able; it initiates, presides over, the reproduction of the cell; in some cases is concerned in promoting its rejuvenescence. We are no longer content to accept as the typical structure of the nucleus of a living active cell an inert vesicle, inclosing a fluid and one or more solid particles. We can recognise in nearly all cases a further structure—a network, coarser or finer, open or close, of threads which pervade the nucleus, and unite the nucleoli to the enclosing membrane. There is no doubt as to the existence of this intranuclear cell. It is even more constant than the nucleoli, for the latter may not always be visible; the network of filaments is seldom absent. First noticed in certain specialised cells, and judged peculiar to them, it is now recognised in cells of every kind, ova, nerve-cells, muscular cells, epithelium-cells, colourless blood-corpuscles, coloured blood-corpuscles of ovipara. As in so many other instances, one is astonished that so obvious a structure should have remained so long obscure.

With this network the nucleoli are connected; indeed, many particles which were before described as nucleoli are now only recognised as thickenings of the network. The network is not inert; it alters from time to time, as do also the nucleoli; it is probably contractile: certainly, changes of shape may sometimes be observed in the nucleus, both within the cell, and when escaped from a ruptured cell. In division, moreover, the filaments and the whole substance of the nucleus undergo changes as rapid as remarkable. Altogether, it is clear that the nucleus is capable of a large amount of independent vital activity. What more natural, then, than to regard it as consisting of protoplasm in no essential point differing from the rest of the protoplasm of the cell? This view (indicated first by Beale, who included the nucleus and protoplasm of the cell together under the term "formative matter") has, in fact, been adopted by Frommann, Heitzmann, Klein, Stricker—histologists who were among the first to pave the way for the general recognition of the reticular structure of the nucleus. They bring the nucleus into absolute continuity with the protoplasm, its filaments prolonged into those of the protoplasm, its membrane little but a closer interlacement of the same filaments, the nucleoli only thickenings of the filaments. Throughout the whole cell-nucleus and cell-protoplasm is a continuous network of protoplasm. Apart altogether, however, from the reasonable doubt of the pre-existence of fibres in all cell-protoplasm, this conclusion is one which, in the face of the striking contrast of chemical nature (as shown by their behaviour to reagents and staining fluids) between nucleus and protoplasm, can in no way be upheld.⁶

Putting aside, then, this continuity with the cell-protoplasm, it is admitted that the typical nucleus is composed of the following parts: (1) a nuclear membrane bounding it externally; (2) a network of fibres; (3) a nucleolus or nucleoli; (4) a clear substance filling the nucleus in the meshes of the network of fibres. 1, 2, and 3 agree, on the whole, chemically with one another, although optically they present differences; they differ entirely, both optically and chemically, from 4; they appear to be more solid, 4 more fluid; they are, therefore, grouped together under the name *nucleoplasm*, or *karyoplasm*, whilst the clear substance is termed the *nuclear juice*, or *matrix*. When a nucleus divides, all its karyoplasmic parts blend; the distinction between them is lost; they are transformed into a system of contorted fibres, imbedded in clear matrix. These fibres undergo remarkable changes, apparently spontaneous, which result in their separation into two groups, and the reformation of the structure of a typical resting nucleus from each group. Into the details of these remarkable changes I do not propose here to enter; I refer to their occurrence only as showing the individuality, the independence of the nucleus. When we believed that a nucleus divided simply by undergoing a process of constriction into two, it was conceivable that the constriction might be produced, not by any exercise of spontaneity on the part of the nucleus, but by the presence of the surrounding protoplasm. Such a notion is no longer tenable; the nucleus possesses an independent activity, as well-marked as that of the protoplasm itself.

It seems probable that we are on the eve of yet further discoveries with regard to the structure of the nucleus. It has,

indeed, been shown by Balbiani that, in certain cells of chironomus-larvæ, the nuclear filaments are transversely marked at short and regular intervals, as if made up of a succession of particles cohering together in series. And it may well happen that this proves a general feature in the structure of nucleoplasm, and that the changes which occur in division may be ultimately referred to rearrangements of their constituent particles. It is even conceivable that we may some day gain an insight into the nature and mode of action of the forces which cause such rearrangement, and even discover means to modify or restrain their action. No doubt, the idea is yet chimerical, but it is almost justified by the prodigious advantages which have of late been made in our knowledge of the structure and changes of the cell-nucleus.

It would appear then, that the nucleus of the cell is not the structureless body which we, at one time, thought it; on the contrary, it has a manifest and complicated structure, which has not even yet been fully unravelled, and, in the performance of its function, it exhibits changes of structure which, until lately, had remained entirely unsuspected. Regarding the body of the cells, we must be less positive; that, in special cases, it is differentiated, is undoubted; that, in all cases, it is reticular, is not proved, although theoretical considerations render such a structure not improbable.

I think we should be cautious of accepting conclusions which, however much supported by theory, do not rest on the basis of incontrovertible facts. Some histologists and pathologists would carry the reticular theory farther even than the limits of the cell. Continuing the protoplasmic network into the intercellular substance, they pervade all parts with it, binding together the separate cells of the organisms with a web, whose closely linked threads are finer than the finest gossamer, conveying the marvellous properties of protoplasm—its formative power, its chemical activities—to the most remote recesses of the organism. Inspired with enthusiasm for a new theory, they do not hesitate to carry it beyond the point which properly observed acts justify. We ourselves should have a care, lest, enticed by the facility with which certain phenomena seem to be explained, we follow too far along paths which lead away from the track of conscientious observation and just surmise.

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF PATHOLOGY,

At the Annual Meeting of the British Medical Association, in
Liverpool, August, 1883.

By T. HENRY GREEN, M.D.,

Physician to Charing Cross Hospital, and Senior Assistant-Physician to the
Hospital for Consumption and Diseases of the Chest, Brompton; President of
the Section.

GENTLEMEN,—The decision of the Council of our Association to confer the great honour of presiding over the Pathological Section upon one so junior as myself, I venture to regard as indicative of a desire on their part to encourage those of us who have still time and opportunity to prosecute more particularly the study of that important subject the annual discussion of which we meet to-day to celebrate. In accepting this position, however, I cannot but feel that, inasmuch as pathology is so inseparably united with, and so largely dependent upon, clinical medicine, the success of our meetings would have been better promoted by the guidance of one to whom age had given a riper knowledge and a sounder judgment.

Upon advance in our knowledge of pathology must necessarily depend advance in the very aim and object of our art—the rational treatment of disease. A survey of the past affords, I think, good ground for congratulation. During the last few years much has been done, much that will be enduring, much that will serve as firm bases upon which we may build in the future. In no department of medicine has progress been so marked and sure. Than such success, what better stimulus could we have to increased and persevering labour?

In considering the prosecution of our work, the number and the complexity of the pathological problems which still remain to be solved; some of which, from the gravity of the issues they involve, are, even at the present moment, assuming so much importance that both the profession and the public are anxiously waiting for the truth, we cannot but recognise the difficulties with which we are surrounded, the responsibility which rests upon our decisions,

⁶ There is not even a probability that nuclei can be formed, *de novo*, out of protoplasm, or from anything but a pre-existing nucleus. The doctrine of free cell-formation, which originated in the days of Schleiden and Schwann, and which supposed the deposition of nuclei in a homogeneous blastema, and the formation of cell-substance around them, has long been given up as a general principle, although the new formation of nuclei, independently of pre-existing nuclei, has long lingered. It would seem, however, that it must now definitely be surrendered. All recent researches on cell-formation and multiplication point to the fact that the dictum of Virchow with regard to the cell must be extended to the nucleus: *Omnis nucleus a nucleo*.

the attitude of scientific criticism which it becomes us to assume.

Of the threefold source of our knowledge of disease—observation after death, observation during life, and pathological experiment—that first named has undoubtedly received the most attention. Morbid anatomy has ever been a favourite study with the junior workers in our profession. It is natural that it should be so; for our work is more capable of demonstration, and for the most part beset with fewer difficulties, than are the other methods of investigation. Let us beware, however, lest we attempt to give to it too great a prominence. I cannot help thinking that there is at the present time rather a danger of erring in this direction. Our literature teems with minute histological details, and our knowledge of the structural alterations which accompany disease would appear to be well-nigh complete. We are thankful that it should be so; and medicine will ever recognise her indebtedness to the long line of workers who have contributed, and are still contributing, to this end. But are we not too prone to be satisfied with our histology? Forgetful of the fact that the structural changes are not the disease, but only one of the many results of abnormal physiological processes, do we not allow these changes to unduly influence us in our estimation of the diseased state? We have attempted, perhaps, to look at and to explain disease too exclusively from an anatomical standpoint, and have thus incurred the danger of losing sight of factors in the process which may be of much greater importance than the revelations of the microscope. We are now beginning to learn, and to appreciate in all its important bearings, the fact that different diseased processes may produce the same structural change; that histological similarity does not necessarily imply pathological identity.

What has come, for example, of our anatomical conception of tubercle? What confusion could have been greater than that which has been existing during all these years from attempting to frame a definition of tubercle upon a purely anatomical basis? The impossibility of such an anatomical limitation has been gradually unfolding itself. As long ago as the oft-quoted debate at the Pathological Society of London in 1873, some of us ventured to question whether what was then known of the pathology of tuberculosis warranted an anatomical definition of the lesions. Five years later, Professor Klebs believed that he had demonstrated the existence of a microphyte in tubercle, to which he attributed its infectiveness. More recently, Dr. Martin, of Paris, proved experimentally that, in the lower animals, lesions produced by the introduction of irritating non-inflammatory particles may be precisely similar histologically to those produced by the inoculation of true tubercle; and now comes what appears to be still more conclusive evidence—the discovery by Koch of the tubercle-bacillus. The results of this tendency are also seen in the classification of disease, markedly so in the case of phthisis. The attempts which have been made during recent years to subdivide this disease into such varieties as “pneumonic,” “tubercular,” and “fibroid” phthisis, have had, perhaps, only a doubtful pathological justification; they have, on the whole, tended to mislead, and, with increasing knowledge, such divisions become increasingly untenable. In Bright’s disease, again, the two types of kidney-lesion represented by the terms tubular and interstitial nephritis have served as bases for the subdivision of a disease, the differences in the characters of which are probably due rather to differences in the intensity and duration of the diseased process than to any differences in its real nature.

There is another matter connected with this branch of our subject to which I should like to allude—the study of disease as we meet with it in the *post mortem* room, apart from minute histological investigation. It is unnecessary, in such a meeting as this, to insist upon the importance of *post mortem* examinations. Of these, the greatest aids to education in diagnosis we possess, it may perhaps be said that he who has completed the largest number of his cases by necropsy, is likely to be the best practitioner. The value of *post mortem* examinations in this connection is so generally recognised, as scarcely to need comment. But, in order thus to utilise our cases, we must be familiar with the naked-eye characters of disease as seen after death. We must be able to recognise and to appreciate the significance of macroscopical changes. A superficial investigation is all that most of us have the opportunity of making; but it is for the most part sufficient. It will make us pathologists; and, although most of the work will be done by those who have time for more comprehensive study, we must remember that advance in our knowledge of disease is a gradual process, and is contributed to by the little workers as well as the great. What, therefore, I would venture to encourage is, an increasing study of the

macroscopical characters of diseased structures. I do so because, in the education of our students at the present day, when a knowledge of minute changes is so increasingly demanded, I think I observe rather a tendency to allow morbid histology too much to replace morbid anatomy. There is, perhaps, a danger of the medical student spending too little time in the *post mortem* room, and prosecuting his work too exclusively in a room supplied with section-cutters and various dyes. I shall not be accused, I believe, of underrating the value of the study of microscopical changes, but I wish to insist upon the importance of endeavouring to familiarise ourselves with the macroscopical alterations with which they are associated.

The value of pathological experiment as a means of increasing our knowledge of disease, it would perhaps be difficult to over-estimate. How much it has done in the past, and how much may we expect of it in the future. What, for example, could be more important than those investigations of Cohnheim which prove conclusively that when blood-vessels are injured either by direct irritants or by the temporary deprivation of their arterial supply, they become so altered that they are no longer capable of retaining the constituents of the blood? This constitutes the very basis of that large class of morbid processes which come within the category of inflammation. Or, again, than those of Koch, Davaine, Burdon Sanderson and others, on the nature and causes of septic infection, and of Koch on tuberculosis? And yet experimental pathology is at present only in its infancy, and the obstacles to its development in this country seem to be almost insurmountable. Condemned by the well-meaning but ignorant public, restricted by the legislature, with little or no pecuniary encouragement, even its utility questioned by some clinical pathologists, on the ground that deductions from experiments on the lower animals are not applicable to man; a means of investigation which is probably capable of giving us a clearer insight into the nature of many diseased processes than is any other method, seems to be well-nigh in danger of extinction. In saying this, I do not wish to be misunderstood. In such experimental methods the value of results must depend upon the skill and special knowledge of the experimenter, and success is only to be acquired by a long and careful training. It is a means of investigation, from the very nature of it, likely to be prosecuted with advantage only by the few. We should see to it, therefore, that every encouragement is offered to the few to do for us that all-important work which we are unable to do for ourselves. We would not blame the legislature for restricting promiscuous experiment, but we would ask them to recognise the great value of this method of inquiry, and doing so, to offer, not only greater facilities, but some practical encouragement to those who are qualified and willing to work. If such were the case, we should be spared the opprobrium of finding English names so conspicuous by their absence in most that relates to this branch of pathology.

Great, however, as is the value of pathological experiment, we must never lose sight of the fact that, its results must be submitted to the criticism of the clinical pathologist before they can be accepted as applicable to disease. Experimentation on the lower animals, however great may be the precautions taken to exclude error, is open to so many sources of fallacy, that even when its results are utilised merely for the purpose of explaining disease in the animals themselves, much uncertainty is unavoidable. But when these results are applied to disease in man, the sources of fallacy become so indefinitely multiplied, that deductions from them are almost valueless unless the conclusions arrived at are more or less in accord with those justified by other methods of observation. If this be so, and I think no one can gainsay it, how exalted among pathological methods becomes the position of clinical investigation. It is the province of the clinical pathologist to utilise the results both of experimental and of histological research, for the purpose of helping him to solve those obscure pathological problems which are daily presenting themselves to his notice. To those who are thus intelligently and studiously investigating disease as it is met with at the bedside of their patients, progress in pathology must ever be largely, if not mainly, owing. Let us, therefore, never be guilty of a tendency to underrate the pathological value of clinical medicine. Clinical facts, remember, are as much facts as are histological or physiological ones, and they can never be lost sight of in pathological generalisations without prejudice to the truth. Let those of us, therefore—and we must ever constitute a large majority—whose work is almost exclusively clinical, remembering the pathological value of our work, endeavour increasingly to aid in the accumulation of clinical facts. Our work is laborious, progress is necessarily slow, we shall probably meet with but little public recog-

nition or reward; but, as we proceed, we shall become increasingly competent to estimate the value and utilise the results of other methods of observation; and we shall have the satisfaction of knowing that our labours are in no small degree tending to further the attainment of that for which we all strive—pathological truth.

In the few remaining minutes of your time that I shall venture to occupy, I should like, if I may be permitted, to exemplify what I have been saying by an appeal to pulmonary pathology. In these days of specialism, such a descent from the general to the particular is, I trust, pardonable. I select diseases of the lungs, not only because they happen to have for me a special interest, but because some of them are just now very prominently before the notice both of the profession and the public.

If we take three such common diseases as broncho-pneumonia, pneumonia, and phthisis, and inquire into our knowledge of their pathology, we shall see how the several methods of investigation have contributed to this knowledge, and how unwarrantable is any attempt to generalise from one method to the exclusion of others. First, of broncho-pneumonia. Our knowledge here appears to be well-nigh complete. Morbid anatomy shows the prominent part played by pulmonary collapse in the causation of the pneumonic process, the inflammatory consolidation being almost exclusively confined to those portions of the lungs in which collapse has taken place. Pathological experiment tells us that stagnation of the circulation, such as occurs in the collapsed lung, so injures the blood-vessels that they become no longer capable of retaining the constituents of the blood. From clinical medicine, we learn that this disease is invariably associated with bronchial catarrh, and is especially prone to occur in weakly children—the two conditions the most favourable to collapse—and that it tends to prove fatal by progressive interference with the respiratory function, its severity being directly proportionate to the extent to which the lungs and bronchi are implicated. The conclusion, therefore, that, in a case of bronchial catarrh, the occurrence of broncho-pneumonia is, for the most part, due to the supervention of pulmonary collapse, is warranted by the several methods of observation we have been considering; and the importance of keeping this pathological teaching in view in the prevention and treatment of this disease, it would be difficult to overestimate.

In the case of simple pneumonia, however, it is otherwise. Of this disease, although we have of late years learnt much—much that is of the utmost practical value—we have still much to learn. The pulmonary consolidation, which constitutes the prominent local lesion, is obviously due to an accumulation of inflammatory products within the air-vesicles; and, judging from *post mortem* examination alone, we might conclude that the disease was a simple inflammation of the lung, giving rise to a varying amount of constitutional disturbance. Such, in fact, is the view that was formerly held as to the nature of pneumonia. But it is not supported by pathological experiment, and it is distinctly negatived by clinical observation. Experimentally, it is found to be impossible, by mechanical or chemical injuries of the lung, to produce that fibrinous exudation which is so characteristic of the earliest stages of the pneumonic consolidation. This, perhaps, in itself is not of much value. The evidence of clinical medicine, however, is overwhelming. A disease, in which there is no relation between the extent of the local lesion and the severity of the general symptoms, coming on with a sudden abruptness, and usually declining with almost equal rapidity, often before the pulmonary consolidation is fully developed, is obviously not to be regarded as a simple pulmonary inflammation, of which the clinical phenomena are symptomatic, but as a general disease, of which the lung-consolidation is the prominent local lesion. This is the present position of our knowledge, surely a great advance on the past, and one which is exercising a most favourable influence upon treatment. But our knowledge is still very incomplete. We have yet to learn the nature of the disease, and the circumstances under which it originates. *Post mortem* examination has, perhaps, revealed most that it is capable of teaching; pathological experiment may still throw some light; but probably to clinical observation, such collective investigation, for example, as is now being carried out under the auspices of this Association, we must mainly look for the further solution of this pathological problem.

And now we come to phthisis, a disease which, perhaps more than any other, has engaged the attention of pathologists; a disease, the very mention of which, in a pathological meeting, is apt to be received with alarm, so endless is the discussion which may arise out of the different views held respecting it. At the same time, the new interest which is given to the subject by Koch's great discovery, and the all-important questions which arise therefrom, will,

I hope, be considered sufficient excuse for venturing on such debatable ground. At the present moment, there is, I believe, no question so much demanding the careful attention of pathologists, as that arising out of Koch's investigations on tuberculosis in their relation to pulmonary phthisis. It is not for me, and it is probably not for many of us here, to say whether Koch's experiments are free from error. In dealing with such complex pathological problems, investigations require to be repeated and varied before the results obtained from them can receive unconditional acceptance; and we must look to our skilled experimental pathologists for a verification, or otherwise, of Koch's results. It is for us, however, when such important statements are made by men whose knowledge and reputation are a guarantee for the value of their work, to give to their statements our careful consideration, and to see how far the conclusions which they necessitate are in accordance with those justified by other methods of research, and not to reject them simply because they may be apparently at variance with existing knowledge.

In the case before us, the statement comes from one of the greatest living authorities on the kind of investigation of which we are speaking, Dr. Robert Koch; and let me add—and surely this must enhance its claim on our acceptance—Koch's investigations have been verified by one of our own countrymen, one who has a special reputation for this kind of work, Mr. Watson Cheyne. Notwithstanding, therefore, that some have questioned Koch's conclusions, and have experimentally arrived at different results, it behoves us, I think, whilst waiting for further evidence at the hands of the experimental pathologist, to submit a question involving such grave issues to other tests, in order that we may, peradventure, by so doing, promote, and even hasten, the attainment of the truth.

Koch's conclusions, up to a certain point, will, I presume, be pretty generally admitted. That the tubercle-bacillus is, at all events, a very constant associate of tuberculous processes is now supported by such an accumulation of evidence, that we are probably justified in regarding it provisionally as a fact. This fact is already being largely utilised for purposes of diagnosis and prognosis, especially in pulmonary diseases, and the work being done in this direction by Heron, Gibbes, Dreschfeld, West, and others, is likely to lead to results of much clinical value. But Koch's conclusions go much farther. In so far as they relate to phthisis, they amount to this—the presence of the tubercle-bacillus is essential to the development of a phthisical process, and if this be so, it follows that phthisis is most probably a communicable disease. How far, I ask, is such teaching in accordance with the present position of our pathological knowledge on the one hand, and with that of clinical medicine on the other?

The teaching of pathology respecting phthisis justifies, I think, the following three propositions. 1. The morbid processes which lead to phthisical consolidation of the lung are inflammatory in their nature; by which it is simply meant to imply that they owe their origin to some kind of injury of the pulmonary tissues. 2. Phthisical consolidation of the lung differs from most other forms of pneumonic consolidation, inasmuch as it exercises an injurious influence upon the adjacent and distinct pulmonary tissues, and thus tends to spread. To this infective property, which varies very considerably in different cases and under different circumstances, the progressive character of phthisical consolidation is largely due. 3. The infectiveness of phthisical consolidation is, in all probability, due to changes in the inflammatory products, for the occurrence of which the presence of organisms is a necessary condition. Koch's investigations seem, therefore, to supply the element that is wanting to establish the truth of our pathology as far as it has gone—they discover the organisms, the existence of which had been foretold. But they do much more than this; they appear to prove that the organisms are specific. This, if true, is undoubtedly a most important additional link in the still incomplete pathological chain, and our previous knowledge of the pathology of phthisis is, as it seems to me, rather in favour of than against its acceptance.

If we appeal to clinical medicine, the greatest, and what seems to some the insurmountable, objection to the acceptance of the tubercle-bacillus as an essential element in the causation of phthisis is the fact that clinical experience seems to be altogether opposed to the view that this disease is communicable. In considering the question of communicability or contagiousness, however, it must be borne in mind that something more than the presence of the bacillus is obviously necessary to produce phthisis. As the organisms are to be found in the breath and sputum of phthisical patients, the source of infection must in some situations abound, and yet we know how exceedingly rare are instances in which it can be shown that the disease has been communicated. This second factor is something

inherent in, and peculiar to, the individual; and, whatever it may be, its existence is in all probability necessary, in order that any bacillus coming from without may exercise an injurious influence. The question of the contagiousness of a disease is sometimes a clinical problem difficult to solve; and although one may certainly state that phthisis is not readily communicable from one individual to another, and that, if it be so at all, the existence of something abnormal in the individual is necessary as well as the presence of the contagium; we are not, I think, justified in refusing to accept this outcome of Koch's teaching simply on the ground that, up to the present time, the evidence of communicability is so meagre. It is for us now to submit this question—by no means a new one, remember—to the most rigid clinical investigation. This is, as you know, being done by the Collective Investigation Committee, and the evidence already forthcoming is sufficient, I believe, to justify a doubt as to whether our previous notions on this subject have not been erroneous.

Another objection to this teaching has been made by the clinical pathologist, on the ground that it takes no account of that all-important element, hereditary predisposition, the influence of which is so marked, that its consideration must necessarily occupy a prominent place in any study of the pathology of phthisis. As to the exact nature of what is transmitted, we are at present ignorant, and it is not my purpose now to discuss it. We shall, however, probably be right in saying that it includes a condition of constitutional feebleness, which renders the individual abnormally incapable of resisting injurious influences. But, whatever it may be, it is something inherent in the individual; and may we not find in it that second factor to which we have alluded, the existence of which would seem to be necessary in order that any bacillus coming from without may cause phthisis?

To the influence of heredity may be added that of the state of the general health. Quite apart from any inherited tendency, there can be no doubt that an impaired state of health greatly favours the development and the progress of phthisis; and it is when these two are associated, as they so frequently are, that we appear to have the condition most favourable to the development of the disease. Such impaired health, therefore, we may regard as another element of that second factor—the something inherent in the individual. In what way these inherent elements furnish the condition necessary in order that the bacillus may become injurious, we have yet to learn. I believe, however, as I have ventured to indicate elsewhere, that an explanation may possibly be found in the existence of slight stagnation of the blood-stream in the higher portions of the lungs, which the inherent conditions tend to produce.¹

It would appear, therefore, accepting provisionally the truth of Koch's investigations, that two conditions, at all events, are necessary for the development of phthisis—the presence of the tubercle-bacillus, and something, inherited or acquired, inherent in the individual. About the latter there can be no reasonable doubt, and although the evidence respecting the former is still quite incomplete, and we must await further and perhaps prolonged investigation before admitting the necessity of an organism from without, our present knowledge, both pathological and clinical, appears to present nothing exclusive to the probability of its truth.

What, then, I would ask, in bringing these few remarks to a close, is our duty with regard to this all-important subject? A pathology of phthisis which includes any such factor as a contagium from without involves issues so grave that we very rightly hesitate in accepting it, and require that it should be submitted to the closest criticism before we allow it to influence us in our practice. The position in which we are placed is undoubtedly a difficult one. To accept unconditionally Koch's teaching as it applies to phthisis, in the absence of more complete evidence, is obviously quite unjustifiable; and, having regard to the important social questions which must arise, extreme caution is necessary in giving publicity to any opinion respecting it. I allude, of course, more particularly to the communicability of the disease.

But are we justified in completely rejecting it? Supported as it is by the authority of its author, confirmed by other competent experimenters; for the most part in accordance with, and not involving any mutilation of, existing pathology; and although in some points apparently, yet not conclusively, at variance with clinical medicine,—does it not demand from us something more than mere careful investigation? Are we not bound, even now, to be more or less guided by it in the prosecution of our practice? In the treatment of phthisis, whilst never losing sight of the conditions

inherent in the individual, should we not recognise the probability that there may be something introduced from without? In attempting to prevent the disease, is not the question forced upon us—How far is it justifiable to allow individuals who are not consumptive, but who inherit a phthisical tendency, and especially when such individuals are out of health, to intimately associate with those who are already suffering from this disease? Such questions as these are now coming prominently before us, and in endeavouring to answer them, we must look to Pathology as our guide. If I read her aright, she is, I think, pointing in the direction foreshadowed by Klebs and Koch, and is asking us, those engaged in the practice of our profession, not only to help her in the attainment of the truth, but also, in the meantime, to act up to our light and to use her, even in her incompleteness, for the good of our fellow-men.

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF PSYCHOLOGY,

At the Annual Meeting of the British Medical Association in Liverpool, August, 1883.

By THOMAS LAWES ROGERS, M.D., M.R.C.P.,

Medical Superintendent of the Lancashire County Asylum, Rainhill; President of the Section.

GENERAL HOSPITALS AND HOSPITALS FOR THE INSANE.

GENTLEMEN,—I propose to devote a short time, by way of introduction to the business of the Section, in drawing a parallel between general hospitals and those specially instituted for the treatment and care of the insane.

In looking back on the commencement of both classes of institutions, and in following the course of their development, and more especially of the legislative enactments relating to both, nothing impresses one more than the fact that, whilst general hospitals were primarily founded, and have been since continuously devoted to the cure of disease, those for the mentally affected have been always more directed towards the safe custody of the patients than to their cure, and more in the interest of the public generally than in that of the patients themselves.

The earliest records of the history of the country show that the care and custody of all the sick were entrusted to the religious houses. "For the sick, if medicine was required, there was none to be had except in the monastery," whilst for the lunatic, chains, fetters, and stripes, were the chief treatment, varied with charms, incantations, and certain herbs gathered at particular seasons; the influence of the moon over mental disease being undoubted, evidence of which remains to the present day in the etymology of the word "lunatic."

That hospitals were founded exclusively for the reception of lunatics, we have the authority of Stowe that, in 1370, a hospital was founded in the parish of Barking "by Robert Denton, chaplain, for the sustentation of poor priests and other men and women that were sick of the phrenzie, there to remain till they were perfectly whole and restored to good memorie." This is perhaps the earliest record of the foundation of a special hospital in England, and it will be observed that it was founded with a curative intention.

At the time of the suppression of the monasteries, during the Reformation, St. Bartholomew's and Bethlehem Hospitals were incorporated by Henry VIII. in 1546-47, both having been previously founded as monasteries some centuries earlier.

To enter at any length into the history of this hospital is beyond my present purpose; but to those who are interested in the subject, and who have not yet read it, Dr. Hack Tuke's recent work, the *History of the Insane in the British Isles*, will be found to contain a mine of interesting information, most carefully and industriously gleaned from every available source.

But taking the period of the suppression of the monasteries as a starting point, and making a comparison between St. Bartholomew's Hospital, as a type of a general, and Bethlehem, as that of a special hospital, how unequal the progress of the two institutions!

Whilst the former has always had for its medical staff the most eminent physicians and surgeons of the day, and its course has

¹ See lecture on the Tubercle-Bacillus and Phthisis, BRITISH MEDICAL JOURNAL, February 3rd, 1883.