

great distress is excited. If this spontaneous varicose aneurism (following Mr. Thurnam's term) take place between the aorta and vena cava, the head, face, and upper extremities rapidly swell, becoming purple and oedematous; while the lower part of the frame remains of the natural size. When it takes place between the sac and the pulmonary artery, oppression, distress, pallor, and speedy or even sudden death, ensue.

In aneurism of the ascending aorta, when the latter is universally dilated, it is lengthened as well as widened. Since it cannot work its way upwards towards the neck, it makes way downwards towards the heart, which it consequently likewise displaces downwards. The result is that these aneurisms are situated comparatively low down in the chest, and present almost invariably below the level of the manubrium. At the same time, the tumour, obeying the direction of the current of the blood from left to right, and from behind forwards, generally presents to the right of the sternum, at or below the second space.

The same rules apply to saccular aneurisms of the ascending aorta; consequently, while twenty such cases presented a pulsating tumour to the right of the sternum, only eight did so to the left, and nearly or quite all below the second cartilage.

In some cases of general dilatation of the artery from the sinuses to the innominate, the valves, although healthy, are too small to close the aperture of the aorta during systole, and so permit regurgitation. In one such instance there was a diastolic thrill over the right second space, and a loud musical murmur was generated there, and transmitted thence over the whole chest and over the aorta down to the umbilicus in front of the sacrum behind. In this case, as in all cases of exclusive aortic regurgitation, the pulse is audible at the wrist when the arm is raised, a systolic shock being then heard, which disappears when the arm is lowered.

In those cases of aneurism of the ascending, as well as in those of the transverse aorta, which present at the surface, if there is no aortic regurgitation or murmur, there is a double impulse, the one systolic, the other diastolic. Corresponding with this double impulse, there is a double sound—the one the first, the other the sharp second sound. This was present in several of the cases brought forward by Dr. Sibson, and was pointed out successively by Dr. Stokes, M. Guérin, Dr. Bellingham, and Dr. Lyons, and is an important sound, since, while this double shock and sound are present over the aneurism, and the impulse and sounds of the heart are present over that organ, there is an intermediate portion of the chest occupied by lung where there is no impulse, where the heart sounds are feeble, and where breath sounds are heard. There is also corresponding dulness on percussion over the tumour and the heart, with intermediate lung resonance.

The effect of pressure of the aneurism on the vena cava was well shown in one of Dr. Sibson's cases, who presented on admission swollen, purple, and oedematous condition of the head, neck, and upper part of the body. The aneurisms of the transverse aorta present themselves behind or at the side of the manubrium. They are held high up by the great vessels. They are thus distinguished from aneurisms of the ascending aorta, which present below the manubrium. The pressure of the aneurism forward on the left vena innominate is indicated by fullness of the left jugular vein, as in a case of Dr. Alderson's as well as of Dr. Sibson's. In Dr. Alderson's case the deep jugular had adapted itself to the new channels, and had shrunk to the size of a quill. Its pressure backwards on the trachea, or œsophagus, or both, excites on the one hand mechanical dyspnoea, indicated by noisy tracheal breathing, or even "stridor from within," the patient habitually stooping forwards to relieve the pressure, and on the other dysphagia. The pressure of the tumour on the left recurrent excites those most characteristic and frequent signs of the aneurism, stridulous breathing, hoarse voice, and hoarse cough.

The feature of the descending portion of the aorta is its comparative fixity to the bodies of the vertebrae, and the characteristic effects of its aneurisms are, pressure upon and absorption of the bodies of the vertebrae; pressure upon the sympathetic nerves, indicated by a gnawing, permanent, local pain in the back; and pressure on the intercostal nerves in the later stages, indicated by intense paroxysmal lancinating pains forward along the exact course of the nerves. Pressure on the left bronchus, told by absence of breathing and immobility to use the left upper lobe, is another frequent important feature in such cases.

The paper was illustrated by drawings representing the post-mortem appearances in cases observed by the author during life.

ADDRESS IN MEDICINE.

BY W. H. WALSH, M.D.

Mr. President and Gentlemen,—The weekly medical press, the graver quarterly journals, the semi-annual retrospects, and, above all, the literature itself which feeds the latter, set forth and register the particulars of last year's scientific and practical achievements. Herein abound not merely the signs of that restless activity which will perpetually push to the surface amid large bodies of intellectual men, but the evidence of real substantial improvement. Whether it be in our knowledge of the chemistry, the physics, or the dynamism of disease, the word "progress" is legibly inscribed on the records of last year as on that of many of its recent predecessors. Now, it seems to me that a brief survey of the ultimate causes and mechanism of this modern progress, especially in regard to its promise of durability and sustainment, may not unfitly occupy some portion of our time to-day. The existing advanced condition of medicine and its brilliant promise for the future as a means of positive knowledge, are, I think, primarily and essentially due to the spread of an improved observation among those working at clinical and subsidiary pursuits, to the patience, precision, and minuteness with which facts are investigated and recorded. In days not very far from our own, and even by the most eminent in our ranks, the observation of medical facts was more or less openly contemned; was disdained by pathologists of mark as the fitting task of the patient drudge rather than of the man endowed with intellect capable of working out the alliances and repulsions of those facts, distinguishing their necessary, probable, contingent, and purely accidental relationships, inducing therefrom general results, and co-ordinating these into a system. The correctness of the facts themselves, absurd as this may seem, was treated as matter of small importance; the first-comer willing to undertake the work was accepted on his own guarantee as an efficient and competent observer, while it was reserved for the intuitive aptitude of the systematizer to distinguish and separate the real from the unreal, and arrive at an issue of pure truth from premisses confessedly containing variable quantities of jumbled truth and error. But two causes have been fatal to imperfect observation as a system, whether by proxy or otherwise. The first of these causes is the failure, one after another, of pathological doctrines founded upon it—a failure necessarily entailed by the abiding, inseparable influence of the original quantum of admixed error, and by the impossibility of fashioning a total of truth out of elements partly true and partly false. It has at length been perceived that fundamental unfitness underlies the whole process. The second cause is, that observation is now undertaken by men fitted for the task, by men of large natural and trained endowment: and the toil is undertaken by these because it is now generally conceded, and by them in particular has been intuitively felt, that the work of observation is alike difficult and exalted; that to make an observer requires a combination of faculties as high, though in some sort of a different order, as to make a speculative thinker; that in the mass and main, and as a rule, to observe facts is as lofty an expression of intellect as to conceive thoughts. True, it must be admitted that in some men whom the *mens divinator* illumines, in whose brains a spark of the divine essence scintillates, thought is grandest; it goes before facts, it creates, it defines these, and leaves experience to drag its slow length along to the goal of truth which itself has long since reached. But we speak of the ordinary type of intellectual men—not of those exceptional marvels, of whom some two or three are vouchsafed to the world in the course of a century. The value of observation being granted, its true function in the establishment of medical science being recognised, that the means of conducting it should be extended and improved followed as a necessary consequence. Hence that constant tendency to the employment of instruments of various kinds, characteristic of the clinical investigations of the day—instruments which have enabled us in many cases to estimate the degrees of the objective phenomena of disease with an amount of accuracy, not figuratively but literally mathematical. Hence the change that may be noted in the programmes of our medical schools. The direct education of observers is now made part of their schemes of instruction, the bed-side training of the senses, through which the objective signs of disease become intelligible—the bed-side training of the judgment, whereby the reality of subjective pains may be estimated—the bed-side training of the logical faculty, that enables us to

discriminate between the important and the unimportant, and fix on the true relationships of the complicated perversions of function and of structure we are called to unravel. Coevally with this practical recognition of the place held by observation has arisen proportionate distrust of the efficiency of the deductive method. The abandonment of this method has followed. At last, we have acknowledged that in medicine there are no first principles, within reach of human intellect at least, wherefrom we can descend to facts *ad libitum*; there are no axioms in our science wherefrom we may fashion theorems and deduce propositions that shall suggest physiological and pathological facts, and establish a doctrine of disease prior to experience. We have even recognised the cardinal principle, that whereas in certain other branches of human knowledge the deductive and inductive methods of obtaining truth are both applicable, in fact, reciprocally sustaining, the one often supplying what the other is unable to give, in our sphere of mental work the smallest step cannot be safely made by the light of deduction. We begin from individual facts, and rise to those general inferences which are our most comprehensive expressions of attainable truth. How different the mode of proceeding in that purely deductive science which deals with the properties of space and number! Here the beginning is with axioms—that is, with propositions by assumptions, so absolutely true as to be genuine truisms, and upon these axioms are founded the details known as geometrical science. True, there have been persons credulous of the reality of first principles, *quasi* axioms of pathology—persons who have actually manufactured these, and attempted to deduce therefrom clinical developments of disease. We have abstract principles of animism, Stahlism, Broussaism, &c. We have had the deductive method exhibited in the concrete form by Cullen in his fanciful account of synocha—a disease which he neither professed to have encountered himself nor maintained to have been seen by others, but which, on the faith of his “Principles,” not only must exist, but must assume the characters, run the course, and affect the modes of termination which he invents as he goes. Such achievements as this have had their day; they are not likely to be revived. But it supplies us with a useful lesson or two. Observe the easy indifference about truth, as conceived by most minds, involved in a procedure such as that of Cullen; and yet no real proclivity to falsehood existed in that man. He was probably as honest in his veneration of truth in the abstract, as the most painstaking Baconian that ever stood, thermometer in hand, by the bedside of a pyrexial patient, and trembled lest he should misinterpret by the tenth of a degree the temperature under the tongue. He was no more dishonest than the keen-witted philosopher, Hume, who, both in morals and political economy, went the length not only of preferring ideas to facts, and professing indifference bordering on contempt both for the process of their collection and for themselves when collected, but of maintaining that when the preconceived ideas of deductive philosophy and the actual observed facts clashed, the collision was unimportant: facts must yield, theory hold its sway unchanged. No, it was not the man, but the system that made light of truth. Cullen and Hume were both equally innocent of designed mendacity.

Again: it is curious to observe that just as in other branches of knowledge—in theology and in morals, for example—so in medicine. Wherever and by whomsoever deductive philosophy was professed, a sort of terrorism was invoked to suppress the right of private judgment—to make the *sic volo, sic jubeo, set pro ratione voluntas*, reign supreme. Note the dogmatism of Broussais—splenetic, overbearing, and intolerant. See him denounce all theorists but himself, and revile searchers after facts, just as certain speculative theologians proclaim that tolerance for any creed but that fostered by themselves is positive impiety. Do I exaggerate in affirming that a few years past it was a work of danger—a sort of chivalry in the field of science—to oppose the prevalent dogma of the so-called physiological school; that a man who failed to comprehend was simply regarded as a dolt, while he who ventured to question took his place amongst the crazy ones of his day? But if we may fairly assume that the deductive method is banished as an acknowledged scientific instrument from the domain of pathology, it is equally certain that indications of an unfortunate disposition to return to it every now and then make their appearance; and yet there can be no reasonable apprehension that any *à-priori* system of pathology will again take serious hold of the professional mind, if men will for once, for once only, accept the warning offered by the history of the past. That history not only teaches us the absolute failure of all such systems, but shows that so long as deductive methods prevail, all progress must, if not solely, at least essentially, consist in the destruction of something that

has gone before—the energies of genius must be wasted in negating the errors of the past. It is noteworthy enough that the story of medical progress in this aspect gives feeble support to, nay, almost clashes with, the opinion held by some speculative historians who regard the dominancy of inductive reasoning as especially characteristic of the Anglo-Saxon mind. The general proposition may be true, or it may be false—I rather think the former; but unquestionably the Celtic mind of France has taken a very large, if not the largest, part in establishing the supremacy of induction as the real instrument for discovering medical truth. Nor, Mr. President, must we forget, as an element of modern progress, that the true function of hypothesis has been fixed: on the one hand escaping the grave fallacy of the deductive system, which first assumes the hypothesis, then assumes its absolute truth, and next reasons down to facts; on the other, we have learned to steer clear of the almost equally serious error of rejecting hypothetical propositions altogether. Hypothesis is now valued at its worth: it is accepted as an instrument of suggestion; it is welcomed as a clue which guides the senses in the observation of facts, and so saves frequent waste of time and energy in the search after, and registration of, particulars that bear little or no relationship to each other, and must *pro tanto* prove insusceptible of furnishing general conclusions. We are at the present day, also, fully alive to the danger of too freely utilizing hypotheses, even in this subdued and suggestive fashion. We have learned to acknowledge that it is often difficult to resist the temptation of squeezing facts to fit the hypothesis for which, as a creation of our brain, we entertain much of the tenderness of a parent for his offspring, and which we cannot be expected to sacrifice without a struggle to the pitiless logic of facts. Hence, perhaps, it is that many of the most solidly established propositions in pathology have been induced from pell-mell accumulations of individual facts heaped together independent of any pre-formed idea whatsoever. Look, in illustration, at several of the most striking amongst the pathological laws discovered by Louis, which, taken alone, would suffice to disprove the notion held by some thinkers, that the search after a generalization involves as a necessity the conception of an hypothesis. No; not only were the primary facts sought without guiding hypotheses, but, in many instances, they were thrown into groups by a natural process of attraction, untrammelled, unthwarted, and uncontrolled by any preconceived idea. Still this circumstance does not negative the value of hypothesis cautiously employed in the suggestive sense. Only let us not take ideas for facts: ideas should be accorded a large place, submitted to the touchstone of experience; never rejected without trial, unless on the very surface they bear the stamp of error, or adroitly seek to conceal an under stratum of folly. In this sense, but in no other, ideas may be welcomed; still so few are the men capable of conceiving such ideas as shall withstand the rude test of experience, and so much fewer are the men who, were their theory annulled, would calmly relinquish its propaganda, that I for one should grieve to see inscribed on the portals of Medicine any epigraph—no matter what venerated authorship it might claim—deifying ideas. I refuse to concede with Wieland, as far at least as medicine is concerned, that “the situation of the most enchanted enthusiast is preferable to that of the philosopher who, from continual apprehensions of being mistaken, at length neither dares affirm nor deny anything.” No, not preferable in the domain of physis. False theory must be worse than no theory at all, in a sphere of knowledge where speculation entails action—where the practice of an art flows from the doctrine of a science.

And so we pass on to the next cause of existing progress—the recognition of the true significance of so-called pathological laws. When Louis first promulgated a series of general propositions on which this title was bestowed, the word was accepted in a more absolute sense than it can fairly command. It was supposed by some ardent persons that these laws were as stable as those of the growth, decay, and death of the organism—nay, as the physical laws of the universe. It came to be held that it would be well-nigh as positive an infringement of the order of nature were tubercle found in any other organ of an adult whose lungs were free from that product; as if the movement of bodies were detected in infraction of the law of gravitation. But the day of these delusions has passed away. We now know that those general results which we take for the expressions of pathological laws in the sense of fixed, forced, pre-ordained and immutable relationships of antecedence and consequence, (without reference to those deeper metaphysical meanings that underlie the idea of law,) have nothing of this scope, of this absolutism, of this grandeur. A larger experience than that which originally worked out these presumed laws

has shown that they are not laws at all in the higher significance of the term. A law of nature knows no exceptions. The so-called pathological laws, on the contrary, are subject to perpetual exceptions; in point of fact, they are at best the generalized expressions of degrees of frequency. When we say, for example, it is the law of such and such a disease that such a combination of circumstances occurs in it, we really mean no more than that in a certain calculable majority of instances of that disease that combination will occur. Now, in thus relinquishing the ambition of establishing laws of pathology assimilable to and co-ordinate with laws of nature, we appear, instead of advancing, to have receded. The progress here savours somewhat of a negative quality it is true; but the ultimate value of surrendering vain pretensions has in all sciences been positive. Closely linked to this improvement, allied to it in nature, stands the corrected estimate of the men of to-day concerning the true import and value of the numerical method, especially in its application to clinical medicine. So long as any one mode of combination of certain clinical facts and conditions can only be spoken of as more or less frequent than another or other possible combinations, the want of definiteness in the idea and in its expression wholly deprives both of either scientific or practical significance. But the case is changed once, by the aid of the simple process of counting, a distinct numerical value is substituted for the vague words "more" or "less;" for conceding, as we have already done, that these numerical expressions of precise degrees of frequency do not carry with them, either directly or inferentially, a revelation of laws in pathology, still they most certainly present average combinations of phenomena of disease in the mass, and so oftentimes furnish most valuable peeps through the obscurity surrounding the natural affinities and repulsions of morbid processes. Now these numerical expressions are, of course, perfectly sound so far as the particular mass of facts from which they were derived is concerned, and they will continue to be sound if applied to a fresh mass of cases of the same nature arising under similar conditions. But a function of more practical importance than this was at one time assumed for them. It was supposed that numerical expressions signifying majorities of variable strength might be subsequently applied in individual cases with almost unfailling surety. Is this the fact? I fear not. It may, for instance, be perfectly true, is perfectly true, that in the great majority of instances chronic peritonitis in the youthful adult is not only diathetic, but especially attached to a certain diathesis—the tuberculous. If not a law, which certainly it is not, this is at least a rule; but if we apply this rule to individual instances, and attempt by its aid alone to found the diagnosis of tuberculous peritonitis, possibly in the very first instance that comes before us of a character to test the point, our faith in averages, as applied to individuals, may be destined to be rudely shaken through the discovery that the chronic peritonitis before us sprang from cancerous and not from tuberculous seed. Now in the recognition of the practical danger of a too absolute application of averages to individual instances there is progress. Some years past there existed much too great readiness to trust to these averages as sure elements of diagnosis. Let us not, however, run into the opposite extreme of undervaluing them. Even in this clinical sense they are of great subsidiary importance, and should never in the balance of *pros* and *cons* in a difficult diagnosis be omitted as more or less serious items of evidence. But, gentlemen, in stimulating, sustaining, and guiding these means of improvement in medicine, is found what may be emphatically termed the dominant intellectual quality of the age—the spirit of philosophic incredulity and of independent inquiry, and the absolute rejection of authority. The active faculty of doubting has acquired vigour, the passive endowment of blind faith has dwindled into decrepitude. Formerly we had a minimum of knowledge and a maximum of credulity; now, we have an ever increasing quantity of fact and involved inference, that is of true knowledge, and a perpetually decreasing aptitude to take things on faith. As in theology and in morals, so in medicine; the conservative spirit has until of late years been almost omnipotent. To the mass of men that which is "grey with years is godly," and in no sphere of knowledge has this been more distinctly felt than in the medical. But of late we have escaped from this sort of willing bondage to the past; the time has gone when it was the highest merit to discuss with acuteness opinions of writers who had gone before, illustrate their surface sense, search out their deep significance (often where none existed), and, in a word, comment through opinion on opinion, *ignotum*, as it were, *per ignotus*, with scantiest reference to original fact. These were the palmy days of tradition and its commentary. True, the spirit which feels that any given doctrine is not of

necessity sound, simply because protected by the ægis of respectable names—the spirit without which the world would never have seen a Galileo, a Columbus, or a Martin Luther—is to be found frequently struggling for existence in the bygone historic and relatively recent periods of medicine; but of late years only has that spirit become absolutely dominant. No amount of past achievement, no perfection of intellectual brilliancy in a teacher, no universality of belief in any particular man's endowment, no humility of hero worship, will save any opinion, any creed, any statement of alleged facts, from the critical revisal of the humblest and newest worker in the field, or protect one or other from inevitable destruction, if that revisal detect a flaw. And so the day is gone by when honest medical men could be found the active or passive supporters of flagrant absurdities, absurdities made decent and becoming by high-placed assurance of their wisdom. When that remarkable sovereign William III., in so many aspects advanced beyond the spirit of the age he lived in, strove to discountenance the superstition of touching for the evil, he was almost the solitary man of mark in his dominions who at once felt the stupid inanity of the practice, and dared run counter to the popular tide of folly. On one single occasion he is reported to have yielded to importunity, and to have laid his hand on a patient; but on that occasion he at least soothed his moral and intellectual conscience by uttering the remarkable words, "God give you better health and more sense!" "Theologians of eminent ability and virtue," says Macaulay, "gave the sanction of their authority to this mummery"—a fact at which the distinguished historian does not appear to profess any particular marvel; but he does signify his wonderment that medical men of high note were to be found among the supporters of the delusion. And yet, as long as the system of authority in matters of opinion prevailed, how could it have been otherwise? While certain sections of the Church consigned even William himself to perdition, by stigmatizing him as an infidel because he refused to believe that the touch of a particular finger could destroy a constitutional disease, it seems well conceivable that the struggling disciple of Æsculapius might not, simply and alone, have felt it inconvenient and dangerous to expose the imposture, but that his intellect might have been at length mastered by his fears, and that he actually ended by believing in the folly at which his common sense and experience originally revolted. And if things are now changed, if the medical profession as a body stands as the practical bulwark of the present day against superstition of all kinds, the reason is, because they have shaken off the tyranny of authority, because they have learned to think for themselves, because they have recognised and utilised the elevating lesson conveyed in the sagacious and eloquent words of one of the greatest English prelates, Bishop Hoadley:—"Authority is the greatest and most irreconcilable enemy to truth and argument that this world ever furnished forth. All the sophistry, all the colour of plausibility, all the argument and cunning of the subtlest disputer in the world, may be laid open and turned to the advantage of that very truth which they designed to hide or to depress; but against authority there is no defence. It was authority which would have prevented all reformation where it is, and which has put a barrier against it where it is not."

Such, it appears to me, Mr. President, are some of the essential causes of modern progress. The greatness of medicine appears to be indicated in the very character of the conditions that make the groundwork of that progress. But is that greatness felt and acknowledged by the world without? To the consideration of this question I propose to devote the remainder of the time that we shall be together to day. Among those beyond ourselves who profess to think at all on the subject of medicine a very general opinion prevails that it is a study of limited scope, but feebly exercising the mental powers. The uncertainty of physic, registered even in professional aphorisms, is appealed to in vindication of the opinion that it holds a low place among the various branches of human knowledge and inquiry. Its practice is disparagingly contrasted with that of the bar, which, it is averred, calls into play all the higher intellectual attributes, and requires besides the gift of ready expression—nay, of eloquent speech—that these attributes may be employed with the maximum amount of advantage. But, above all, the lay reasoner, who slights medicine, fancies he has settled its intellectual claims by affirming that it is not a science. Not a science! True and false. True, unquestionably true, if we limit the term to systems of knowledge so perfect that from first principles we can reason down to individual facts prior to the actual observation of these, and then by the facts themselves when observed, demonstrate the absolute truth of the principles which enabled us to foresee and predict them. This

prescient faculty, which is the familiar power of astronomy, does not appertain, and conceivably never can appertain, to medicine, and in this sense medicine lays no claim to rank and title as a science. The leading character of astronomy—the faculty of predicting individual facts as well as classes of facts—lies beyond our sphere. Let us see how near to this *Ultima Thule* of perfection we can reach. We begin by observing facts indiscriminately. We find that these, by reason of certain affinities and resemblances, certain repulsions and differences, subdivide themselves into natural groups. By scrutiny of individual facts forming these groups, we find certain of the number distinguishable by special characters from the rest, and arranging themselves in sub-groups; and by continuing this process an indefinite number of times, varying with the nature and complexity of the original ground from which we started, we reach the individual facts again. By comparing groups and sub-groups of facts together, we succeed in tracing a clue to their alliances and repulsions; and, once possessed of the clue, we can by its aid predict with very considerable probability the future modes of grouping of similar facts under similar conditions. But we cannot do this with surety, and we cannot predict for individual instances. For example, we have learned to foretell, from the observation of masses of cases of pneumonia, from successive groupings and sub groupings of these, that under given conditions of age, sex, constitution, season of the year, antecedent health, mode of treatment, &c., the rate of mortality will stand at a certain percentage; but we cannot even approach to certainty in determining what place a given individual shall hold in his class—whether he in particular is to live or to die. Here we are reduced to the calculus of probabilities, and prescient science is at an end. But upon the same reasoning title may be refused, though with less absolute cause, to chemistry. The reaction of bodies cannot be always predicted prior to their being brought into contact out of general principles. If all forms of inductive knowledge are to be refused the rank of sciences, we must accept the sentence for medicine, and content ourselves with such comfort as may be derived from the excellence of the company in which we fall.

Now, in point of fact, though this decision against the claims of medicine to the title of a science is very much the settlement of a baseless dispute,—a dispute about words,—it leads, I cannot help thinking, to an important issue—to more than one important issue. Because medicine is not a pure science after the manner of astronomy, the further conclusion is jumped to—that it has no claim to a high place amongst intellectual pursuits at all; that its attainment is easy; that its cultivation requires but little peculiar training of any kind, and none of a lofty order. All this appears clear to the public mind and to official shrewdness; and hence follow some curious consequences. Unless on this ground, it appears to me impossible to understand the leaning of Government repeatedly exhibited to hand over to the charge of persons without medical education the conduct of public inquiries in their essence purely medical. How else can we explain the fact that when, some years ago, the Horse Guards—better things exist there now I know—determined that there should be a statistical investigation into the health of the army, the management of the inquiry was entrusted—to whom? To a captain in the service, whose education and antecedents—estimable officer though he doubtless was—could scarcely have been of a kind to qualify him for a searching scrutiny into pathological facts. And how else than on the notion that medicine is not a science, and that its pursuit demands no scientific training, can the recent singular attempt of certain legal members of the Legislature be explained—an attempt by which, if successful, skilled medical opinion would henceforth and for ever have been excluded in cases of lunacy? True, this is not the overt motive; but that it is the real and underlying, though hidden, cause, there can, I think, be little reasonable doubt. The more ostensible, indeed the actual, avowed, main reason is this: It is said that the contradictions of medical experts *inter se* are so constant and so flagrant, that jurymen are likely rather to be led astray by the conflict of their opinions than guided by the clearness of their technical knowledge; that the task, puzzling enough to an ordinary jurymen, of weighing and combining facts and interpreting clearly undisputed professional evidence concerning those facts, becomes impossible if, in addition to all this, he be called upon to choose the true, or the nearest to the true, amongst a host of irreconcilable dogmata, and, as elements of, and guides to, this choice, be required to determine the relative credibility and relative scientific competency of opposing skilled witnesses who hold opinions mutually subversive. It may be conceded in honesty, that here is a difficulty; but the *onus*

probandi that this difficulty makes the chances of the jurymen failing to reach the right conclusion greater than it would be without such conflict of opinion, rests with those who oppose skilled testimony. Now, they have never made even a reasonable approach to proving this. And those members of the bar who not only take this view hostile to medical opinion, *quoad* lunacy in the abstract, but urge its practical adoption, appear to me to have overlooked two most important consequences of such adoption. They forget that witnesses as to matters of fact—how often have we all of us noticed this in courts of justice!—witnesses to matters of fact, thoroughly honest, conscientiously desirous of stating to the letter that which they believe to have impressed their senses, often contradict each other; simply because observant power and the conditions under which it is at one and the same moment exercised by different people, more or less widely vary. If the bar were logical, they should then plead that witnesses as to matters of fact might henceforth be silenced. They forget, too, that the very arguments used to prove the inapplicability of scientific evidence in matters medical, apply with equal force to skilled opinion in every form of legal inquiry. Who has ever been present at investigations into the causes of a railway accident, without listening in bewildered astonishment to one learned engineer pooh-poohing with patronizing scorn the plain statement of the equally learned engineer on the other side? Have you ever chanced to hear architects disputing themselves in scientific opinion concerning the foundations of a house? I have. Did the architects and surveyors subpoenaed in the case of the Tottenham court-road fall of houses, some few years since, absolutely agree in their opinions as to the mechanism of the catastrophe? or did they contradict each other, not only as to matters of mechanical principle in building, but as to the apparently simplest and pettiest details concerning brick and mortar? Had the forensic crusade against scientific opinion in cases of lunacy proved finally triumphant, then should, as the logical consequence, all forms of skilled testimony be henceforth banished from courts of justice. And again, the lawyers forget, that if ever there was an instance in which the *tu quoque* argument could be used with crushing force it is this. What! they who never can agree, who differ in points that to the common sense of mankind seem plain and self-evident—they, of all men, deny the worth of skilled testimony altogether on the part ground of nonconformity in the opinion among those giving it! Listen to a single instance of the degree to which the opinions of the most eminent interpreters of the laws may clash. A man enters into a contract of marriage. Shortly after the contract has been duly and legally made, he is seized with hæmoptysis of the gravest character. All the symptoms of rapid pulmonary consumption follow. He declines to marry on the ground of this change in his health. The lady proves recalcitrant, and eventually brings an action for breach of promise. The medical and lay evidence goes to prove that the defendant could not proceed with the marriage without probably inflicting mischief on himself and possibly even endangering his life. The jury accept this view, and find a verdict in his favour; absolving him, in fact, from his engagement. But the judge reserves a point of law after this fashion (and there is not a more learned judge adorning the bench than the judge before whom this case was tried)—“It being fully conceded, through the finding of the jury, that the defendant would have jeopardized his life by fulfilling his contract, was he legally justified by that danger in withdrawing from its performance?” The question was sent up for decision to the court above; there the presiding judges differed. Subsequently the moot point was referred to the thirteen judges, and in this learned conclave the difference of opinion reached the conceivable maximum; for not only were six of the number for the defendant and six against him, the presiding judge giving his casting voice, but the individuals of each group of those who agreed on the main issue disagreed as to the grounds leading to their agreement. The arguments by which the learned persons supported the same opinion not only seriously differed, but in some instances were mutually destructive. The very argument which led one judge to the conclusion he adopted would have led another, adopting on different grounds the same conclusion, to reject it. And these are the men who would silence medical opinion in courts of justice on the ground that it is not always consistent. Now, it is not a matter of real importance to the issue; still it is not unworthy of passing notice how small is the claim of the bar to deal severely with the imperfections of medical knowledge. They find it a hard task enough to master their own written code of doctrine; nay, they often even fail in this. They might, then, it appears to me, have a more sym-

pathizing regard than they habitually display for men whose sphere lies in the unwritten secrets of physic.

See the difference in the problems with which we have severally to deal. We are called upon to penetrate the obscurest mysteries in the creation, the most entangled complications in the most elaborate of God's works—the diseased actions and conditions of the bodily and mental man. Painfully toiling step by step at phenomena, which, to our limited ken, are perpetually clashing with and veiling each other, assuming new forms—chemistry, physics, and special dynamism associated in the most subtle combinations; they deal in the main with what they themselves manufacture, the laws of the statute-book and formal precedents, things that are marked on the surface and deeply into their substance, with all the relative pettiness of human productions, and yet they are perpetually warring in their interpretation of those very small matters. We have to discover the hidden, the unknown, the uncontrollable; they to interpret the obvious, the known, the conventional; and yet to their own deficiency in the performance of this comparatively easy task, they are proverbially purblind, whilst to the least trip on the part of those who deal professionally with the obscurities of physic they are lynx-eyed. And there is a third contradiction to which the forensic logic that would exclude medical opinion in the case of alleged lunatics directly leads. Inasmuch as a conclusion must be drawn by the medical untrained jury from the facts, and without any aid in the interpretation of those facts from persons whose previous business in life is to study such facts, the bar become partisans of the general doctrine that the man who knows least about any given subject is best able to form a sound opinion concerning it. How this *reductio ad absurdum* is to be evaded, even by legal sharpness, I confess appears to me impossible to imagine. If it be urged, that in many instances of alleged lunacy, where the legal question of competency to manage affairs is an essential question, the case is often a complex one, other matters besides psychical peculiarity being mixed up with the inquiry; if it be pleaded that there may be and often are circumstances lying without the pale of purely technical interpretation which may be as clear to ordinary men as to those specially trained in cerebral pathology, and which furnish their contingent to the total judgment; if this be urged, as it is known to be urged in the very highest quarters, the answer is easy; for, obviously, any argument to be found here simply amounts to this, that one important element in a verdict is to be neglected because other evidence besides itself exists and may be utilized. Another objection made to the admission of skilled opinion in cases of lunacy is that medical observers, having arrived at certain crude disjointed theories concerning insanity, must perforce take certain views in individual cases accordant with those theories, and that consequently their argument is assured in the form of actual advocacy before the facts. But in the first place, this objection, if well founded, would be equally fatal to the evidence of professional experts of every class; and consequently one of two things—either the facts are clear, self-evident, well-defined, when theory would not be wanting, or the facts are obscure, entangled, and imperfectly defined, when the theory, if wrongly obtruded by the medical witness, will prove totally without influence on the minds of the jury. To this fragmentary notice of the subject of skilled testimony in cases of lunacy I must limit myself on the present occasion. I have merely glanced at some, without even remotely pretending to have exhausted the consideration of any one, of the numerous fallacies of the opponents of such testimony; but I think I have said enough to make it at least strongly probable that the commonly paraded objections would not have taken the form of practical and active opposition had they not been sustained and encouraged by some form of support in the back-ground, and that support I believe to be furnished by the prevalent notion, probably specially prevalent in the legal profession, of the non-scientific or feebly scientific character of our pursuits.

In its general relationships to the march of civilization, how is medicine regarded by the lay world? First let us ask ourselves what are likely to be the claims of medicine in regard to the civilizing process, and what, in general terms, may be set down among its actual civilizing achievements? It cannot, apparently, be questioned that civilization has been the highest where intellectual development has obtained its maximum. This seems to be predicable not only of different countries compared with each other, but of the same country at different periods of its own social evolution. The history of the world points to a direct ratio between vigour and buoyancy of national intellect, and activity of material advancement; but the vigour of intellect could only be practicably useful in so far

as it is used for the discovery of the truth of things. Truth, then, is the real factor of civilization. In proportion as it is worked out, so has advanced, or will continue to thrive, civilization. Now medicine has, except in the rarest instances, exhibited itself in the van-ground in the honest search after the truth of Nature as she is. To the medical philosopher, truth, irrespective of the consequences to which its discovery may seemingly or really lead, has ever been the ultimate object. In this aspect, the calling he exercises, as also the method and manner of knowledge on which that calling rests, stands at the absolute head of human pursuits. Contrast him with professional theologians, ever dreading lest some new revelation of Nature's ways may falsify some cherished dogma, and consequently often exercising their fullest influence to thwart the demonstration of those laws which the Creator has designed for the regulation of the universe. Remember Galileo! See of what dignified, honest life, medical work is composed in comparison with that of the professional advocate, who starts on his career with the fact staring him in the face that, on the doctrine of chances, one half of his future years must be passed in advocating error, and making the worse appear the better reason. What intellectual training is that of the medical observer compared with that of the diplomatist, the quality of whose aspirations after truth may be gathered from the well-known apophthegm of one of the most successful of the class,—that the use of language was to conceal our thoughts. Compare the medical thinker, again, with some self-elected teachers of psychical philosophy, who, revelling in the wantonness of undenied intellectual power, write "Strange Stories," all well-fitted to shake the foundations of belief, and to destroy perception of the differences between the real and the unreal in persons of feeble organization than themselves.

It seems impossible, then, that in intellectual guidance and in civilizing influence the medical philosopher could do otherwise than hold a high place, or take other rank than amid those bands of inquirers who search for primal truth, fearless of consequences—among mathematicians, moralists, political economists, and statisticians—among the foremost, though often unseen, movers in the march of progress. The civilizing influence exercised by the medical profession in maintaining the bodily vigour of the individual citizen, in suggesting and perfecting the means for his physical development, in securing a healthy mind in a healthy body, in proposing measures of State utility wherever considerations of public health justify his interference, occasionally modifying the penal code in the sense of mercy—these are overt services, to be seen of all. Less obvious, but not less real, is the truth that medical inquirers have been pioneers leading to the general adoption of the numerical method of moral, social, and political inquiries, the utility, nay the necessity, of which appears to have been first seriously felt through the proofs given by Louis, that pathological facts which from their nature seemed to be insusceptible of the process might be made the subjects of numeration, and so forced to furnish inferences of deep practical significance, unattainable by any other means. But I believe the unseen, intangible, underlying influence of the earnest truth-seeking quality of medicine plays a yet loftier part than any of its overt achievements in promoting the great cause of civilization. Still, gentlemen, all this has been little recognised. Within our own ranks there have been few to receive it.

Occupied with effecting a progress, rather than estimating its mechanism, we have said little of our own claims as human civilizers. Lay notices, especially in this country, of the part we have played have been, as might be expected from much that has been already said, to the last degree scanty and unsatisfactory; but a lay historian has at last appeared upon the field who has not only himself felt, but endeavoured to make others feel, that medicine *quoad* civilizing influences, has not existed in vain—has not been cultivated by some of the greatest among ancient and modern observers and thinkers without in some measure influencing cotemporary civilization. Henry Thomas Buckle, in the introduction to his vast proposed survey of the influence of human knowledge on the material progress of man, has assigned to medical philosophy its place in contributing to the civilization that we glory in to-day. To Buckle, indeed, we are indebted for first giving form in print to the idea of a history of medicine in its relationships to civilization; and probably in no pages of his immortal work does that philosophic eloquence which marks its style, that fitness of expression to ideas—such fitness that no word can be changed without weakening the force or making the sense less definitely clear—show forth in stronger relief than in some of those devoted to medical questions. And yet did not Buckle, with all his original genius, with all his keenness in seeing the

reality of things and separating the substance from the shadow, with his faculty of close reasoning, now suggestive, now exhaustive, with his power of judgment at once brilliantly acute and logically sound—yet did not Buckle, with all these high endowments, avoid error in dealing with matters medical. And, as it appears to me, his mistakes are traceable especially to two causes, to which reference has already been made; and further, as his falling into these mistakes furnishes apt enough illustration of the views I have taken, of their ordinary manner of causation, a very few words concerning their actual nature will not be misplaced. In the first place, Buckle's was an eminently deductive mind. In every stage of his argument this quality forces itself upon the reader. He starts from speculative opinions, then seeks for facts whereby to sustain and verify them. Well and good! capital! excellent! so long as the facts are of the kind desired; but when facts of an untoward kind present themselves, either in their own intrinsic simplicity or through their connexions with consequences hostile to the deductive base of operations, as it may be called, Buckle has no compunction about throwing them aside as if they were false or valueless. For instance, hereditary influence is a fact standing inconveniently in the way of certain dogmata: its reality is at once denied, and so one of the plainest experimental results in the whole domain of biology, healthy or morbid, is set at naught. Again, it happens to be incompatible with certain views of the causes of great politico-theological events, that the influence of race should be recognised: race is contemptuously thrown on one side—race, that element in the compound condition of man's existence, to which an unbiassed survey of the events of history gives a leading place among the rulers of destiny, is spurned as scarcely playing even an insignificant part in the drama of the lives of nations.

In the second place, Buckle wanted special knowledge. He had not been trained in the observation of disease at the bedside. He had read of it—he had mused on it—he had not observed it; and it is a beautiful study for ourselves to see to what extent a man of enormous intellectual force can reach in the comprehension of disease without having actually watched it. I know no more beautiful study in this respect than Buckle's book. To this want, combined with the deductive leaning of his mind, must be ascribed his lending his influence to the sustinment of that, as it appears to me, grave fallacy, that pathology may be manufactured out of physiology prior to experience. Technically ignorant and deductively acute, he would have no difficulty in admitting that the combinations of disturbed dynamisms and changed structure, which make up disease, might be framed *ad libitum*, prior to experience, out of elementary data of healthy anatomy and physiology. The experience of a few weeks in the wards of an hospital would have taught him a different lesson. He would have recognised that, all-important though physiology be, its scientific function, *quoad* its pathology, is a humbler one than that of creating; he would have found that physiology supplies a standard of comparison for morbid conditions—that it may furnish explanations of those morbid difficulties as they arise, and that it may suggest subjects for clinical inquiry; but he would have found that creative faculty it has none. Still, in glancing at these shortcomings of Buckle in respect to certain points of medical logic and philosophy, let us not forget our debt of gratitude towards him. It is a singular fact—I make the statement defying contradiction—it is a singular fact that the deep influence exercised by Bichat on the advancement of scientific medicine—and on the sciences of observation in general—has never been so clearly defined, still less so eloquently expressed, by any medical writer as by Buckle; nor has the genius of Hunter ever received a nobler tribute than that raised to his memory in the fervid pages of the lay historian. Here, Mr. President, is not the place to chronicle the life of Buckle—to analyze his original gifts of mind—to marvel at the variety and profoundness of his scholarship—to hold up for the well-nigh worship of the young his power of concentrated and enduring toil, his self-denying existence of student seclusion—to sympathize with his scorn of sycophancy to the accidentally high-placed who have greatness thrust upon them—to ring the echoes of his Saxon devotion to glorious liberty—or to mourn in bitterness of grief over that untimely death, fatal to the completion of perhaps the grandest literary effort ever conceived by the mind of man. But this is the legitimate place to speak of Buckle in the aspects a moment since referred to. Buckle has in truth bound himself to us; he has bridged over the chasm that separated the medical from the rest of the scientific world; he has made clear to all whose understanding is not thoroughly obtuse that there is something lofty and ennobling in the study of the human structure in health and

disease—that it involves forms and varieties of inquiry almost co-extensive with human knowledge, both giving and receiving in all conceivable directions.

Finally, Mr. President, I have endeavoured thus to show to what causes the evident progress of modern medicine is substantially and in ultimate analysis due; and it appears, from the limited survey undertaken, that these causes are of the most promising kind for the future of our science. Medicine has, in truth, not advanced of late years from the exceptional achievements of any single individual gifted with extraordinary powers. Were it so, the continuance of progressive movement could not legitimately be looked for, as Providence but rarely sees fit to create specimens of the highest attainable power. No; our advancement has sprung from the substitution of one true for many false systems of duty—of one true system, which is capable of being efficiently wielded by that multitude of men, no doubt with well-marked intellectual aptitude, honesty of purpose, zeal of character, and vigour of will, who are day by day added to the ranks of physic—earnest workmen for humanity's good. And I have also produced some evidence that the importance of medicine as a portion of the vast edifice of human knowledge is beginning, not only to be vaguely guessed at—to be lightly and passingly touched upon—but to be seriously and systematically stated by laymen of the deepest reach and most brilliant quality of thought. By this recognition of the nobleness of our studies, we are raised in the social aspect, and a worthy and kindlier reciprocity of scientific interest between ourselves and the world of other professions for ever insured. Our civilizing mission is acknowledged and proclaimed; and so when we look around on other pursuits, and watch the men engaged in them—while we admire the toil and sympathize with the toilers, we need feel no discontent with our own; we need not regret that we fill a place in the band of those who strive to lighten the mighty weight of physical and mental woe that afflicts our kind; but with something of the earnest and exalted pride that in his art animated a Correggio, each of us exclaim, as we ponder over the achievements of our years, "And I, too, tread the paths of physic."

On the motion of Professor Bennett, seconded by Dr. Bowes, a vote of thanks was unanimously accorded to Dr. Walshe.

The Report of the Medical Benevolent Fund was read by Mr. Toynbee, which stated that the total amount of bank stock standing in the names of the trustees was £4450. The number of annuitants was twenty-four, seventeen of whom received £20 a year, five £15, one £13, and one £10. The sum distributed during the past year in donations was £967, which had been divided among ninety-seven cases.

The Report was unanimously received.

After a brief pause, during which an excellent luncheon was served to the numerous assemblage, a paper was read, of which the following is an abstract:—

THE SUCCESSFUL TREATMENT OF ANEURISM BY FORCIBLE FLEXION.

BY ERNEST HART, ESQ.,
SURGEON TO THE WEST LONDON HOSPITAL.

Soon after I had the honour, in a paper at the Medico-Chirurgical Society, of introducing to the profession in this country the method of treating popliteal and the like aneurisms by forced flexion, I was much gratified by observing that, at a branch meeting of the British Medical Association, a distinguished provincial surgeon—Mr. Prichard, of Bristol,—having taken note of that communication, had essayed the method, announced the success of it in his hands, and stamped it with his approbation. Now that this method has been for some short time on its public trial, and that it has entered—I hope I may venture to say it—into the recognised practice of surgery, a very brief *résumé* may not be unacceptable to the Association of what I take to be the principles of its action, and of the results of its application.

The cases in which I am cognizant of its having been successfully employed are now ten in number. They are all cases of aneurism of the extremities, for the most part popliteal. Indeed, by the nature of this method of treatment, its application is confined to the extremities. But here the general law of position of the great arteries to some extent favours it. Aneurisms in the limbs are most commonly seated on some one of the larger arteries of transmission: these follow a definite law of location. Arteries of distribution do not follow any definite course; they, however, are more rarely the seat of aneurism.