. In two works on kidney disease which appeared in the same year

(Uwins—Dawson) nephritis is indeed mentioned, but it is clear that it was confounded with calculus. In Forbes's 'Cyclopædia,' published in 1834, some advance was made. Nephritis is included ; the appearance of a granular kidney described as met with in dropsical persons, and stated as the probable cause of the dropsy. There is also a description of a soft state of kidney which corresponds to suppurative nephritis.

The pathology and clinical signs of renal disease were therefore not known to the general mass of the profession at this time.

I have dwelt upon these two great discoveries of Laennec and of Bright because of their potent influence upon the development of medicine which was going on apace in this country at this date, and also because their history exhibits most strikingly the state of general medical knowledge then existing.

If we turn to other branches of medical science, our perception of the imperfect light in which the doctors of this time worked is rendered still more clear, and our wonder at the marvellous advance of the last sixty years still more profound.

The subject of diseases of the nervous system was in a confused and rudimentary state. Only a few diseased conditions of the brain and spinal cord

were recognised, as we may learn by turning to the text-books of the time. The discovery of the function of the anterior and posterior roots by Sir Charles Bell, which seems a matter of ancient history now, was comparatively recent then. The discovery was made in 1811, but his first work on the subject appears not to have been published until 1824. It was not until 1833 that Marshall Hall published his demonstration of the automatic reflex action of the spinal cord—distinguishing it from muscular irritability, showing its independence of volition and sensation, and the functions of the brain. The localisation of function in the nervous centres was almost entirely undetermined.

Thus the list of nervous diseases comprised little more than atrophy and hypertrophy of the brain and cord, inflammation of the substance and membranes, hemiplegia and paraplegia, with one or two local paralyses, and functional disorders. The practitioners of that day knew nothing of the localisation of nervous diseases in the brain and cord, of peripheral neuritis, of the significance of superficial and deep reflexes or electrical reactions. The great advance in the knowledge of nervous diseases is almost entirely the work of our own day.

One of the best illustrations of the condition of

knowledge at this time is afforded by parasitic diseases. Nowadays every student can talk glibly enough about cocci and bacilli and bacteria; but in 1831 men were not only absolutely unacquainted with micro-organisms, but with parasites of any kind, except only the intestinal worms and the coarser denizens of the external surface of the body. Hydatids were indeed recognised, but were regarded as pathological cystic formations, not as parasites. It was not until 1844 that they were supposed to have any relation to tæniæ, and they were then regarded as perverted tapeworms, which by some strange chance had strayed into a wrong host, and became dropsical and degenerated in consequence. Of the wonderful transformation of the cyst-worm into the cestoid they knew nothing, for the discovery was not made until twenty years later, when Kuchenmeister demonstrated by his experiments what had been with great acuteness inferred by Sir Richard Owen, from their structural resemblances, a few years before. The trichina spiralis had not appeared upon the scene, although its discovery was imminent at this very time. Hilton first noticed it in 1832, and in 1834 it was fully described and named by Professor Owen. It is interesting to read the comments of Sir Thomas Watson upon this discovery a little later.

He speaks of the trichina as a very strange and puzzling kind of parasite. 'One would imagine,' he says, 'that the presence of innumerable living beings in or between the muscular fibres would be likely to give rise to symptoms. We might expect pain, muscular debility, embarrassed movements. But no indication of the presence of these parasites in the living body has been afforded; it appears to be unconnected with any form of disease.' The explanation was, of course, that these trichinæ were in the encysted state—quiescent, inert—until set free by the gastric juice on entry into the stomach of some new host. The severe symptoms which occur during the active invasion of trichinæ correspond very closely with Sir T. Watson's adumbration when actually observed by Zenker in Dresden twenty years later.

The first discovery of one of the smaller parasites of the external surface was on the eve of demonstration. The existence of the itch insect had for some time been suspected, but its presence was actually demonstrated for the first time by Venucci, an Italian student, in Paris, in 1834. The fungus of favus, more important still, was found by Schonlein in 1839; that of ringworm by Malmsen, and tinea versicolor by Eichstadt followed in 1844.

These discoveries of microscopic insects and fungi, the first demonstrations of pathogenic micro-organisms, which were made about this period, are of exceptional interest. They were the first step towards the discovery of the dependence of contagious diseases upon the introduction of organic germs into the body.

The second great step towards this end was the revival of the doctrine of the ferments—an idea as old as Hippocrates, surviving in the humoral pathology, but revivified by Liebig through the analogy of alcoholic fermentation, shown by Pasteur later to be effected by the agency of the yeast plant.

The third great step in the discovery of parasitic disease thus inaugurated was the demonstration by Chauveau, in 1865, that the infective agent in contagious disease consisted of minute but distinct solid particles suspended in fluid—particulate—not of soluble chemical materials dissolved in it. This he did by the ingenious but simple experiment of allowing the infective lymph to diffuse through a porcelain diaphragm into distilled water. The residue conveyed the disease: the diffused liquid was incapable of doing so. These experiments were afterwards repeated by Dr. Burdon Sanderson, who

confirmed the results, and added a proof so perfect and so neat that I cannot refrain from stating it. He found that if vaccine lymph were diluted, the chance of effective vaccination with the diluted fluid was in inverse proportion to the degree of dilution. The virus was not weakened by dilution. If it took effect at all it took full effect. Whether the vaccination was effective or not clearly depended upon the chance of one or more solid particles being included in the inoculating liquid or not.

Such were the discoveries in parasitic disease which were on the verge of disclosure, although still hidden in 1831.

To turn to another subject for illustration, the distinction between typhus, typhoid, and relapsing fever had not been made. The three forms were confused together under the term 'continued fever.'

A glimmering of the truth was just becoming perceptible. In 1826 it was suggested that relapsing fever might be a modified form of typhus, and in 1829 Louis made a similar suggestion with regard to typhoid.

It was not until 1836 that typhus and typhoid were first noted as distinct diseases by Gerhard of Philadelphia, and Lombard of Geneva, and still more emphatically by Dr. Stewart in 1840. A similar

observation with regard to relapsing fever was made by Dr. Henderson of Edinburgh in 1842. The absolute independence and individuality of the three forms was not finally and conclusively demonstrated by Sir W. Jenner until 1849.

I have endeavoured to show the position of medical knowledge in some of its chief branches. I can merely indicate some other blanks which remained. Thus, Dr. Hodgkin had not published his discovery of lymphadenoma, a disease now familiar enough to us, although the paper in which it was described must have been already in preparation, for it was read before the Medico-Chirurgical Society on the 10th and 24th of January, 1832.

Dr. William Addison of Great Malvern had not observed leucocytes lying outside the vessels in inflammation, for this observation was made in 1843, and the actual process watched by Dr. Waller in 1846, twenty-one years before Cohnheim's final demonstration of it in the mesentery of the frog. Embolism was not known. Infarcts had just been described by Hodgkin in 1829 ; but the recognition of their nature and all the mechanism of embolism was the work of Virchow and Kirkes some twenty years later. Leukæmia was unknown, although in 1827 Velpeau had described the appearance of the

blood in a case of enlarged spleen as though mixed with pus. The white cells were not seen by Hughes Bennett and Virchow until 1845.

Again, disease of the suprarenal capsules, and idiopathic or pernicious anæmia, which seem to the modern student ancient and well-established diseases, were at this time discoveries of the distant future. They were not described by Dr. Thomas Addison until 1855. Similarly, the connection between membranous laryngitis and diphtheria was not suspected. I might give any number of like instances to illustrate this point, but these will suffice. Moreover, the practitioner at that day not only had to do his work by the light of imperfect knowledge, but he had few appliances to aid him in diagnosis. The stethoscope was almost the only instrument of precision which he possessed, and in the use of this, as we have seen, few were expert. The use of the microscope in medicine was in its infancy. Casts of the urinary tubes, for example, now common objects, were only beginning to be observed in 1844 by Henle, and were not described fully by Dr. George Johnson until 1852; low powers only were used. Sir T. Watson, in his Lectures, speaks of casts as hair-like threads, very slender fibrinous coagula, showing, I think, that he had viewed them through a low power. The clinical

thermometer, originally invented by Sanctorius of Padua in the seventeenth century, and adopted to some extent by Van Swieten and De Haen in the eighteenth, did not come into fashion as a clinical instrument until the development of it by Barensprung, Traube, and Wunderlich in the early fifties. The ophthalmoscope invented by Helmholtz dates from 1851, the laryngoscope of Avery the same year, the sphygmograph of Marey, 1863. In respect of drugs and other remedial agents, again, the resources of the physicians were confined within narrow bounds in 1831. Chloroform, for example, was discovered in that year simultaneously by Sabeiran in France, Guthrie in America, and Liebig in Germany, but its anæsthetic power was not demonstrated by Simpson until 1847.

The special action of digitalis, and of iodide of potassium was unknown, and the innumerable powerful agents brought to light by the skilled processes of modern chemistry and physiological research were still hidden secrets of therapeutic art, not absolutely perhaps to the unqualified disadvantage of the patients. The physician of that day had to ring the changes on calomel, opium, colchicum, antimony, bark, salines, blood-letting, and blisters. It is worthy of note, however, that the external applica-

tion of cold for the reduction of febrile temperatures, which now plays an important part in the treatment of pyrexial disease, had become general in Liverpool in the hands of Dr. Currie, Brandreth, and Gerard as early as 1797. They used it in typhus and scarlet fever, in ague and small-pox, and in convulsive nervous disorders.

Looking back, then, at this time, when the state of medical knowledge was such as I have endeavoured roughly to depict, when light was just breaking into dark corners, when great discoveries had just been made, when the first dim signs of greater discoveries still were faintly discerned as coming in the near future—we can understand how men were stimulated by recent achievement to earnest endeavour to win fresh secrets from nature.

To the enthusiasm born of this spirit our Harveian Society owes its origin, and it would be of great interest to discover what part it played in the medical progress of the day. It numbered amongst its members the leading practitioners of this district, and some of the foremost physicians and surgeons of the time. Anthony Todd Thompson and Marshall Hall were its first Presidents.

They were followed by a number of distinguished men, amongst them Hope, Sir David Barry,

Benjamin Phillips, Theophilus Thompson, C. J. B. Williams, Clendenning, and Hodgkin.

The discussions in which these men took part would have been of great interest to us, had we been able to review them now by the light of modern medicine. Unfortunately, however, the early records of the society are wanting. I have searched for them in vain. They are lost.

I have succeeded in gathering a few items only. I learn from my friend, Dr. Pollock, who has known the society from comparatively early times, and has rendered it such excellent service, that Dr. Marshall Hall brought forward here his plan for recovering the drowned, and also a suggestion for the employment of tracheotomy to relieve the epileptic fit.

I have discovered a Presidential Address by Dr. Hodgkin, delivered in 1847, on 'Medical Reform,' in which he proposed the establishment of a general State or Government examination, the passing of which should confer the sole legal qualification to practise. This idea has been partially realised in the conjoint scheme for examination by the Colleges of Physicians and Surgeons now in operation.

A careful scrutiny of the journals of this period has disclosed no records of papers read or discussions

held. They do not appear to have been published after the more assertive fashion of our day.

I gather from Dr. Hodgkin's address that the early meetings were chiefly clinical, when cases of interest were brought forward and debated, and analogous instances related and compared. From my experience, such clinical evenings are more popular, more useful to the members, more fruitful for the extension of medical knowledge, than the delivery of set papers. I would commend to the consideration of the Council whether this department of the society's *rôle* might not with advantage be extended.

Of the work of those distinguished members who come nearer to our own day I can venture to say little. Time is needed to afford the perspective necessary to enable us to appraise their work at its just value.

I will not presume to criticise the living. There are two names of men who have joined the majority, however, which I cannot pass over without a word, viz. those of Dr. Sibson and Mr. James Lane, colleagues and valued friends of my own at St. Mary's, and former Presidents of this society.

Dr. Sibson was the first Harveian lecturer. He gave a series of demonstrations of arterial tension

in Bright's disease, a subject then beginning to attract special attention by the observations of Traube, thus developing a line of clinical research which has been most fruitful in results. Mr. James Lane, to whose initiative the Society owes the foundation of the lectureship, delivered the second course, containing valuable practical observations on syphilis. Both have passed away, leaving honoured memories behind them.

In conclusion, let me congratulate the society upon its present satisfactory position. This is largely due to the energy and zeal of the two excellent secretaries, Dr. Hill and Mr. Roughton, to whom I tender my best thanks for the unstinting service they have given.

I feel confident that under the leadership of the able and popular President you have elected to succeed me, the coming year will be one of even greater prosperity than the last.

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