ADDRESS IN MEDICINE,

BY

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CAUSAL RELATIONS IN DISEASE.

INTRODUCTORY.

PROMPTED by a deep sense of my own unworthiness, and in a spirit of unfeigned humility, I am compelled to avow that no less deserving and yet no more grateful recipient of the honour your Council has conferred upon me has ever delivered the Address in Medicine before the British Medical Association. The honour I esteem in proportion as I feel it to be unmerited, and its value is enhanced because of its inception in the minds of my kind hearted colleagues, for, according to the saying of Ovid:

Acceptissima semper
Munera sunt auctor quæ pretiosa facit.

I am afraid, however, that in making their present selection they have credited me with worth and fitness that I do not possess, and so unintentionally have placed me in the performance of my present task under a disability from which relief can only be found in the indulgence of a long-suffering audience.

It has been the custom of readers of the Address in Medicine to select a subject of general interest and wide application rather than one of narrow limitations, though perhaps equally attractive, and to this time-honoured fashion I have determined to adhere—a resolution to which I am practically compelled in the absence of the fruits of original research in the laboratory. And this I admit without apology, for the lines of my professional life have fallen in spheres so trite and commonplace as the wards of a general hospital and the *post-mortem* room, wherein the work is so engrossing in character as to absorb so much of the time of a busy practitioner as almost to preclude him from the privilege of such research.

On reviewing the past fifteen years, during which it has been my privilege to be Physician and Pathologist to the Royal Infirmary of this city, I naturally ask what phases of clinical work have chiefly interested me. This question I suppose every physician propounds to himself from time to time. But the answer was of special importance to me, because therein I thought I might reasonably expect to find a solution of the one pressing difficulty of my life during the past six months, namely, on what subject should I address this august assembly? I have no hesitation in saying definitely and decidedly that etiology and diagnosis have proved most profitable and interesting, and that in their study my *post-mortem* work has been particularly helpful. These are extensive branches of clinical medicine, and cannot here be comprehensively dealt with; but they furnish me with special lines of thought full of interest and of great practical importance, for there is perhaps nothing more useful and suggestive than an inquiry into the relation between causation and treatment, and the general bearing of etiology on diagnosis, not only as supplying a possible clue to the nature of the disease, but as explanatory of some of those modifications which are so common in clinical pictures. In other words, I propose to discuss some prominent morbid relations, and hope to suggest some of the lessons which their study supplies.

In offering the few remarks it is my privilege to make upon this subject, and in regarding this opportunity as "the season when to take occasion by the hand" by emphasising the bearing of etiology on diagnosis and treatment, I desire it to be recognised that my efforts are only to be considered as suggestive, and by no means as exhaustive or complete. "The tower but not the spire we build." And if the manner in which I shall discuss the subject be thought to derogate from the credit of the long list of pioneers whose names are honourably associated with this

work, I must express regret, and claim the indulgence usually accorded to him who attempts to crowd a quart measure into a pint pot. And to this favour I feel all the more entitled, when I recollect the advice tendered me by a greatly esteemed and high official of the Council of our Association, who, when congratulating me upon the high honour to which I was called, added significantly, "Don't make the address too long."

THE BACKWARDNESS OF ETIOLOGY.

It will be admitted that etiology and treatment have lagged far behind diagnosis in the scientific race in which we are all interested, notwithstanding the substantial impetus given by the labours of explorers in the microcosm of disease; still, even with this exception, causation has as yet hardly received the recognition and consideration to which it is entitled. One is tempted to ask why this should be. May it not arise from an insufficient consideration of the possible end to which this great principle of causation may lead? And are we not too prone to multiply causes unnecessarily, and, I venture to think, at times flippantly against the protest of science and philosophy: "*Entia non multiplicanda sunt præter necessitatem*?" Might we not with advantage take a leaf from the chapter of modern experimental pathologists, and aim at demonstrating that a disease can be referred with more justice to one definite cause than to several, each credited with its *pro ratâ* share of statistical responsibility? It will no doubt be objected that no matter how desirable such a demonstration may be, our present knowledge does not admit of it. But even if this be so, an attempt at least should be made to differentiate morbid conditions that may by their close symptomatic resemblance to each other be confounded, and to refer each to its proper cause. It should also be borne in mind that many so-called causes do not strictly merit the name, although they have long figured as such in the textbooks whence we draw our inspiration, and wherein we have been accustomed to the qualification of general statements by others vague and conjectural, but warranted nevertheless by authority, and made expedient by a conscientious and self-imposed restraint upon a natural tendency to indulge in generalisations. Authority has much to answer for. But it must not be forgotten that statements ear-marked and accepted under the ægis of authority are, in many instances, the simple and untried opinions of those whose opportunities were by no means equal to our own, and that the responsibility for the chaotic state of our code of laws rests with ourselves, for have we not failed to discriminate between fact and opinion? Let it not be supposed that in entering this protest against the blind acceptance of the views in their entirety of the intellectual giants who have preceded us, I am attempting to throw off the yoke of authority, or am desirous of doing an injustice to those to whom we owe so much. Indeed, we should be the ungrateful inheritors of the valuable legacy bequeathed to us if we did not in turn seek to hand it down to our successors amended and amplified according to the light given us by the indubitable advance of science in our day, for as Goethe says, *der Mensch soll immer streben zum Bessern*.

DEFECTS IN TERMINOLOGY.

The divergence of opinion that exists in reference to the true causal factor of some diseases may, in a measure, be due to the employment of terms which do not convey precisely the same meaning to all who use or read them. Hence, naturally, ensues a confusion which I venture to think will gradually disappear as we come to prescribe to ourselves as an ideal to be realised more and more fully with the progress of our knowledge the reference or reduction of morbid conditions to their true and definite cause or origin. The employment of this method will eventually lead up to the formation of a nomenclature which, in addition to being strictly accurate, may prove of immense therapeutical and diagnostic value, and guard us against the mistakes incident to the habit of giving to a group of symptoms or a pathological condition a name which may indicate a discoverer, but does not indicate the nature or origin of the disease. It may be said that there is nothing new in this, as, when practicable, disease is being named with the object of securing the recognition of its cause, witness malignant and rheumatic endocarditis. In this there is an element of truth, but only an

element, and it is far from my intention to claim any originality for the main proposition, for *nil dictum quod non dictum prius*. Indeed it is just the recognition of the value attaching to names so framed that begets dissatisfaction with the old terminology. To the clinical teacher and his class the advantages of this system are enormous, whilst its adoption in leading to important differentiations must be helpful to medicine at large.

An apt illustration is furnished by those numerous and seemingly distinct lesions of skin, eyes, brain, joints, lungs, etc., which aforesaid we regarded as separate entities, but which we now know have one causal factor in common, the bacillus of tubercle, linking all together. Thus the study of tubercle grows in importance as its circle of causal relations widens, and each new fact in its life-history or its manner of decay throws more light upon this great clinical family.

It may fairly be required of me to show what conceivable or probable advantage this method possesses, for on all hands it will be readily conceded that mere novelty or ingeniousness is not a sufficient justification for departure from long established canons. With all the reserve that should characterise the advocacy of what some may regard as an act of empiricism, I plead that the persistent attempt to reduce morbid conditions to a single cause will have a material and beneficial bearing upon the method of treatment. We are all agreed that a cure to be effectual must reach the root of the disease. Once let us be assured that we have discovered the true origin, and the proper treatment, if adopted early in a remediable case, may reasonably be expected to be successful. None of us needs to be told how ineffectual treatment often is simply because it does not reach the mainspring of the mischief. In a sacred book the prophet Elisha is represented as healing waters that were "naught" by going to their springs and "casting salt in there." The springs must be salted if the streams would be cleansed; for *a puro pura defluit aqua*.

Another advantage to be reaped from the recognition of unal causation in morbid relations is its diagnostic help, for if the patient's symptoms point in the direction of a possible disease which in the experience of the practitioner is commonly preceded by a definite causal factor, and we can assure ourselves of the presence of this factor, then our evidence is strengthened, and our diagnosis materially confirmed. To the objection that the number of cases of striking morbid affinities are too few to be of any practical value in this direction, it may be replied that the bond of union between certain associated lesions is being daily more recognised, and that the circle of causal relations is ever widening so as to include an increasing number of diseases. Is it not then a fair assumption that new connections now hidden or obscure may before long be made apparent?

ILLUSTRATIONS OF UNAL CAUSATION.

With this indication of the scope and purport of my subject I may proceed to test its practical bearing by a brief examination of a few of the more prominent morbid conditions whose claim to belong to the category of diseases having a constant cause is pressing. In this class I include the relation between syphilis and locomotor ataxy, which I believe from my experience to be constant. I regard, and have long taught accordingly, locomotor ataxy as invariably the result of antecedent syphilis, and that (some statistics notwithstanding) where you find the former you may declare with a feeling of absolute certainty that there has been at a previous date the latter, to which this locomotor ataxy is in some manner due. In an address dealing with general principles it is not convenient, or perhaps expedient, to discuss in detail dry statistical questions. I can only, therefore, very briefly point out the views of present-day neurologists on the subject. On referring to Dr. Gowers's excellent work,¹ we find the following opinion expressed: "It is probable, indeed, that taking all cases, a causal proportion of three-quarters will be nearer the truth." And again, further on, the statement: "It is certain, however, that syphilis is not the only cause of the disease. In a few cases (less than 10 per cent. of the whole) it can be excluded with confidence." It is just this opinion, based on the "less than 10 per cent. of the whole, excluded with confidence," that I desire to ques-

tion; and I venture to do so by asserting that my later experience, under very favourable opportunities of studying the question, has taught me that "specific" disease underlies all cases of tabes—a view which is rapidly gaining ground in the profession, and, in the somewhat cynical language of Horace, *Interdum vulgus rectum videt*.

In view of the importance of the investigation, it is fair to ask ourselves if we intend to wait until in 100 per cent. of cases locomotor ataxy can be unerringly traced to this cause before we believe in the inductive force of the 90 odd per cent.? I venture to say that such a course would be a virtual expulsion from medicine of the inductive method, which has hitherto been its best friend and its glory. This method, as you all know, does not profess to deal with every conceivable case, but only with such a number as may legitimately command assent and establish confidence, and is, with its results, valueless if we may not reason from such a large majority to the whole. For, as John Stuart Mill says, "Induction is the process by which we conclude that what is true of certain individuals of a class is true of the whole class, or that what is true at certain times will be true under similar circumstances at all times." It would be an interesting experiment to try to ascertain how much of the "less than 10 per cent." would remain if the patient who gives evidence could be guaranteed a good memory, and indemnified against the penalty which a true record might entail.

It is surely unscientific, as, indeed, it is irrational, to refuse to accept fully and unreservedly the teaching of the available statistics on this point. The evidence is overwhelming, and must convince all but the most cavilling critic. And it seems to me that the force of this induction can be weakened only where under a general term diseases are included which should be differentiated, as has already been largely done within my own memory. Dr. Gowers himself has done yeoman service to medicine by leading us towards a stricter system of nomenclature in the case with which we are dealing.

But why, you may say, insist upon such a rigorous application of the law of induction? Because to withhold that traditional consensus of medical opinion that necessarily guides the plurality is to leave a wide loophole, which, by the difficulties attending the discovery of the specific factor in the history of the case, is rendered still wider, until there creeps into the mind a suspicion that after all the proportion of "non-specific" cases is much larger than is generally supposed. Such a course begets a loose and disastrous state of things, the consequence of which is that the aid of a valuable diagnostic test factor is lost, and functional nervous conditions are often invested with a pathological dignity which they do not deserve, and violence is done to our nosology and injury to the patient.

Another disease which my experience compels me to ascribe to the same specific factor is general paralysis of the insane. Here the burden of proof is attended with still greater difficulties to the statistician, who has in many instances to deal with patients whom Nature in robbing of past memories has deprived of the ability, if they possessed the will, to furnish a true record. And further, amongst general paralytics we find a larger proportion of women than amongst tabetic cases—a fact which lends an additional complication to the case. Notwithstanding these obstacles, we find that one of the most recent declarations goes so far as to claim that at least 65 per cent. of the cases are specific,² and we know that years ago Steenberg and others contended for the unal theory, namely, that there was "no general paralysis without syphilis." Jacobson, in subscribing in large measure to this view, asks why it is that, in spite of the results that have led these writers to this opinion, but few modern alienists have been found to give support to the doctrine. And his answer is, "because no one has yet been able to definitely trace the syphilitic infection to every case of general paralysis. And the frequency with which different investigators have detected it varies it to an extreme degree." Here, again, I beg leave to apply the inductive method and appeal for support of the views of Steenberg, to which I can honestly say my own not inconsiderable number of cases have led me. The importance of adopting this view, if it be correct, can scarcely be overestimated, for it will surely clear

¹ *Diseases of the Nervous System*. Second edition. 1892.

² Jacobson, *Journal of Mental Science*, April, 1892.

the way for a reliable diagnosis if we can with confidence say that in eliminating syphilis in a mental case we are practically excluding general paralysis—a factor that may turn the balance where, as often happens, we are compelled to catch at diagnostic straws. If further evidence were required in support of the theory of the constant connection between syphilis and general paralysis of the insane, over and above that furnished by statistics and interpreted in the light of the inductive method of reasoning, it is, I venture to think, to be found in the fact that this disease is virtually the only progressive mental disease with which we are familiar, with a definite and recognisable anatomical lesion. And this fact alone justifies us in pronouncing in favour of a special causation, for it singles out the disease, and seems to warrant the conclusion that, as a definite pathological process has been at work, so has there also been a special etiological factor.

It may be rash in the light of past failures to predict much therapeutic benefit from the recognition of the cause in the instances with which I have been dealing, but may we not fairly ask ourselves the question, might not the results have been different had we applied the remedies earlier? For it will not be straining to an unreasonable extent our knowledge of “specific” lesions to suggest that preceding and underlying the morbid changes of the nature of sclerotic degeneration with which all pathologists are familiar, and which, as we know, have defied our remedies, there is a stage of specific myelitis and neuritis that tends to run an extremely chronic and insidious course, and may be most difficult to recognise clinically, unless perchance a timely warning be given by a ptosis or a diplopia, etc., when the administration of suitable remedies might be expected to yield a good result. How many cases of locomotor ataxy and general paralysis have been prevented by the timely use of antisyphilitic remedies, when at the outset a clue is forthcoming, it is impossible to say.

THE RELATION OF ANEURYSM TO SYPHILIS.

I have yet another sweeping accusation to lay to the charge of this many-headed hydra, the extent of whose subtle and baneful influence we seem scarcely yet to realise. For many years I have been deeply interested in the study of the connection between syphilis and aneurysm, and in common with others have been struck with the frequency with which the former occupied a prominent place in the history of a sacculated aneurysm. I have pursued the inquiry with special care, with the result that since I commenced the investigation no indisputable case has come under my notice in which specific disease was wanting, excepting two or three examples of that interesting condition, acute softening of the arterial wall, with aneurysmal bulging, in malignant endocarditis. In order to emphasise this statement you must allow me to remind you that you are at present in a part of the country where aneurysm is an exceedingly common disease; witness the fact that during the past ten years 145 cases of aneurysm of the thoracic aorta have been treated in the wards of the Royal Infirmary.

I am not endeavouring now to make out a case in favour of the specific character of the aneurysmal process, for I do not deny the possibility of sacculated arterial bulging to the extent worthy of the name of aneurysm, in conditions other than those due to syphilis of the arterial coats. But I would suggest that there is something special in this form of arteritis that renders the affected part of the vessel peculiarly liable to yield to pressure. It may be that the involvement of the middle and outer coats, and their transformation into fibrous tissue, so common in syphilis, will account for this liability; and I have found it a good practical rule not to diagnose aneurysm in a doubtful case in the absence of syphilis—a rule that must be more or less limited in its application, as it necessarily presupposes an accurate history. Arterial strain through hard work is of course a factor in the production of aneurysm, but it is one that in the vast majority of instances requires to be multiplied by specific arteritis in order that the product be aneurysm, and then it is the lesser quantity of the two.

THE RELATION OF EMPYEMA TO PNEUMONIA.

The causal relation of pneumonia to empyema, one of the closest if not the most constant, the latest textbooks notwithstanding, is another exemplification of the subject. True

there are cases of pus in the pleural cavity that have not arisen out of an attack of pleuro-pneumonia, due possibly to an acute septic pleurisy, or to tuberculous pneumothorax, or as the result of a simple effusion becoming contaminated by septic matter, say by means of a trochar, but these are exceptions. Exclude them, and it may fairly be concluded that empyema, the common disease with which practitioners are so familiar, may be ascribed to an antecedent pneumonia. I must apologise for here repeating opinions to which I have already given expression in more than one journal, but the matter is so important from a diagnostic and therapeutic point of view that I must run the risk of incurring the charge of being wearisome, but “*Bis ac ter quod pulerum.*” It is of the greatest moment to recognise an empyema as early as possible, for the sooner the pus is evacuated the shorter will be the surgical treatment of the case, and the better the prospect of permanent recovery; and further, if taken very early, a single tapping without incision and drainage may effect a cure. Now the diagnosis of these cases turns largely upon the discovery of the pneumonic factor; in other words, familiarity with the fact that effusion following pneumonia is very apt to be purulent furnishes a clue to diagnosis.

THE RELATION OF CHOREA AND RHEUMATISM.

Few subjects have excited more interest than the relation between rheumatism and chorea, and none has occasioned greater divergency of opinion. It must be admitted, however, that the discrepancy relates chiefly to the question of degree, for nearly all allow a certain affinity between the two diseases. It is most unfortunate that something like a general agreement cannot be effected, for the present very unsettled view of the question is eminently unsatisfactory. It may be said that the present state of our knowledge does not warrant a consensus of opinion. That may be true, but it is no reason why, out of our present knowledge, however limited, a theory should not be advanced in thorough harmony with well ascertained facts, which may at any rate be a guide to fuller revelations with increase of information. If it be admitted that a certain proportion of cases of chorea are associated with rheumatism, the inquiry naturally divides itself into two parts, the nature and extent of the relationship. My own cases greatly favour the idea of a singularly frequent and close connection between the two, so much so that I am inclined to the view that, in a certain proportion at least, chorea is simply one manifestation of the disturbance by the rheumatic poison of the function of somewhat unstable and growing cortical cells; and if this conception be tenable in a few instances, may it not be extended so as to include the whole of the familiar group of typical choreic cases so common among the children of nervous and rheumatic families, and prone to yield to large doses of arsenic? Again I ask, are we, before admitting the inductive force of the majority, to wait until in every case we can demonstrate a clearly defined rheumatic illness, as at present understood, in the form of gross articular lesions? Is this scientific? May we not have, without joint mischief, a rheumatic diathesis to which belong the choreas, the pleurisies, the headaches, the cutaneous eruptions, etc., so frequently associated with rheumatism?

THE CAUSATION OF CARDIAC DISEASE.

The study of cardiac lesions throws into relief the importance of tracing diseases to their true causes. They are all referable to primary conditions—rheumatism, gout, specific fevers, tuberculosis, sepsis, alcoholic excess, syphilis, etc., which are singularly fruitful in morbid products. These conditions constitute a formidable list, to which we are constantly adding, and each is responsible for heart affections having more or less characteristic clinical and pathological features. The immense importance of investigating cardiac diseases upon the basis of their antecedent and underlying morbid states cannot be overestimated, for the treatment and prognosis depend largely upon the particular agent at work. The same remark, I need scarcely say, holds good in the case of lesions of other organs, a fact that in many instances has been fully recognised. But its application in heart disease is perhaps more than ordinarily conspicuous. It has been the habit of our schools to attach too much value to the diagnosis of the particular valve lesion, and too little to its cause.

Thus, for example, we content ourselves with the shallow and often unimportant observation that this or that case is one of mitral regurgitation or double aortic disease, a diagnostic shibboleth that any novice may apply; forgetful that what is of paramount importance is not only the primary reason for the existence of the lesion, but also the cause upon which the symptoms belonging to any subsequent attack of cardiac failure may depend. Thus the inquiry would necessarily embrace the questions in the case of toxæmic lesions—gouty, rheumatic, alcoholic, etc., is the poison still circulating? and what is the prospect of its return? Difficult and at present more or less problematical are these questions, but to them we must direct our attention before we can hope to prognose and treat heart disease successfully.

An attack of rheumatism may damage a valve and leave behind a permanent legacy, but how innocuous that structural change may be, so far as the health of the patient is concerned, so long as his real enemy—rheumatic poison—remains absent, is well known to all practitioners. For there can be no doubt that the injured endocardium is very apt to suffer further damage under the influence of fresh rheumatic attacks, however slight. And it is these intercurrent rheumatic accessions, often subtle and unattended by any outward manifestation beyond fever, that are fraught with so much risk. So also is the case of gouty mischief, but here the causal factor is more constant and insidious in its operations, and its ravages are too often deeply rooted before they are recognised, a fact that should compel us to greater diligence in unearthing a latency so dangerous.

In syphilis we have a much more common cause of heart disease than textbooks admit, and surely a morbid agent that might be met and treated if recognised at the outset; but how seldom has the attempt been made, though specific arterio-sclerosis is, I venture to think, an obvious pathological entity.

The varieties of alcoholic heart disease are now receiving attention, and, with increasing clinical and pathological information, it is to be hoped that fewer mistakes in prognosis will be made; for often the alcoholic heart fails suddenly, giving rise to an unexpected catastrophe, despite the most favourable prognosis, because the mischief, being more of the myocardium than the valves, has remained undetected.

Careful attention to these etiological circumstances will often enable us to help the poor cardiac backslider in a more substantial way than if we simply consider his defective machinery and treat his more pressing wants, regarding the intercurrent attacks as natural and unavoidable incidents in the course of the disorder. It has several times fallen to my lot to demonstrate in the *post-mortem* room that the valve lesion from which the patient had suffered was due to a chronic tuberculosis of the pericardium resulting in general adhesions, and that the valves supposed to be diseased were in themselves healthy, though the orifices were dilated and the myocardium degenerated. Obviously the treatment and prognosis of such a case must differ from the treatment and prognosis of cardiac failure the result of old rheumatic pericardial adhesions, or of a heart damaged in early life by scarlatinal valvulitis and broken down by excessive work, or of a rheumatic case marked by returning and often protracted pyrexial attacks, or of an insufficiency directly due to alcohol.

Would a substantial reduction in the number of chronic heart cases, as the result of the recognition of the fact that many are due to curable and preventable causes which an early and successful treatment might reasonably be expected to subdue, be a dream too visionary for us to indulge in? I think not: but assuredly until we fully appreciate the causes a dream it will remain.

A USEFUL GENERALISATION. A SURGICAL ILLUSTRATION.

But perhaps it would be difficult to select a better illustration of the benefits that have arisen from allowing ourselves to be influenced by the view that the true or ultimate causes underlying diseased states are, in the botanical sense, solitary, and that the paths along which they travel are comparatively few and well beaten, than the knowledge that guides advanced surgeons in their dealing with suppurative peritonitis—the knowledge, namely, that these cases always

result from infection of the peritoneum with pus microbes. We pathologists have helped our surgical friends to give a practical turn to this fact by recognising that the infective microbes gain an entrance either by perforation of a viscus—ulcerated appendix, bowel, or stomach—or the rupture of a puerperal abscess, a generalisation that serves a useful turn in practice, though scientifically speaking it may not be quite accurate.

An extension of this line of thought cannot fail to convince us that our nosological tables hardly contain the name of a systemic disease that is not simply the reflection of some primary morbid state with which we are more or less familiar. Many of these, it is true, are known to us only as subtle influences, but they are becoming rapidly less shadowy, and before long it is to be hoped that the bright light begotten of the fierce competition and zealous spirit of work so characteristic of the present day, will reveal their true nature. At present, like light itself, they are not understood, and are scarcely discernible, until by falling upon some organ with whose form and functions we are familiar, they are reflected to the eye of our understanding.

The cases I have cited are few, but sufficiently typical in character to illustrate the importance and benefit of tracing a disease as far as practicable to one definite cause, and, did time permit, I might strengthen my argument by instancing the connection between gout and uric acid, acromegaly and pituitary growths, myxœdema and atrophy of the thyroid, Addison's disease and lesion of the suprarenal capsules, etc.

THE MODIFYING INFLUENCE OF ONE DISEASE UPON ANOTHER.

Passing on from the part of my subject that purports to give prominence to the causal relations of morbid entities, I come to consider briefly some possibilities in the modifying or controlling influence exercised by one disease upon another, and even in this enlarged sphere of inquiry we walk by the light of this principle of causation.

I may commence with this proposition that familiar clinical pictures of disease of an organ are often the product of the symptoms peculiar to it, and of others which are the expression of impaired function of another organ. In other words, the full symptom-complex of disease of one organ may be largely contributed to by disease of another, whether produced by the same or some other cause. I can best illustrate my meaning by an example. A patient addicted to alcoholic excess for a considerable number of years is known to be suffering from a group of dyspeptic symptoms, ascribed by the patient, friends, and doctor to this cause, but without a suspicion of gross organic mischief underlying them. This state of health interfered but slightly with the discharge of public and private duties, and indeed, beyond loss of flesh and some discomfort, there was a complete absence of signs of disease. An attack of pneumonia, which tried his powers of endurance considerably and was followed by a slow convalescence, completely altered the character of the case. Within a fortnight or three weeks of the incidence of the pulmonary disorder the abdomen swelled and its superficial veins enlarged, the lips became red and the tongue glazed and red, the face pinched and the urine scanty and full of urates, the motions black, and in every way a most suggestive picture of cirrhosis of the liver was presented, a transformation that astonished the doctor and friends, and was not at first sight easily explained. The only reasonable assumption to account for the apparently rapid development of cirrhosis of the liver was that the disease had existed for a considerable period in a latent form, and that the obstructed portal circulation was compensated for by the usual auxiliary relief, which served to reduce congestion, and thus keep in check the tendency to ascites, etc., until the attack of pneumonia, by producing cardiac insufficiency, upset the balance, and led to the appearance of the typical signs hitherto absent. Let us observe that though the pathological changes characteristic of cirrhosis were present, the clinical aspect of the case was incomplete until a new and necessary factor, which had nothing to do with the original disorder, revealed the true nature of the illness. It seems a just inference that the cardiac affection made an important, if not essential, contribution to the completion of the symptom-picture. Indeed, I am disposed to think that in cirrhosis of the liver generally the

occurrence of the abdominal dropsy largely depends upon the supervention of heart weakness. In further support of this view I may mention a very striking case recently admitted to hospital under the care of my colleague, Dr. Limont. The patient, a waiter, a confirmed toper, though active and in fair health, and capable of performing his daily work in a large hotel, developed very suddenly the symptoms of portal obstruction—ascites, etc., as the result of a few days of exceptionally heavy spirit drinking. His abdominal distension, becoming very great, was relieved by tapping, but an attack of peritonitis rapidly brought the case to a fatal termination. A *post-mortem* examination revealed a small hob-nailed liver, extensive splenic adhesions, an ample auxiliary portal relief, and a heart greatly dilated. Here, again, we would have had an incomplete clinical case, but for a symptom which the liver borrowed from the heart. In this connection I may remind my audience that it is not unusual for the dropsy of chronic Bright's disease to be determined by heart weakness. Another illustration of the truth of Agrippa's fable, as told by Livy, in which he insists on the intimate connection between the various parts of the human body.

But it is not enough to say that clinical pictures may be the joint products of symptomatic morbid relations, for they are often presented to us under the modifying influence of a tendency or bias, lent by a former or a concurrent disease; that is to say, it is quite common to meet with pronounced modifications in the clinical pictures of disease which can only be accounted for on the principle that one disease has a determining influence upon the aspect of another. The following illustration will explain my meaning. A young man, aged 22, suffered in the autumn of 1891 from an attack of alcoholic paralysis which revealed itself in the usual form of weakness in the legs, flapping of feet in walking, with, eventually, ankle-drop and paralysis, and paresis of the extensors of the fingers, along with which were severe myalgic pains. He recovered satisfactorily, and at the end of four months he was able to walk, and a little later to ride his bicycle. In May of the following year he had an attack of diphtheria, and in a few weeks, while strictly temperate, he developed, without any defect of sight or impairment of swallowing, numbness and weakness of the legs and hands, and indeed a very similar condition to that which had existed previously, except that the pains were absent. In this case the diphtheritic palsy was exhibited in a somewhat unusual form, in consequence of a predisposition to a special paralytic distribution lent by an attack of alcoholic neuritis. I am aware that cases of diphtheritic paralysis are met with where the lower extremities bear the brunt of the poison, but it is exceptional to meet with so pronounced a paraplegia in the entire absence of the usual signs, such as failure of accommodation, dysphagia, etc.

Or take this case. A man in the prime of life, who was much addicted to alcohol, came under observation for complete double drop-wrist, presenting every feature of a plumbic case. His history revealed the fact that twelve years previously he had suffered from lead palsy, with blue line, the result of drinking contaminated water, and as he was recovering he acquired intemperate habits, and developed into a chronic toper. The consequence was that the paralysis of the extensors became more pronounced, and remained permanent. There was absolutely no paralysis of the lower extremities, and the knee-jerk was normal.

The modifying influence of one lesion upon another may be, as is well known, decidedly beneficial. Thus, sarcoma and lupus may yield to the poison of erysipelas; of which fact, it is reasonable to suppose, more will be made in the immediate future.

The controlling effect of disease on disease is also to be seen in the fact that it is quite rare for pulmonary tuberculosis to spring up in the course of a cardiac affection. And, again, it is not unusual when lardaceous disease develops in consequence of chronic phthisis, to observe a distinct tendency on the part of the tubercle to be arrested in its growth. And, further, in this connection I may mention that I have been struck with the rareness with which a pneumonia complicated with jaundice proves fatal.

I make a brief allusion to these points in order to realise the main aim of this address—namely, the arousing and

quickenings of an interest in the study of morbid relations, wherein our success will much depend on our having a ruling principle for our guide, and a definite object and end in view, both of which I have endeavoured to indicate. An inquiry into the symptoms, diagnosis, prognosis, and treatment of disease cannot fail to be greatly facilitated by keeping ever before us the particular cause or causes at work in producing an uncomplicated disease, or in furnishing the kaleidoscope aspects of many of our cases. In one way it seems a work of supererogation to inculcate the close observance of causes; for, as a rule, the practitioner gradually comes to perceive its importance, and shapes for himself an unwritten compendium along this line. His "experience is by industry achieved, and perfected by the swift course of time." Automatically he seeks for the cause, and is—or should be—satisfied only when he has found it; and that he may register his observations he is driven to an inaccurate system of nomenclature, which shall possess for him both an etiological and a diagnostic value. Every advance in this direction is a positive gain to medicine; and whether we succeed in tracing a disease to its true origin, or attempt the more arduous task of disentangling the morbid web with a view of assigning its due proportion of responsibility to each factor, and so of dealing with the whole, we are engaged in the employment of a method which, if developed, may make a revolution both in the name and treatment of diseases. This is no mere fanciful theory, for it will be found that every scientific medical man unconsciously and even automatically, as I have said, finds himself perpetually engaged in practising it, and the measure of success he enjoys as a practitioner of the healing art is determined largely by the degree to which he permits himself to be led by its light. A man may never be fortunate enough to make an abiding contribution to the science of medicine strictly so-called, and yet may all through his life have given the public the practical benefit of his originality and independent thought in the adoption and practice of self-taught rules without the aid, or even in spite of the trammels, of hampering traditions. And there are many such amongst us who whisper their worth in the ear of the public, but seldom seek to proclaim it from the literary housetop. To him belongs the credit of benefiting mankind by the practice of unformulated methods, and he shall have his reward: *Denn Gott lohnt Gutes hier gethan, auch hier noch.*

I dismiss from my view the mercenary rewards (if any) that fall to our lot, for the passing of milestones is not the completion of the journey, nor even the applause of the public the height of our ambition or our goal. Our highest honour is the gratitude of humanity, and our best reward the growing assurance that we are lessening or alleviating the pain of many and lightening the load of life. We may not all lead medical opinion from an editor's chair or an author's desk, but every one of us in the employment of a reasonable, natural, and simple method, can bear our share of duty which is none the less honourable or beneficial that it is written, not on paper, but on human flesh and blood; as Cicero has said: *Homines ad deos nulla re proprius accedunt quam salutem hominibus dando.*

ON October 26th the Paris Surgical Society will celebrate the fiftieth anniversary of its foundation.

M. HALLOPEAU and M. Straus have been elected members of the French Academy of Medicine.

M. PASTEUR has been elected an Honorary Member of the Vienna Academy of Sciences.

THE *London Gazette* of July 28th announces that the Queen has been pleased to grant to Mr. Percy Athelstan Nightingale, M.B., C.M. Edin., her authority that he may accept and wear the insignia of the 3rd class of the Order of the Crown of Johore, conferred upon him by the Sultan of Johore, in recognition of his services while employed in the service of His Highness.

So many of the charitable institutions of Australia have suffered financially owing to the suspension of payments of the banks, that Sir Alfred Stephen proposes that a week of self-denial and abstinence from luxuries shall be observed, and the money thus saved be devoted to the maintenance of the charities. The Mayoress of Sydney has consented to act as treasurer to the fund.