AN ADDRESS

ON THE

COLLECTIVE INVESTIGATION OF DISEASE.*

BY SIR WILLIAM W. GULL, BART., M.D., F.R.C.P., F.R.S.,
Physician-Extraordinary to Her Majesty the Queen, etc.

MR. PRESIDENT AND GENTLEMEN,—When invited by you and the Council of the Metropolitan Branch of the British Medical Association, to address this meeting on the Collective Investigation of Disease, I gladly accepted the honour, since the whole sympathies of my life are in the direction of this movement.

After the admirable addresses made in different parts of the country on this subject, first at Chester by the late Dr. Hughes Bennett, Dr. Waters, and Dr. Ransome, then at Cambridge by Professor Humphry and Dr. Mahomed, and lately at Birmingham by Dr. Foster, Mr. Macnamara, Professor Haycraft, and others; though I cannot hope to present this matter in any new aspect, or to add anything to the arguments in favour of it, still I am glad of an opportunity, in conjunction with Sir James Paget and others on this occasion, to take part in promoting what promises to be of so much advantage both to the public and the profession.

This meeting may be considered as a supplement, though it is but accidentally so, to the Bradshawe Lecture, given a month ago at the Royal College of Surgeons by Sir James Paget, in which he endeavoured to draw the mind of the profession into new lines of pathological inquiry, and to consolidate the results by the formation and extension of museums of morbid anatomy.

Now, although morbid anatomy is at an immeasurable distance from a great part of medical pathology, as physiology is a distinct science from anatomy, still both are built on anatomy. Without morbid anatomy our work would be foundationless and in the air. Nevertheless, my object to-night is to direct your attention to, and to enlist your services in, the investigation of a region of facts, which in large part, at least, lies at a far distance from the gross mechanical terminations of disease as they come before us in the *post mortem* room. And although the hoped-for gains of this collective investigation movement may not be such as to admit of being labelled and placed on the shelves of a museum, they will serve to throw a new light on what is already placed there, and will at least help us to a better knowledge and practice of our profession.

You will admit that a migraine headache, an attack of asthma from the smell or sight of some particular object, the troubles of digestion from mental work and anxiety, the special liabilities to disease of certain families, and the cloud of small ailments which often make life intolerable, though having no morbid anatomy, deserve a more exact study than they have yet obtained, whether for the satisfaction of the intellect, or for the latent pathological meaning they may convey to us; and especially, further, that when we have learned to question Nature, we find she has much more to tell us, even on trifles, than we had expected.

The idea of a collective investigation of diseases, in which every member of the medical body shall have his effective part, though obviously not to be realised at once—not, indeed, until the sun of science, which is the true Apollo of Medicine, has risen far higher in our sky than now—is yet one that at once challenges our ready and best efforts for its realisation.

Whilst the morbid anatomist is engaged in our hospitals and medical schools in demonstrating the effects of disease on the several organs and tissues of the body, we desire that all the practising members of the profession over the country, in the colonies, and in other parts of the world, should assist in the inquiry as to the origin of diseases—their early symptoms; their mode of spreading in families; their combinations; the causes of their intensity; their modifications in individuals; in families; their occurrence according to time of year; locality; sanitary conditions; occupations; and many other circumstances, some as yet but dimly discerned, and others not yet suspected. The value of this movement will, I believe, be obvious to all after but little consideration, for it will be admitted that had we leisure, proper means at our disposal, and from previous training a fitness for exact observation, we should

find in general practice one of the most valuable fields of pathology, as here and here only we have before us the earliest signs of departure from health, and the only opportunities for tracing the course of a disease from its beginning to its end. Having passed many years in hospital and private practice, I have come to see that experience gained in the latter is necessary for the correction of that acquired in the former, especially as helping towards a truer pathology.

In hospitals we have more largely to do with organic lesions and with isolated cases of acute inflammations or developed fevers, and in all with an incomplete personal history and without any family record. We cannot thus learn with any exactness either the beginnings or endings of disease. Patients come under observation with their maladies far advanced, and they often pass from observation but imperfectly cured, thus leaving fallacious histories, both in pathology, and therapeutics; and if they die, morbid anatomy can often give but a confused and inextricable mass of facts, which it may be difficult or impossible to put into their true relations. One might as well hope to determine the physical geography of a country, by measuring and analysing the contents of its rivers as they fall into the sea, as to hope to reach a true pathology from studying alone the results of disease on the *post mortem* table. Let it, however, be remembered, that we still insist upon the fundamental necessity of morbid anatomy as the only basis of true advancement. I am glad, therefore, to be informed by Dr. Mahomed that it is in contemplation to associate paid pathological experts and morbid anatomists with this movement. By such means alone can we make our results permanent stepping-stones for those who follow us.

As a passing illustration of what is here insisted upon respecting the relation of pathology to morbid anatomy, and as indicating what we may expect from wider research, pardon me if for a moment I refer to renal diseases. One of our recent and best writers on the subject concludes by expressing his conviction that "there is but one Bright's disease";—this honoured name of Bright defining a state found after death, and fixed upon as the battle-ground of renal pathology—and I see that the latest statement of to-day, drawn from experiments on animals, is to the effect that destructive changes in the kidneys have but one form and lead to but one result in the tissues of these organs.

I appeal, however, with some confidence to our present limited experience in general practice, and with more to that wider experience which will be gained by Collective Investigation, whether there are not to be found quite different beginnings, quite different courses, and conditions requiring quite different therapeutics, for that which in the *post mortem* room is regarded but as one pathological state.

In disease, one stream of morbid action naturally falls into another; and, whilst morbid anatomy gives us the final synthesis of results, there is but one possible means of analysis, and that through noting beginnings, order, and progress. Diseases are apt to so far assimilate as to become much alike towards death; but our work lies in a far other direction.

Believing, as I do, that more than we dare now expect will grow out of Collective Investigation, we must not be over sanguine as to its immediate fruits. The essentials for success are not only the *numbers* but the *intellectual organisation* of the movement. If we aspire to have the arms of Briareus, we shall need the eyes of Argus; for Nature is very much a Sphinx, and will answer no question put to her if it be open to evasion. Yet in truth it is not so; for truth lies on the surface, if we had minds trained, and free from prejudice, to see it.

The plan of this movement at present is to draw up memoranda on the several subjects for inquiry, and to issue with these cards of questions to be answered. Already such important memoranda have been issued on Acute Rheumatism by Drs. Goodhart and Barlow; on Acute Pneumonia by Drs. Sturges and Coupland; on Inherited and Acquired Syphilis by Mr. Macnamara and Dr. Barlow; on Diphtheria by Mr. Shirley Murphy; and questions on the evidence of the Contagion of Phthisis by Dr. Burney Yeo. This plan leaves nothing to be desired.

Nothing can be more useful and instructive than such memoranda. When extended over the whole range of medicine, they will place before the practising members of the profession in every locality, more or less succinctly, the state of our knowledge on the different subjects proposed for inquiry; and they will, whilst they indicate what we want to know, inform us of our ignorance on the various subjects.

The main difficulty lies in properly formulating the questions to accompany the memoranda. They must of necessity be so simple, pointed, and incisive, as to admit of no vague answers. This, with-

* Read before the Metropolitan Counties Branch of the British Medical Association.

out saying it, calls for a great amount of knowledge and intellectual combination, and no slight mental perception; for rightly to ask questions of Nature is the highest science of the intellect.

This is, indeed, the vital centre of the whole movement. If the Committees issue for any inquiry a definite question, and, that being settled, follow it up by another, and so on in series, Nature must at last be driven into a corner, and be obliged to say "Yes" or "No."

If it be that truth is hidden in Nature as a stimulus to the intellect in the general pursuit of knowledge, to us this obscurity of things has a double meaning where duty and interest come in to urge forward the pursuit.

And here we ought to remind ourselves that, if there is an experience which teaches, there is a much larger experience which is fallacious. Hippocrates rightly begins his aphorisms with this reminder.

Numbers, without perfect organisation, can effect but little or nothing. We require not only the fulcrum of ascertained facts upon which to base our movement, but such intellectual combinations and direction of effort as is in a manner required in mechanics. If, says Bacon, men had attempted mechanical labours with their hands alone, and without the power and aid of instruments, as they have not hesitated to carry on the labours of their understanding with the unaided efforts of their mind, they would have been able to move and overcome but little, though they had exerted their utmost and united powers. Yet men, he adds, are hurried on by senseless energy and useless combinations in intellectual matters, as long as they expect great results either from the number and agreement, or the excellence and acuteness of their wits.

And we may add, that as great mechanical results cannot be obtained without engines framed by the wit and hands of many men, neither can a knowledge of such facts as we have to deal with be attained without an exact *mental* and *numerical* combination of the members of our profession.

It will perhaps, and naturally, be objected that it is almost impossible to organise for any useful purpose the labours of men already overburdened by the cares and fatigue of practice, and that there is neither time nor fitness for delicate inquiries on their part. Admitting that this objection is valid, it may be urged in reply that it need not be insuperable; that if this movement makes some demand upon the busy practitioner, he will (as remarked by Mr. Macnamara in his speech on this subject at Birmingham), in proportion to the help he affords in carrying on this work successfully, receive back quite as much as he gives. That, further, it cannot be denied that when we see the meaning of the apparent trifles which in practice would otherwise oppress and worry us, our burden is thereby much lightened, and that nothing could encourage us more than to feel that even one daily observation recorded was adding to our general store of knowledge, and making the path of practice more easy. There is no tonic to the mind greater than the sense of work done; and our journey is likely to be made shorter, as it certainly will be easier, if the way is illuminated.

We, indeed, owe it to those members of our profession, who are admittedly overwhelmed by the apparently senseless details of their work, to promote a movement like this, the object of which is to bring order into their chaos, and to help them to stamp a scientific value upon facts hitherto only burdensome. If we compare the unflagging interest of any pursuit where the aim is high and clear with the tediousness and wearisomeness felt when working in the dark, we shall readily admit that we are actually lightening the burdens of practice by thus adding to them, and by giving some portion of them a sense and meaning.

It is the spirit of a man which enables him to do his work lightly and cheerfully, and he will certainly be helped in this by a combination with fellow-workers on the same subject.

There is, however, a further consideration which should weigh with the practising members of our profession. This combination for the purpose of extending medical knowledge is an important duty in itself to all concerned, for however slowly, and at first imperfectly, such a combination may operate, the smallest progress is a great gain to society and to our profession, and if favoured as it plainly claims to be by all the members of it, and helped by the efforts of our best minds, cannot, but, in the course of time, lead on from what is now but dawn to daylight.

As to the objection on the score of fitness, if this were urged as an argument against this movement, it would stand self-condemned on every ground; for if the members of our profession are unfit to observe, they are largely unfit to practise.

Probably not so much as this is meant, when it is said that the working members of the profession cannot help us much, but rather

that until the special workers in the sciences of physiology and pathology have made more advance, it would be useless for untrained practitioners to attempt anything novel.

But we believe this to be one of the fallacies of the day, and one of the causes which retard the progress of practical medicine.

Without in any degree depreciating the more recondite study of physiology and pathology, whilst indeed hailing with thankfulness the light such studies give us, and honouring those who in that behalf work for us, we may fairly maintain that we have been too indifferent to the value of the facts which lie nearest to us in our daily work. The feeling that only experts in science can do anything, and that we must wait until they, from their centre, move us, or we shall not move successfully, is only partially true.

To say that this Association is beginning at the wrong end, is to misapprehend how knowledge is gained. The history of medicine is in our favour. We do not wait to discuss the nature of sensation; or how it is that the peritoneum is painful in inflammation, before we give opium for its relief. Currie proved the use of cold effusion in fever more than half a century before physiologists began to suspect that there was a nerve-centre in the cord for controlling animal heat; and as yet they have not taught us specially how to utilise their discovery. We have long known the curative effects of quinine in ague, though physiologists have not yet determined the operation of the *miasm-organisms* which are present, and which may give rise to this disease. Laënnec developed his method of physical examination of the chest on the commonest and nearest principles.

All practice obviously lies in a knowledge of proximate facts, and it is equally obvious that that practice will be most guarded and exact which is guided by a knowledge of all the facts, both near and remote; still we are not to esteem lightly that which is near, because we are ignorant of that which is remote. It is good knowledge to have learned that fire burns and water wets; though obviously it is much better knowledge to know all the facts of combustion and wetting; the combinations, the oxidations, the adhesions, the capillary attractions, the amalgamations, the endosmoses, the exosmoses, and the many other molecular changes which attend these processes.

It would appear to be one of the faults of the medical education of to-day, which this movement may in some degree correct, to lay undue weight upon ultimate facts, whilst we neglect those which are near; to indoctrinate the student with the belief, for instance, that if he can run off upon his fingers the supposed ultimate constitution of the gastric juice (about which we are still very ignorant), that he has learned something respecting the digestive process; or that, by reciting the atomic composition of an organic substance, as muscle, he has learned something about it, though, in fact, nothing but what appertains equally to mere dead substance.

It will not be the least valuable result of this movement, if it correct our prejudices in these matters; and hence it has already been well observed that one of its effects will be educational on the whole profession, from the youngest student to the oldest practitioner. It will quicken and keep alive a sense of docility, the want of which is the cause of that confirmed prejudice which we often call experience, and which blocks the way of progress.

If the eye sees no more than it brings with it the power to see, then any objection against fitness must be met by the several Committees of the Association, when they issue their questions, by accompanying them with such memoranda as instruct the working members in the way of observation in any particular direction.

We, of all men, ought to be the last to be discouraged by the small promise of beginnings. Had we been present at the dawn of organic life, we should hardly have predicted its wonderful results as seen in past and present times. This collective investigation has been started on the principle of giving significance to what has hitherto been neglected; it would therefore be directly contrary to the *animus* of the movement if we underrated the power and probable success of efforts which may at first produce but little fruit.

The first intention of the Association respecting this movement was to obtain a better notification and more complete statistics of disease; and this was carried out to some extent by Dr. Ransome in Lancashire and Cheshire, but apparently not much came of it. The present form of the movement is chiefly due to Dr. Mahomed and to the advocacy of Professor Humphry at Cambridge in 1880, and subsequently to the meeting lately held at Birmingham.

Fifty-four Committees have been already organised, one of the Branches having six Committees, and another four. These Committees include from eight hundred to one thousand of the chief practitioners in England, Scotland, and Ireland, already pledged to the work,

Dr. Mahomed acting as honorary secretary to the whole, and who may be called its moving spirit.

It is not my province on this occasion to indicate specially what should be the subjects for inquiry, or to discuss the memoranda which should be issued upon them, but I may, perhaps, be permitted to refer to the subject generally. For instance, on the contagiousness of phthisis a preliminary difficulty occurs as to what is meant by phthisis.

Does the term include all the cases of destructive pneumonic changes, beginning from the apex, or only those of a distinctly tubercular type? Cases beginning acutely with fever and hæmoptysis, and ending fatally in a few weeks, and others with pleurisy or bronchitis, and lasting perhaps twenty years, or only those springing up without much observation until the infiltration of the lung is far advanced? To properly distinguish the cases will be an affair of much difficulty, since they approach from various points and intermingle inextricably. If the evidence of the communicability of phthisis should appear to gain force by extended inquiry, it will then obviously be necessary to determine with more than common accuracy in which form of phthisis this communicability occurs. If only in the more chronic forms, then whether the phthisis of old age is to be classified in this respect with the phthisis of the young.

It is plain that if in this movement of collective research we see a land of promise before us, we must at the same time recognise that the way to it is by a narrow and dangerous path. For whilst truth must be accepted when it is proved to be such, nothing will be more pernicious than false conclusions and partial truth sanctioned by so large a body as this. The opinions of a single individual would have less weight, and would take longer to permeate a profession, than conclusions having the colour of authority, and where the error would be perpetuated by numbers interested in the fallacy.

Assuming, again, respecting this question of the communicability of phthisis, that the presence of bacteria in the expectoration is characteristic of the disease at a given stage, we should then have to determine whether they were present at all stages; and whether the phthisis bacterium received its specific form from the exudation in which it was found, or whether its presence determined the specific form of the exudation. The fallacy of putting effect for cause is here imminent; and families might be broken up and society much alarmed by conclusions having but little foundation.

As genealogical trees, showing the rise and extension of families, with their many collateral branches and intermarriages, are found in great houses, and are regarded with pride and veneration, so we could wish that, in a like manner, *life-histories* were found in every family, showing the health and diseases of its different members. We might thus in time come to find evidence of pathological connections and morbid liabilities not now suspected; and we might discover means of prevention by a better knowledge of the origin and extension of maladies through blood-relation. The proposal of constructing such family life-histories is an important part of this scheme of Collective Research; and more would certainly be gained socially by the genealogies of health and disease, their connections and causes, and by tracing the strength of the strong as well as the ailments of the weak in a family, than from such barren histories as I have referred to, which tell only of inherited fame, but do not indicate the way to perpetuate and augment the inheritance. Unfortunately, there is a feeling of safety in ignorance; and there prevails in us a sort of blind superstition, a survival of the darkest ages of man, which makes us think that there is a kind of religious trust in not seeking too minutely into the ways of life; as if the intellect of man were the servant of impiety rather than as it is—the handmaid of all that is good to us. Again, there is another hindrance to our obtaining all we want in this matter of family history: “all men think all men mortal but themselves,” and there is a half-conscious sense of shame in admitting any liability to human frailties. This makes it a great difficulty to obtain the truth we want, though we may well excuse it, and believe it to be an instinctive tribute to the righteousness of Nature's laws; and an unconfessed confession, that many of our diseases and weaknesses are due to our own fault, and ought to have been prevented, as we trust, they will be through this movement in the course of time.

This matter of life-histories is no new subject, though it is one to which this Association especially wishes to give a new impetus. All practitioners of large experience would be able to tell us something concerning the associated occurrence of diseases in families, which are not generally supposed to have any connection between them.

Dr. Cheyne, in his article on epilepsy, in the *Cyclopædia of Practical Medicine*, says we conceive that epilepsy is as certain a mani-

festation of the strumous diathesis, as tubercular consumption, psoas abscess, etc. Now it is, of course, not to be maintained that tubercular disease and epilepsy are one state; but it does not seem improbable that diseases from hereditary defects of organisation may evince themselves in most different ways, and that there may be a common underlying bond of pathology between them. No advance of our knowledge on these and many similar points can be made through morbid anatomy. It is only through family life-histories that the subject can be studied. Such histories, incomplete as they now are, are often of great use in practice. Take, for instance, the anæmia and langour so much complained of in the girls of a family. Though, of course, such ailments are often due to the want of physical and intellectual training, they are by no means altogether so; and certainly the most difficult and intractable of them are not to be so explained.

We must, in many instances, have the life-histories of the parents or more remote ancestors, before we can fully unravel the causes of irregular menstruation, hysteria, anorexia, uterine flexions, and the like. And the same line of inquiry applies to the headache, pallor, dyspepsia, and seminal hypochondriasis of the males of such families. Further, take another example in acute rheumatism, upon which already a memorandum and questions are before the Association. The hereditary transmission of the rheumatic diathesis, its occurrence in intrauterine life (as appears to be shown by some of the congenital cardiac malformations), and its association with other diseases prevailing in the same family, throw a light on its pathology not to be gained at the bedside, or in the *post mortem* room.

Life-histories, as Sir James Paget pointed out in his Bradshawe Lecture, would give us the genesis of new and rare forms of lesion, and I hope I may add they would also show how the organic laws, favoured through generations, prevail over and wear out disease from the stock.

Again, if I may be allowed to refer once more to the still vexed question of the pathology of the contracted kidney and its relation to cardio-vascular changes, my friend Dr. Sutton and myself believe that it is only by a careful record of the life-histories of such cases that the matter can be settled. We believe it will then appear that the same pathological tendency in the arterioles may give rise to lesions in various parts, and not in the kidneys only; to a retinal apoplexy in one, a cerebral apoplexy in another, to chronic changes in the cord or brain and cord in a third, to contraction of the kidneys in a fourth, and in all to muscular changes in the heart.

Family life-histories would show whether it be true or not that the ailments of childhood and age—eczema, bronchitis, diarrhoea, etc.—have a parallelism in their pathology. Certain it is, they have much in common as to their occurrence. In old age, these ailments are called gouty, and in infancy and childhood catarrhal. It would be a point gained, if it were cleared up; whether, in fact, childhood is as gouty as age; or whether the word gouty, as applied to the diseases of the latter period, has any actual pathological value. If an old man's urine deposits largely crystals of uric acid, whether he be distinctly gouty or not, we refer it to that state; though such deposits are as frequent in infancy and childhood; and uric acid is the chief component in the urine of many creatures lower than man. Varied and numerous family life-histories might place on a surer basis our knowledge in this and a thousand matters.

Take the inquiries of to-day respecting infectious diseases, and the increasing evidence that such states are due to agents which we may hope to fix and analyse. These, if ever completed, will still leave untouched a vital question in the solution of which this collective research will have to take an important if not an isolated part:—I mean how it is, that the same poison acts with such varying intensity in different families; in some, the organism breaking down at almost the first touching of a poison; others suffering but little, and others having almost or altogether immunity.

Does this depend upon certain family peculiarities, and if so, what? Does the immunity come from ancestors having passed through the ordeal, as occurs to the individual in vaccination and syphilisation? Family histories will show us how far these immunities and susceptibilities extend; and with what peculiarities of the nervous system they are associated, and how acquired. Allied to this inquiry, is also that of the action of endemic poisons upon the new-comers into a district; at which I can only glance.

In the early part of this address I asserted, that collective research would give a new colour and meaning to many ailments, and show their relations to later organic changes; and I will conclude what I have to say, by referring to a set of ailments which very largely claims the attention of the practitioner, and a fuller investi-

gation;—I mean a set of cases whose life is nothing but ailment. Their physiology, if I may so express it, is a state of pathology. Their nervous systems are so sensitive, and their digestions so feeble, that they know nothing of that satisfactory resistance to disturbing causes from without, which we call health; and who often suffer as much from futile attempts to cure them, as from their congenital defects.

These cases supply a constant *clientèle*, and I can hardly say how much we should add to the happiness of mankind by a better understanding of them. Of course, I exclude those dilettanti *malades imaginaires* who may be said to enjoy bad health; whilst the class of cases to which I refer of right claim our greatest sympathy.

But I feel that I must not longer tax your patience. From the nature of the case, it would be impossible to indicate, even in a cursory way, the many bearings which this collective research may take. One thing is certain: every question settled will but open another, and give renewed encouragement for fresh exertions. In all its aspects, this project, set on foot by the British Medical Association, cannot but advance the highest interests of our profession, and the more surely, in proportion as it extends our knowledge of the prevention and cure of disease.

This is the age of combinations; but I know of none which has a purer object than this: for what can surpass that whose purpose is the investigation of truth for the good of man?

AN ADDRESS ON THE COLLECTIVE INVESTIGATION OF DISEASE.*

BY SIR JAMES PAGET, BART., D.C.L., F.R.S.

MR. PRESIDENT AND GENTLEMEN,—When it was proposed to myself, as well as to Sir William Gull, that we should give addresses here on the subject of collective research, it seemed to me scarcely possible that two addresses on the same subject could be given on the same night; and when Sir William Gull was good enough to promise that he would speak first, I felt nearly certain that he would leave me nothing that could be said, except after the manner of useless repetition. I shall, therefore, speak very briefly; and I will add emphasis in proportion to the shortness of my speech, by admitting that I entirely agree with everything that Sir William Gull has said. I will ask you to allow me to flatter myself so far as to say that, if I had spoken first, I should have said the very same things. If I may condense what Sir William Gull has said at full length, it seems to me he has shown perfectly how this design of the British Medical Association may fulfil the two great purposes which we should have constantly in mind in our profession—the increase of knowledge and the improvement of the mind of the observer; for these things can rarely go apart.

I cannot but admire how well a large assembly such as this, representing, as it does, a yet larger Association, has declared itself to be in the state of mind most favourable to the acquirement of knowledge—the state of partial ignorance. The list of questions which is published upon each of the papers sent out by the Committee indicates that we are not ashamed to confess our doubts on some of the most important things that come before us; that we are prepared to start confessedly ignorant on many points upon which we are supposed to have complete and final knowledge. I think that, in common with most scientific men, we may boast that this is rather rare; that there are large groups of men, and those much esteemed, who rarely express doubt on anything, and thereby command the assent of those who listen to them. Without expressing the smallest preference for one side more than the other, I would say that this is best to be found amongst politicians, in whose speeches we almost entirely miss the words which are most familiar to ourselves—"perhaps," "possibly," "I rather think," "I would venture to suggest." I have looked with much curiosity, not for the sake of acquiring political knowledge, but for the sake of comparing the political and the scientific mind, to see if in some of the best and most renowned speeches I could find one expression of the kind. Not one is there. We must therefore be content to put up with what may be regarded as a sort of unpopularity if we confess ourselves to beginning with doubts, in order that we may be more sure in proceeding

towards knowledge; for there is no state of ignorance so hopeless, so profound, as that which cannot even doubt.

The questions also show well how very large the inquiry is, and how various the objects that must be had in view in every research that we undertake. Sir William Gull has rightly said that I had in view in the Bradshawe lecture the promotion of museums of morbid anatomy, and I am quite conscious that, whenever one undertakes to promote one thing, it is very difficult to avoid an appearance of depreciating others. I put, as it were in parenthesis into the lecture, some words which probably attracted no attention, and might have been omitted, implying that I should be the last to hold that any one method of inquiry on any scientific subject can be sufficient; and I said, what I venture to repeat, that the pathologist who thinks lightly of observation in practice, and the practitioner who thinks lightly of observation in pathology, will neither of them attain to more than that partial view of truth which is nearly as bad as error. And so I think, although still very highly estimating pathology, as illustrated by morbid anatomy, one may nevertheless speak with an unbounded desire for the success of an inquiry such as this, in which morbid anatomy may be almost wholly left out of sight. It is impossible to reckon what shall be ultimately the relative degrees of importance of the several methods of inquiry. I would rather hold that it is altogether childish to endeavour to say which method is best. One rule may be held for all; that is for each man best which he can do best, whether by force of circumstances, or by his own natural ability. And as one cannot but observe that the chief design of this collective inquiry is to bring into the fields of knowledge all that may be gathered in general practice by family and general practitioners, so I would not hesitate to name the knowledge which may be so gained as probably likely to lead to the very highest knowledge, and even the solution, of the most difficult problems in pathology that can be set before us. I would have dilated on this subject, but that Sir William Gull has treated it so fully in regard to the attainment of family histories. There is certainly no other means so good as that which may be possessed by those who have known families for generations, and who can, of their own knowledge, and not on the fallacious and often very false reports of relatives and friends, declare what has been prevalent in this and that household. And yet, if one should set before one's self the gravest and most important problem in all pathology, it would be that which concerns the inheritance of disease; and as Sir William Gull has rightly stated, the inheritance, not of disease alone, but of that which from generation to generation shall obliterate the disease which one ancestor may have acquired. Let me observe that this is a kind of knowledge which can be gathered in the most ordinary pursuits of life; it needs no minute inquiry. The mere recollection of the daily life that has been spent in this or that small village, among the two or three generations in one family, may recall it at once, and this is what we most thoroughly need, this personal and exact knowledge. And when I speak of knowledge that may be gained in the study of common things, there always comes to my mind the great example of Darwin, whose renown will last as long for the manner in which he pursued his knowledge, as even for the grand knowledge which he acquired. I have often felt in Darwin's greatest inquiries, and I would cite, as the chief among the kind, his last work on the influence of worms, that there are very few facts which might not have been observed by the common daily labourer in the field. He had the rare power of taking the common things that other men waste, and out of them making the grandest material of scientific work. So that it is vain to say that in any branch of practice, "I have no opportunity for scientific inquiry; I cannot investigate this; I can contribute nothing to that which I see the scientific members of the profession are doing." It requires merely the opportunity of a practice in the country, and the mind and resolution of Darwin, to bring great pathological conclusions out of the most ordinary facts of daily life in general practice. And if one wanted another motive for this, it would be the improvement of the observer's mind, and the charm which, as Sir William Gull has well said, he would find in the promotion of his own work, and in the mere pleasure of observing and finding his conclusions.

We are all very apt to think that, as we grow older, we acquire experience and grow wiser. I have lived long enough to feel and discern the exceeding fallacy of that as a general rule; and yet I venture to say that the starting with a distinctly observing and scientific mind is of the first importance in our lives; for the whole career of a man's life, his whole real success in practice—or, to speak more to the subject of the evening, the whole value of what he may contribute to knowledge may de-

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