AN ADDRESS ON THE INTERNATIONAL COLLECTIVE INVESTIGATION OF DISEASE.

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By SIR WILLIAM W. GULL, BART., M.D., F.R.S.,
Consulting Physician to Guy's Hospital, etc.

GENTLEMEN, FRIENDS, AND COLLEAGUES,—I esteem it a great honour to be deputed to address you on this occasion, and on a matter which promises to contribute to the advancement of medical knowledge by a wider and fuller method than has yet been sufficiently attempted.

The subject of my address is the institution of an International Collective Investigation of Disease. This object, I need not say, is intimately connected with the main purpose of these Congresses; if not, indeed, in the natural course of things, an integral part of them. At present, we come together as individual cultivators and practitioners of medical science, each contributing his quota to our knowledge on this or that subject, which may or may not have been selected for thesis and discussion beforehand. Nor are our labours, even under these conditions, without excellent results. But a feeling is growing, and already widespread, that by a more organised combination, a more intimate and better co-operation, not only the members of these Congresses, but all the working members of our profession throughout the civilised world, might sooner or later be organised into a body of fellow-workers, associated for collecting information on medical questions over the widest area.

There is perhaps no profession which enforces upon its individual members so strongly as does medicine, the necessity of continued observation and intellectual cultivation; and there is no means by which this cultivation can be so well promoted as by meetings like the present, and by the active organisations, in the form of committees or subcommittees, which, we hope and anticipate, may spring from them.

The human intellect in its single and separate operation may produce wonderful results; yet, isolated as a man may seem to be in the intellectual labour which occupies him, he makes but little progress apart from the aid and co-operation of other minds which have worked and are working in the same direction. And though, doubtless, it will always be the privilege of the highest intelligences to clear the boundaries of knowledge, and to throw the rays of their genius into the surrounding darkness, yet all must be agreed on the great and almost supreme value of the intellectual co-operation of less gifted minds in the simple observation of facts, and especially when the needed facts are scattered over a wide field.

However much apart, as I say, a man may seem in the work of his intellect, he is really much less so than he seems to be; for, as we cannot refer the strength of our bodies to any particular food that we have taken, so neither can we track the thoughts of our minds to the sources whence they may have been fed; and, if our social nature has been raised from savagery to civilisation by social combinations, it is even more necessary that our intellectual nature should be helped in its growth and nurture by such intercourse and association as are here presented, and which we now propose further to advance.

My great countryman, Bacon, has not generally been well received in the school of German philosophy; so that I must use some caution in citing him in favour of intellectual combinations in pursuit of the sciences. Still, on thinking over the bearings of collective investigation of disease, I could not but feel that, although three centuries have elapsed since he broached this question, and little has been done in it since, we might be about to realise his idea of a *Novum Organon*, the formation of a new intellectual machine for removing and overcoming the obstacles to our medical progress.

The purpose we have before us is no less than this; to enlarge and methodise intellectual co-operation, whereby not only the active, but the at present inactive, faculties of observation of the wide-spread members of our profession may be combined into one or more lines of energy. I am not unmindful how much this presupposes; how it assumes the combination of exact observation and record, with refined criticism and analysis; how it demands the highest scientific perception, with the humble collection of the meanest facts; how, in fine, it means the development of intellectual combination into many forms of organisation which should be not one but many instruments of re-

search. Such a forecast may, perhaps, lead us to exclaim, "Who is sufficient for these things?" Yet happily the answer is near; time, though short for the individual, is inexhaustible in the race; the intellect is in its infancy, its powers of growth unexhausted; and to these, in their evolution, there appears to be no limit. The work to be done is unchangeable; and there are eager and willing workers in all lands, who only need the encouragement and direction of the master-minds of medical science to set them to work.

Happily, the phenomena which demand our investigation, though complicated and transient, are, it need not be said, the result of unchangeable laws. The capriciousness of Nature, as we speak of it, is but the weakness of our own sense and understanding, and its so-called mystery and obscurity but the darkness in ourselves.

The physiologist and the pathologist have to admit that not even in the caprices of man is there capriciousness; that mental caprices have their organic basis; that the whims, the fancies, and the prejudices of the human mind, not less than the changing activities of the functions of his body, or the revolution of the earth on which he lives, are but the resultants of unvarying laws, unchangeable as the fixity of the stars on which he gazes with wonder and admiration.

We may therefore have this encouragement, that, when any of our work is done, however small and trifling it may seem, it is done and settled for all time, or at least so long as the laws of organisation remain what they are; that the clearing of a fact in respect of disease will remain an imperishable inheritance of knowledge to those who follow us, so long as there is disease in the world. We may support our labours therefore, with a feeling of surety that the problem before us is a settled problem, however difficult its solution. Nature will not delude us, however much we may delude ourselves.

A superficial survey of the Sections of this Congress shows that the student of medicine occupies a unique position in the pursuit of knowledge. For instance, if I turn the leaves of the programme, I find that the Section of Anatomy invites attention to cell-nucleus and cell-division, the lowest unit of organisation; and the Section of Psychiatria to the psychic-epileptic equivalent, which, as no one will deny, stretches to the other pole of knowledge; hence, surely no man more needs to lay hold of the unchanging idea of law than the student of medicine, as he ranges from one extreme of observation to another.

In the International Investigation of Disease, the prevalence of mental disorders in different nations will be a subject of inquiry; also the forms which these maladies take, according to nationality, occupation, stratum of society in which they occur, etc.; the use of stimulants—alcohol, opium, hemp, tobacco, etc. This work obviously lies not only in the physical and physiological conditions of life, but in their intimate correlatives, the facts of psychology. The thoughts of the student of medicine must, therefore, range from the atoms that build up the textures to the hopes that make the man. And even perhaps further than this; for in most of us the unsatisfied mind cannot answer itself with the measurables and ponderables of physical science, however much it may admit that all else is dependent upon them, but will strive to go behind appearances and feelings to the substratum of their existence, and cannot find rest for its foot until it traces their relation to an unchangeable first cause.

Du Bois-Raymond might well remark that the problem of organisation would be comparatively easy, if it were unattended with feeling and thought; that it would then, as it were, present us only with an astronomical problem, in which the atoms of things would, after the manner of the heavenly bodies, move on in their several orbits in lines which science might hope to deal with. For it is within the scope of science, even as we now understand it, to track an atom of oxygen, nitrogen, hydrogen, or carbon through its synthesis in a plant to its combination in the elements of brain or muscle, and still on to its resolution and return into its original form, with corresponding evolution of force. But when this shall have been done, we have learnt nothing of pain, nothing of emotion, nothing, indeed, of the science of many of the common facts with which medicine has to deal every day. How different would be the problem for the physician if disease were unattended by pain or emotion, if the patient were indeed patient as unorganised materials are; if there were no hopes or fears on the part of the object to be dealt with, and no embarrassment on the part of the practitioner from desire to please as well as to heal! Quacks would then be banished to limbo. The course of nature could be watched until the equilibrium of health was attained, and the temptations to polypharmacy would be no more. But then, on the other hand, we should have nothing to do with life in its higher form, which is our ever-abiding problem, and we should miss the stimulus of affection, which, like a constant trade-wind, carries us forward—at least the majority of us.

If for these preliminary remarks I need any apology, might I not

say that I have come from the country of Newton and Harvey, that I have travelled by the home of Spinoza, and that I am speaking in the presence of the countrymen of Descartes and Leibnitz?

Gentlemen, I do not appear here to-day on my own behalf, but on behalf of the Collective Investigation Committee of the British Medical Association. The British Medical Association is an Association of most of the medical practitioners in the kingdom of Great Britain and Ireland; and it has, I believe, affiliated associations in our colonies. It numbers from ten to twelve thousand members, all, or almost all of them, actively engaged in the practice of medicine. It is organised into about forty branches, with their presidents and committees. These branches have frequent meetings for the purposes of professional intercourse, and for reading and discussing papers on medical subjects. It has, moreover, a weekly journal of very extensive circulation, which records the transactions of the different branches, and supplies to its members lectures and communications on the most important current subjects in medical science. This Association has been founded over fifty years.

For some years past, and especially during the last four years, it has been felt that the aims of this great Association might be advanced to a higher point by establishing an organised plan of medical observation amongst its members; and already subcommittees, to the number of fifty, comprising as many as a thousand members, have been established to carry forward this proposition. I am deputed by the Central Committee of this Association to ask you to-day to extend this combination by establishing an International Committee or the Collective Investigation of Disease, and I trust this may commend itself to the Congress.

Such an International Committee would have objects entirely germane to the recognised purposes of the Congress itself. Probably, one might go even further, and say that such an International Committee should form an integral part of these International Medical Congresses, and that their operation would in time become one of the most fertile sources of international medical advancement. I am happy to think that but little argument will be required in this assembly to commend this proposition to your favour. Already in Berlin, the Verein für innere Medizin has adopted the principle, and has established a committee, after the fashion of the British committee, to carry forward the work. The President of the Society, Professor Frerichs, has given his entire adhesion to the movement, and Professor Leyden has accepted the presidency of the committee itself, which includes also very distinguished names; and I hope that one or more of our colleagues from Berlin will to-day add a few words in favour of the proposition I am called upon to make.

In putting arguments forward for the establishment of a National Collective Investigation in its more limited form, it was permissible to insist upon minor advantages to be expected from such association: I refer to its educational value, since it could not but happen that every co-operating member would learn much from the investigations proposed; and that, whilst he promoted science, he would no less promote his own intellectual status.

Again, I pointed out, in my address in London, that the family physician or practitioner has a sphere of observation specially favourable to the study of etiology and modes of extension of communicable diseases. He has also special advantages for pathological study, which are not so much afforded to the professor in an university or hospital. It is his privilege to see the earliest beginnings of disease, and to have the opportunity of tracing its evolution and decline; or, when so favourable a course does not happen, the steps of pathological progress are before him; whereas, at the end of life, when the whole organism crushes downwards into a chaos of pathological forms by the advance of disease, it is often impossible on the *post mortem* table to say where the failure began, and how it has advanced. The family physician's observations should thus supply a corrective to a too exclusive mechanical pathology.

Amongst the subjects suggesting themselves to us in England in near relation to this matter, was the formation of life-histories and family-histories in respect of disease; and these could only be obtained through the family physician. If such histories could be widely and accurately recorded, the natural associations of different forms of disease in individuals and families would be made evident, and might afford suggestions as to pathological relations not now suspected—relations between diseases which are separated in nosological treatises, but associated in Nature. This mode of inquiry, through family-history, would open a wide page for the record of the pathology of ailments, a page than which there is none more interesting to the practitioner of medicine, since seven-tenths of his work, and perhaps more than this, lie in efforts to correct physiological deficiencies, and to maintain some near approach to the equilibrium of health, which a feeble organism, unaided, cannot reach.

On this part of the field it is that we can study the relations between functional and organic diseases, especially in the range of nervous affections, where the degenerative tissue-changes in one individual of a family may be compared with the physiological disturbances in another. It is also in these family-histories that we might hope to have elucidated the difficulties of correlated pathologies: why, for instance, in a numerous family, whose members are living under the same conditions, one or two should become the subjects of pulmonary phthisis, one or two the victims of rheumatism, another of epilepsy, whilst the others maintain a healthy equilibrium. If such a history should be repeated in ten thousand families, or in a hundred thousand, we surely might hope, by careful collation of the facts, to come to the groundwork of these differences, and to determine the rule which separates the epileptic and the rheumatic from the inroads of phthisis.

So much for the more limited, though not less important, advantages of national collective investigation; but in an international collective investigation the ground widens very much, not only from the different intellectual characters of its working members, but also from the greater variety under which disease presents itself.

The first gain, no doubt, will be from the intercourse and reaction of different national modes of thought, with and upon each other. It need not be said that the ways in which any subject may be viewed do not depend upon the subject itself, but upon the varied capacity of the minds brought into relation to it. Minds evolved during ages under special local and national conditions, and educated in lines of their own, cannot fail to give new direction and shape to the questions proposed for solution.

Each national mind will feel a different mental necessity. This view might be much further enlarged, if this were the occasion for it. At the Congress in London, I endeavoured to show that each nationality produces its own scientific school, and not least in respect of the science of medicine. One nationality is more distinguished by its powers of analysis, another by its powers of synthesis; one is critical, another historical; one characteristically anatomical, another physiological. Even if this occurred to only a limited degree, there must follow an interchange and fertilisation of ideas. And let no one believe that this is a dream. We have reached no more than the threshold of intellectual evolution.

International associations like this will serve to nurse and nourish these powers in the future. But, however this may be, these movements and combinations are in accordance with the spirit of the age. It is on all hands felt and acknowledged that the individual worker, apart from his colleagues, cannot hope to do much in any department of science, and that it is only by combination of members and subdivision of labour that scientific observation can be carried on successfully. The establishment of an international collective investigation of disease, appeals to us from every side; personally, as through its influence our energies are stimulated, and our intellectual activity varied; socially, since observations made in one country cannot but correct or support those made in another; collectively, since science is cosmopolitan, and can only grow well when fed from all sources. It has been objected that the results of collective investigation must, from the nature of the case, be inaccurate, superficial, and so far useless; that they are more likely to confirm prejudice than to extend knowledge; and, as regards their scientific value, science is made up of quite other stuff. Now to this it may be answered that knowledge advances in many ways; not only by the investigations of the gifted workers of science, but by the casual observation of an isolated fact. Indeed, it has hitherto been objected, at least against the science of therapeutics, that more has been gained by accident than by methodical pursuit. The truth is, that one step of knowledge, however gained, leads to another. The accidental infection of a milkmaid's hand, casually brought to the notice of Jenner in a country surgery, has not only contributed more than any other discovery to the limitation or suppression of a loathsome disease, but it has opened up pathological speculation of a far-reaching character, and which to-day, in the hands of Pasteur and his colleagues, reveals the hope that we have the key to many if not all infective diseases.

All will admit that, in the daily routine of practice, facts are occurring that are worthy of record; and that medical science loses much by the want of such record. By the combination proposed, what are now casual and wasted observations, would be methodised and stored for arrangement, comparison, and deduction.

The English Committee has proceeded as follows. Having first determined certain subjects for Collective Investigation, cards of queries have been framed, and distributed through the different Sections, leaving each member to select such subjects as he might prefer.

As regards those life-histories of which I have spoken, patients and

practitioners are advised to apply to the Association for blank books for these records. The head of the family, or other person, is directed to request the medical attendant, after each illness, to write a brief account of it, with any note he may be pleased to add; so that, after the manner of genealogical trees—which show the distinctions of a family through the heroic deeds of its ancestors—there should be a genealogical history, in time to come, of all the diseases to which its members have been subject from generation to generation. Such books might also become a record of health as well as of disease; and so show not only how families, and from them nations, decay, but how they grow. No doubt, such records even of a thousand families would contain singular revelations, and place many of our pathological ideas in quite a new light. We might learn that, with tendencies to organic disease, there was less tendency to epidemic influences; that, if diseases were prone to change their form and multiply, they were equally, if not more, prone to lose their forms in a reversion to health; that the occurrence of one disease might confer an immunity from another.

These life-histories might, perhaps, seem to have a more national than international value; but, if they could be obtained, they would supply material for probably the widest inductions. But there is this difficulty about them. Fear, as the wise man said, is a bad counsellor; and, unfortunately, the construction of these life-histories is beset with fear. There is a lurking dread in every man, and in every family, of exposing their frailties. This presents an almost impenetrable barrier to gaining the facts upon which life-histories are built. There is a fear, also, in the minds of the inquirers themselves, since there is a natural disinclination to intrude into secrets which are so anxiously guarded. But, whilst we bear in mind the wise man's dogma, and remember that fear is a bad counsellor, we may resist its counsels as far as we can, where so much is at stake.

The clinical subjects which at present have been selected are acute pneumonia, chorea, acute rheumatism, diphtheria, and inherited syphilis. Cards of queries have been issued on each of these subjects, and each card is accompanied by a memorandum stating the object of the particular inquiry, and directing the attention of the observer to the queries proposed. These memoranda are of the first importance in this movement. It is on them that our hopes of success must be grounded. It is intended that they should indicate the defective state of our knowledge in the subjects brought forward for investigation. In doing this, their value to the profession, and to the progress of our knowledge, is almost incalculable. It is no small matter to have set before us in a simple manner what the present state of our knowledge is, the defects which have to be made good, and the inquiries likely to lead to such a result. Text-books on medicine will hereafter have to follow something of this leading. Hitherto, and for the most part, they have aimed too much at satisfying the reader, and, in order to make the treatises complete, have assumed a knowledge both in pathology and therapeutics too little justified by fact, and so have hindered, rather than promoted, our progress.

These memoranda are intended to be critical suggestions and suggestive criticisms on the state of knowledge respecting the subjects brought forward. They are the centre of the proposed system. It may fairly be expected that each issue of them will be more exact and incisive. The successful framing of them is the one important object to be attained. Through them, the influence of the more advanced intellects in the profession will extend through the whole of it, and will reach its most scattered and distant members. Nor can we doubt but that there will be a reaction from the periphery to the centre, maintaining a living cycle of active mental co-operation. Who can limit the effect of such action and reaction? or deny that there will thus arise new energy and new genius? for, happily, the cultivation of science need not remain the privilege of professors and teachers, but may become the common inheritance of all.

The volumes I hold in my hand are the first published records of the Collective Investigation in England. They contain reports and memoranda on the communicability of phthisis, on acute pneumonia, chorea, acute rheumatism, and diphtheria.

I would refer, by way of illustration of my remarks on memoranda, to the Memorandum on Acute Rheumatism. This recites our deficiencies on the etiology of this disease, and might also include its pathology; the exceptional ways in which rheumatism occasionally develops; its probable relation to the state of the nervous system; the new aspect of the pathology of joint-affections in relation to the spinal cord; the relation of the intensity of the rheumatic affection to anæmia; its clinical relation to tonsillitis; the intercurrent affections of the skin; and last, but, unhappily, not least, the treatment. I might add, however—not as a criticism, but as a suggestion—that an exhaustive memorandum on acute rheumatism, showing the real state of our knowledge respecting this disease in all its relations, and

the more than vagueness of our treatment of it, would have a value which I should find it difficult to express. Long-standing prejudices, which for the most part are entertained and fostered as if they were established and confirmed experience, would go down before the exposition of such a memorandum, and its results in International Collective Investigation, as dry leaves before an autumn wind.

When inquirers on a given subject agree at the outset what are the imperfections of their knowledge respecting it, they naturally combine the more heartily in its further investigation.

The aspect of an inquiry from a personal standpoint, and that from a collective investigation standpoint, present the most important contrast. In the one, the Ego, however subordinated, is not lost. It lurks at the centre of the operations, and the results or supposed results of personal inquiry are too apt to be regarded as property, to be defended against all inroads, whether this property be worth defending or not. The observations and experience of any one man, however gifted, cannot be considered more than suggestive, and cannot have any authoritative value until confirmed by the repeated observations of others.

In a collective investigation, whether national or international, the individual and the results of his inquiry are obviously less open to distortion from personal favour or prejudice. This freedom from undue influence gives a greatly preponderating advantage to such associations as we are here met to establish. Here the observer has no selfish interest in the result of the inquiry. His work will be equally good, whether immediately fruitful in positive results or not. The isolated observer, on the other hand, is apt to have interest in little else than positive results. In therapeutics, the truth of this is too abundantly evident, to the confusion of medical practice, and to the vaunting of remedies and methods which are mostly of no value, or may be even worse than useless. It has not escaped the remark of the master of the logic of facts, that "*Habet enim unusquisque præter aberrationes nature humane in genere, specum sive cavernam quandam individuam, quæ lumen nature frangit et corrumpit—ut plane spiritus humanus (pro ut disponitur in hominibus singulis) sit res varia et omnino perturbata et quasi fortuita. Unde bene Heraclitus homines scientias querere in minoribus mundis, et non in majore sive communi;*" not in the common world, but in the world of themselves.

One can hardly forecast the amount of good influence on therapeutics, if, instead of individual assertions respecting the value and success of this or of that drug or method, we had the teaching of calm and impersonal results deduced from an international area of inquiry, so large, that the individual observer would be lost in the result.

Whilst our English Association has, as I have described, put forward several subjects for collective inquiry with the memoranda and questions I have named, our German colleagues have determined upon a somewhat different method. They have selected but one subject for investigation, thinking it better to exhaust that before taking up a second or third; and they have naturally selected a subject which at the moment prominently occupies the attention of the profession in all lands, I mean pulmonary phthisis. Upon this, they have proposed four points for solution: (1) the heredity of the disease; (2) the communicability of the disease; (3) the cure of the disease; (4) the transition of pneumonia into phthisis.

These propositions are accompanied with many questions of detail which I will mention presently. The English Committee, at the beginning of last year, also proposed as a question for collective investigation the communicability of phthisis, and have reported upon it. The German inquiry is not yet reported upon. The important discovery of Koch that there is a specific organism associated with pulmonary tubercle, and his infection-experiments, have naturally excited the greatest interest respecting the communicability of the disease. It is well known that, at different times and in different countries, the contagiousness of phthisis has been confidently believed in; and, as Professor Ewald remarked at a meeting of the Verein für innere Medizin, observers in America, in England, and in Germany, have supported the belief in the contagiousness of phthisis by important records respecting it. Still, until Koch's discovery, the professional mind was very much asleep about it, or with only now and then a half waking dream.

The English Collective Investigation Committee have put forward the question of communicability in the simplest form—"yes" or "no"—as follows: "Have you observed any case or cases in which pulmonary phthisis appeared to be communicated from one to another? Please answer Yes or No." It will be observed that this is not a mere voting question as to a man's belief, but whether he has observed any case or cases of the apparent communicability of the disease. "Out of the 1,078 members of the Association who returned answers

to the questions issued on this subject, at least 261 believed they had seen cases of phthisis which have originated in communication from one person to another; about thirty-nine more have seen cases which have made them doubtful whether phthisis may not be so communicated; while 105 have offered facts and arguments which seem to them to negative such a view. One hundred and fifty-eight of the affirmative returns refer exclusively to cases observed between husband and wife. Communication between husband and wife is mentioned, together with other cases, in thirty-four of the remaining returns. So that 192 observers report cases of supposed communication of phthisis occurring between husband and wife."

There are also a number of special returns where the disease seems to have spread from patient to nurse, and from friend to friend. I am not here, of course, to discuss the pathology of phthisis, or of its communicability from person to person, but to argue for the advantages of collective research. And, perhaps, no subject could be selected better to demonstrate these advantages than the supposed contagiousness of phthisis. By extending the inquiry over a wide area, the recurrence of striking cases of apparent communicability are repeated over and over again, and must naturally impress the mind more than a case or two occurring in individual experience. The Committee, therefore, justly remark in their report, that they hope that this first step in collective investigation into one of the most important questions connected with the etiology of phthisis, will be of value in leading to more open-mindedness in the discussion of disputed questions of experience and observation.

We shall await with interest the report of the Berlin Committee, for its card and queries is much beyond "yes" or "no." It presents seven sections: (1) General questions as to the history of the family of the patient, the subject of phthisis; (2) antecedents of the family; (3) previous diseases of the patient, as, for instance, whether of phthisical habit or not, what diseases preceded the development of phthisis, as scrofula, measles, whooping-cough, chlorosis, etc.; (4) on the question of heredity follow queries on health of parents? either dead of phthisis? and when? have they suffered from scrofula? disease of the bones? lung diseases not phthisical? as pneumonia? pleurisy? gangrene? bronchitis? etc., were the grandparents or parents related in blood? age of father or mother at the birth of the patient? were they sober or not? etc.; (5) contagiousness or communicability—was the disease taken from husband by wife? or from wife by husband? when the malady began? was the communicability between brothers and sisters or relatives or servants? was the contagion from residence in prisons, establishments for the insane, barracks, or infirmaries, etc.? was the infection by residence, clothing, or beds? by nourishment as through the milk of scrofulous animals, or consumptive nurses, etc.? (6) the curability, including only such cases where at least two years had elapsed from the beginning of the disease—where did the arrest or cure seem to begin? did this come from the improvement of the general condition? are cough and expectoration gone? have the physical conditions altered, and in what way, etc.? (7) the transition of pneumonia into phthisis—the day of the beginning of the pneumonia? its seat, as at base, apex, or middle of lung, etc.; right or left lung? expectoration—sanguineous? rusty? grass-green, etc.? signs of re-convalescence? when the first signs of tubercle occurred? etc.

I have stated just now that I appear here on behalf of the British Medical Association for Collective Investigation; but I have to add that it is my duty to lay before you further what has been done by the Collective Investigation in Berlin. Within the short time that the Berlin Association has been in operation (spring, 1883), its activity and success have been remarkable. The Berlin Association had already, in February 1884, nine months after its establishment, fifty-five branches; and Herr Leyden reported to the February meeting of the Verein für innere Medicin a communication he had received from Professor Rauchfuss, in St. Petersburg, informing him that they would establish there a separate Association for Collective Research, after the plan of that of the Verein für innere Medicin, and in relation with it. In Paris also, in the Société des Hôpitaux, there had been negotiations respecting the collective investigation of the Verein für innere Medicin, and a similar one would be there arranged.

In the June sitting of the Committee, under the presidency of Herr Fraentzel and Herr Leyden, Herr S. Guttmann, the Secretary, reports that the German Association is daily making progress and exciting great interest; that on all sides, from Denmark, Sweden, Switzerland, Italy, Spain, and America, there were communications asking for information respecting this collective investigation, and with a request for the cards which had been issued.

With the Verein für innere Medicin, there are associated of the Berlin institutions the "Königstadt-Verein," the "West-Verein,"

the "Südwest-Verein;" and for the associated societies out of Berlin there are correspondents for the medical societies in Rostock, Schwerin, Güstrow, Münster, Minden, Arnberg, Ratisbon, Paderborn, Aurich, Thüringen, Holland, Meran, Hagenau, Salzbrunn, Frankfurt (on Main), Köslin, Freiburg, Prague, Elbing, Mainz, Hanover, Gratz, Bochum, Frankfurt (on Oder), Hesse, Wiesbaden, Danzig, Memel, Marienwerder, Friedeberg, Lübeck, Chemnitz, Nürnberg, Bremen, Pomerania, Breslau, Giessen, Dresden, Posen, Essen, Halle, Kiel, Basel, Göttingen, Liegnitz, Riga, Davos, and Marburg.

Further, there is an Association for Collective Research of the Institutions for the Insane, and already Herr Jastrowitz reports a combination of twelve of these institutions in different parts of the country. There is also a similar movement for inquiry into the health and the diseases of the inmates of prisons and infirmaries. In fact, our German friends have in many lines outrun us. Their exertions and their success in promoting this organisation make it superfluous for me to add arguments in favour of the proposition before this meeting.

I hope I may congratulate this sitting of the International Medical Congress in Copenhagen, upon the happy incident that we are to-day called upon to centralise these operations which have begun in England and Germany, into an International Committee of this Congress, whose function will be to promote them in all lands; and, by the continued co-operation of these Congresses, carry forward a movement the fruits of which, as I have already said, it will be impossible to overestimate; whether we limit our view to the results on our members, on the profession as a whole, on the public good, or on the brotherhood of nations.

There is but one caution, and that lies against our attempting too much at first. In our scientific ambition, it would not be difficult to overleap ourselves. It is by a little well done that we shall do much; whereas, if our deductions be hasty, incomplete, and unfounded, the authority which will naturally attach to these researches will be much more obstructive to the cause of science than the fallacious dogmas of separate individuals. If our hopes and prospects are encouraging, the steps we take cannot be too wary. The purpose of our association could only lead to failure, and, perhaps, even a gigantic failure, if the movement were not waited upon by strict caution and exact criticism. If the work proceed at the present rate, a few years will witness one of the greatest and most useful movements in modern times. The founding of an International Collective Investigation of Disease will promote the national movements of the same kind in all countries, and will give a stimulus to international emulation under the happiest form.

In the time at my disposal it is impossible to sketch, even in outline, the number of subjects which claim attention. The committees of the different sections, as the work progresses, will select their subjects from their own point of view; some, with a broader purpose, will take the more common maladies, and the more curious the rarer ones. One of our English colleagues would have concise descriptions of rare maladies prepared as an entomologist would have them prepared, of newly discovered insects; printing distinguishing features in italics. He instances rhinoscleroma, Hebra's prurigo, morphea, Alibert's keloid of scars, Addison's disease, Hodgkin's disease; cretinoid state in the adult (myxoedema); congenital absence of special bones (such as radius and tibia, with associated portions of carpus or tarsus); cases of spina bifida, illustrating either results of treatment or survival without it; cases of sacral tumour; cases of "the recurring iritis in young persons;" aneurysms in the orbit; the osteitis deformans of Paget; disease of joints in ataxy ("Charcot's joint-disease"); non-malignant growths in the tongue; hemiglossitis; Kaposi's disease.

The arguments for these more curious inquiries are not far to seek, since diseases which are rare in some countries may be frequent in others, and, by their frequency, afford the required ground for the study of their pathology. I might instance the glandular and elephantoid diseases of China, and the successful labours of Manson and Mayers, which have traced them to the presence of the *filaria sanguinis hominis*, producing lymphatic obstructions and filarial thromboses. The elucidation of such a piece of pathology, though the disease be limited to China, must have a good influence on the whole medical mind in breaking down preconceived opinions, and in showing that we cannot go too far afield for our knowledge. It supplies, moreover, a further argument for the international collective investigation of disease.

Gentlemen, whilst I thank you for the favour with which you have received this address, I cannot but express my deep sense of the imperfect way in which I have set forth the greatness and importance of my subject. I can now but commend the matter to your good efforts, assured that, through them, success will not be wanting.