current at all, it can only be in the act of closing, thereby changing the normal second or flap-sound in the manner just described. In many cases of anæmia, the second sound is not altered, and, in not very few, neither is the first sound, except when the heart is excited; these differences point to slight normal varieties of structure which become evident only under the test of disease. But, in all cases of simple anæmia, the normal ventricular sound may be heard distinctly in the cardiac region, and in many accompanied by a murmur, loudest at the base.

In certain cases of extensive disease of the valves, both obstructive and regurgitant, I admit that murmurs are produced which, to a great degree, supersede the natural sounds of the heart. This is when the lesions so much derange the mechanism of the heart as to convert the normal shock-sounds into vibrating current-sounds. For example, in obstructive disease of the aortic orifice, the systolic action meets with such a vibrating resistance to the blood which it expels as to react on the tightened ventricle and make it vibrate in unison with the obstacle and current in front; thus the normal shock-sound originating in the walls and valves merges in a noisy current-sound or murmur, whose vibrations pervade the whole cardiac region, but still are loudest in the direction of the current. In extensive regurgitant mitral disease, a like vibrating current goes back through the mitral orifice, masking or superseding the normal first sound by a loud murmur transmitted through the cords to the apex below the left breast. In more moderate degrees of this lesion, the normal double sound may still be heard at the base and over the right ventricle.*

In regurgitant disease of the aortic orifice, the normal second sound is absent or impaired, being replaced by a diastolic murmur. The reason is obvious, the valves can no longer tighten; but even then the simple flap of the pulmonary valves is often still heard to the left of the mid sternum. In that remarkable and not uncommon lesion, button-hole contraction of the mitral orifice, which is both obstructive and regurgitant, the long systolic and presystolic murmurs often supersede all normal sounds at the apex, but at the base the normal sounds are usually still more or less distinct.

In conclusion, I have only to repeat the inferences with which I terminated my former communication; and, to make them more intelligible, I will adopt some expressions borrowed by Dr. Leared from French writers.

The normal sounds of the heart are of the nature of *shock-sounds*, which can be neither produced nor imitated in liquids alone. These sounds can be readily imitated by the tightening of woven fabrics or animal membranes under water. Similar sounds constantly accompany the forcible contraction of various muscles in man and other animals. It has been demonstrated by experiment that a similar sound continues to accompany the contractions of the ventricles of an insensible animal without the presence of blood and without the action of the valves. Although it is thus proved that the essential cause of the first sound of the heart is the muscular contraction of the ventricles, it also appears from experiments, and is conformable to reason, that the presence of the blood and the action of the auriculo-ventricular valves contribute to its loudness and clearness by increasing the strength and extent of the sonorous vibrations.

The second sound of the heart has been clearly proved by experiment to be caused by the tightening of the arterial valves by the recoil of the blood in the great arteries at the moment of diastole.

Murmurs are essentially *current-sounds* produced by some modification of the direct and usually silent course of the blood in the heart or blood-vessels. As they can be exactly imitated by the flow of water through an India-rubber tube, there is no reason to doubt that they are produced in a similar manner by the passage of the blood through or across a form of solid, which throws it into sonorous vibrations. This may occur at any of the orifices of the heart or in the blood-vessels. When in the heart, a murmur may either accompany or replace the normal sound. In cases of moderate disease and of anæmia, it merely accompanies it; but, if the lesion causing it be such as seriously to damage the structures, it may impair the normal sounds, both valvular and muscular. Further, a strong grating murmur at an orifice may not merely mask the normal sound, but may assimilate its vibrations to its own and convert the whole sound into a loud murmur.

* Dr. Broadbent says: "I have long been in the habit of noting the occasional presence of a first sound, together with a systolic mitral murmur, as a favourable element in prognosis." My experience is the same, and extends, as a general rule, to most structural diseases of the heart and lungs, to this effect: lesions are serious, not in proportion to the new signs or symptoms which they cause, but more in proportion as they supersede the signs and symptoms of normal structure or function. I referred to this inference in a paper on the "Prognosis and Treatment of Organic Diseases of the Heart" in the *London Journal of Medicine* for April 1850.

AN ADDRESS

ON THE

EXPERIENCE AND EXPECTATION OF CLINICAL RESEARCH.

Delivered before the Clinical Society of London, on Friday, January 26th, 1877.

By GEORGE W. CALLENDER, F.R.S.,

Surgeon to St. Bartholomew's Hospital.

GENTLEMEN,—If an earnest desire to promote the exact observation of clinical facts should largely influence me as a member of this Society, that earnestness should be intensified by the responsibility I have incurred in venturing to accept office as your President—intensified by the knowledge that your friendly judgment has placed me in a position which demands that, by precept and example, I should plead for precision in clinical research.

If I had only to speak to you in words of thanks, my duty would soon be ended; for I can but say that I value most highly the honour you have conferred upon me. In recalling your confidence, I shall be encouraged in the endeavour, so far as I can, to do it justice.

It has, however, become a custom of the Society that your Presidents should venture from time to time to offer suggestions as to how the objects we have in view may be best promoted; a custom which it grows more difficult to observe as its novelty wears off, and yet—having in mind changes in the grooves of thought, in the method of pursuing investigations, the gradual accumulation of facts tempering and consolidating the knowledge with which we begin each new session of work—one which it seems to me most desirable to conserve. In addresses which have been given from this chair, your Presidents, in a practical way, and with the force of experience (all the more forcible from the clear and thoughtful words in which it has been spoken), have described the duties we owe to a Society which is founded expressly for "the cultivation and promotion of practical medicine and surgery": words which express as could scarce any other the character and scope of our task, for they foreshadow an increase of knowledge; but, however far in advance of the past, a present knowledge which in its future must always be capable of cultivation and promotion. Our work is, indeed, bounded by a horizon which recedes before us; but, in our pursuit of it, as we ascertain and establish facts, and, so far as we can, as we eliminate sources of fallacy, we shall find that we are making progress and are strengthening our position for a further advance.

If I diverge in any way from the steps of my predecessors, who have spoken chiefly on the manner in which the objects of the Society may best be attained, it will be to take advantage of the experience we have thus far acquired, not only for the evidence which it gives of the extent to which we have acted up to the expectation of our founders, but as showing the direction in which we are being carried, and in which our future work promises, I think, to give the best clinical results.

We began by setting up for ourselves a high standard, and, if we have not quite attained to it, we can trace through our *Transactions* an honest endeavour to do so; and we can be sure that we have encouraged—I would venture to say we have practically established—a precision and order in the observation of clinical facts, which by itself is a substantial gain. If we look away from our more immediate proceedings, and if we remember that those who were interested in the formation of the Society are largely responsible for clinical research in our hospital wards and elsewhere, we shall recognise in every direction that an effort is being made to observe disease and its treatment in a more critical and accurate manner than heretofore. In the treatment of wounds, for example, whatever plan is followed (and many are and have been advocated), all have in common the care which conducts them, and which insures that every detail shall be scrupulously attended to. If I may single out one fact in illustration of this, I would remind you that, after being present at, and taking part in, our debate on pyæmia, Mr. Cadge effected a revolution in the results of the treatment of wounds in the Norwich Hospital; above all things, by seeing that the nursing, the cleansing, and the dressing were strictly, and hence successfully, directed. I say work is more precise in hospital practice and elsewhere; for, in using the latter word, I would recall the fact that there are points, such as those of history and of affinity, and the close watching of disease through the course of many

years, which are undoubtedly more fully and more surely elucidated in private than they can be in hospital practice; and I think the Society would gladly welcome the renewal of reports, as they may be termed, such as those brought before us by Mr. Prescott Hewett, and drawn from such experiences. Let me mention, too, in passing, and with reference to precision of observation, that, by close watching of the sick, we are constantly gaining information, as it were, in the by-ways of practice; not, it is true, respecting details of essential importance, but yet information which helps occasionally to stand us in good service. Of such is the fact that a rapid rise of temperature, after, for instance, an operation, may be due to emotional causes of an evanescent, and consequently of a comparatively unimportant, character; and of such is the knowledge of the exact rate of the repair of wounds, by which we can tell the time which a given surface will take to heal. Such, too (and I take shame to myself for being unable to speak with certainty on the subject), should be our knowledge respecting the asymmetry of parts, to which attention has recently been directed in America, with reference more especially to the length of limbs, a detail which, all students of art are aware, received attention from the ancient sculptors.

But, if medicine and surgery are being cultivated with greater care and precision, can it be added that we are thereby promoting our knowledge of the treatment of disease? I turn, perhaps with an undue bias, to surgery. I find that the careful supervision of operations and other wounds has proved, if rest and cleanliness and constant attention are combined, that the most serious hurts are repaired with an absolute certainty (not merely through a precarious convalescence), injuries being recovered from which some years ago would have been considered irremediable. If erysipelas, despite precautions, attach itself to a wound, the close watching of the case now enables us to detect the earliest symptom of the disease, and, twelve or twenty-four hours before the outcome of a rash, to commence the treatment of the affection. Pyæmia has been proved to be a preventable disorder. Skin-grafting—to name some of the facts brought more directly under our notice—has been introduced; Esmarch has described to us his plan for operating without loss of blood: the use of torsion as a hæmostatic; that of various antiseptics for the dressing of wounds; the subperiosteal resection and the subcutaneous section of bones; the surgical treatment of aortic aneurism; the removal of tumours by caustics and by the elastic ligature; the surgical treatment of neuralgia and of local paralysis, have been before us, among other subjects, for consideration, and surely some of them may be regarded as representing distinct gains to our means of treating surgical affections. In nearly every instance, the gain, if any, in treatment has been due to precise observation of the cause and nature of the trouble to be remedied, and to the direction of natural forces for its relief; and, as in most instances of real progress, the means used for controlling these forces, when put before us, are found to be of the most simple kind.

Precise observations of the conditions which cause disease must always be a subject of great clinical importance for critical research. In this direction, the study of material pathology, to a certain point, has been cultivated with success. There are, probably, no facts of greater practical moment than are such, for example, as taught us the connection between rheumatic fever and various changes in the covering and lining of the heart; or those which throw light upon that form of embolism which results from the setting free in the circulation of small particles of fibrine. But there are inquiries beyond those which are satisfied by the examination of diseased structures—those which reach to the causes of disease—inquiries which, amongst others, come within our province where we join hands with pathology in its experimental researches. Why, for instance, should such a degeneration of tissue arise as occurs when cancer is developed? and why should such degenerate cancer exhibit active growth? Or, what are the causes which determine the presence of tubercle? No higher duty can be ours than that which leads our inquiries in this direction, for of such causes at this time what do we really know? And how can we look—discuss each malady how we may, backwards or forwards—for any better treatment of these diseased conditions (save such as comes to us by hazard) until we have learnt more about the causes of those errors of nutrition which lie, in all probability, at the root of the mischief? By inquiries, too, directed to the causes of disease, we may expect to advance in our knowledge of that best of treatment, the preventive—the controlling of the forces of nature, so that disease may be avoided.

There is also good work to be done by the study of the natural history of disease: a part of our programme which will, I trust, be attended to in our future as fairly as, perhaps even more thoroughly than, in our past proceedings. This study is, in truth, that of disease as observed under the most careful supervision, with rigid exclusion of complications from preventable external influences; that is, free from

errors of management—all, in a word, that due watching, rest, cleanliness, temperature, and suitable nourishment can do, and seeing (so far as we can without risk to the patient) how disease comport itself under such conditions: clearing, in fact, the ground for precise observations of the medicinal action of drugs, and for gauging their exact effects.

And this it is which must always be the great feature in our prospective work—the attainment of that knowledge of therapeutics of which, one of our Presidents told us, as a trustworthy science we have only the expectation; and of which he also said that he often wondered how vaguely, how ignorantly, how rashly drugs are often prescribed. I am far from saying that the Society might not have done more with reference to the study of the action of drugs; and yet our *Transactions* are not barren of laudable endeavours to obtain more knowledge of their influence upon disease. If it is a lesser function to discharge, we have at least sifted the reputed powers of various medicines, and have reported them as untrustworthy; and such inquiries have been illustrated by valuable experiences, and have led to interesting discussions. We may remember, indeed, that to this Society we owe the introduction of a discussion extending over several meetings, and setting an example which has been elsewhere followed, and if it have not been so here, it is because it is in the nature of the ordinary work of the Society that the subjects brought forward at its meetings should be freely discussed; and interesting as have been many of our communications, they have often been supplemented by most valuable comments from those who have spoken upon the subjects which they illustrate.

But our *Transactions* show a long list of trials carefully conducted to test the action of various remedies. Amongst such may be mentioned notes on the local effects of opium upon the bladder, the influence of ipecacuanha upon diarrhoea, the value of quinine in pyrexia, the use of salicylic acid and of other antiseptics, the employment of copaiba in ascites, the local effect of the hypodermic injection of strychnia, the use of bromide of potassium in epilepsy, of nitrite of amyl in angina pectoris, the action of iodide of potassium upon tumours, the employment of salicylic acid in rheumatism, cold affusion in rheumatism, and cold baths for pyrexia in phthisis. If, then, we may not have attained to much, I think these examples—and others might be added—show that an attempt is being made to investigate the action of drugs.

When we reflect upon the difficulties inseparable from the work we profess—work which is strictly limited to the record of facts, and of which mere opinions form no part—when we consider the varied phases of disease, and the variable soil in which it acts, it is clear that it can only be by slow degrees that we shall attain to any fulness of knowledge of the means for combating disorders by the use of medicines. Were it not for the encouragement given by the facts thus far ascertained, which prove the influence over disease of certain remedies, and the potent action of drugs upon the healthy organs and tissues of the body, we should, I fear, be disheartened by the cloud of difficulties with which an empirical system, continued through ages, has been allowed to envelope the relations between disease and remedies for its cure. It is to remove this—to base our treatment upon facts—that we are trying to make therapeutics not an expectation but a trustworthy science. If I may point to an example of the system on which each one of us should work, I would instance the recent appointment, by the Council, of committees for collecting on definite subjects an array of facts from which definite conclusions will be drawn. We can each, individually, cast our work in a similar mould. It must be precise, and it must be matter-of-fact. In this spirit, we must try to learn more about the causes of disease, and we shall have a better knowledge of preventive treatment. We must endeavour to learn more about the action of drugs, and we shall know better how to cure disease. Even now there is so much of which we are ignorant, and the field of inquiry is still so vast, that there can be no question as to any dearth of material upon which to energeise our work. That work must, indeed, have led to an infinite advance beyond our present knowledge of the art of healing, before the time shall come when we may slacken in our efforts for the cultivation and promotion of Practical Medicine and Surgery.

It is announced that Dr. Henry Muirhead, of Bushyhill, Cambuslang, has offered to the University of Glasgow the sum of £2,100 as an endowment of a Demonstrator of Physiology in connection with the Chair of Institutes of Medicine. The object of the endowment is the promotion of medical science by the training of young men of suitable capacity to become teachers and investigators of physiology; and, as this training is best attained by actual work in the laboratory and by practical experience in the art of teaching, the demonstrator will be regarded as a teaching assistant to the professor, while he will also be encouraged to pursue independent original investigation, and will be aided by the use of all the laboratory appliances.