but very unsatisfactory drawings of uterine lymphatics by Moreau; but we do not yet really know their actual anatomy, though very much progress has been made by the anatomical investigations of Lindgren, Fridolin, and especially of Leopold. The completion of this work will be an addition to the theory of puerperal pyæmia.

Following the as yet indefinite notions of a wound-fever, came a further true advance from Boyer, Legallois, Cruveilhier, Tonellé, Dugés, and Simpson, who combined to demonstrate the identity of what would now be called the rough morbid anatomy of patients dying after surgical and after obstetrical wounds.

Then came a grand piece of progress, consisting in the discovery and descriptions of phlebitis and lymphangitis, which we owe to Dance, Duplay, Cruveilhier, Robert Lee, Hecker, and Buhl.

We now reach our own times, and have a still grander progress of our knowledge to record, in the discovery of thrombosis and embolism by Virchow, Kirkes, Cohnheim, Buhl, and many others.

After these, come researches, whose bearings on this subject are certainly very important, but which are, in many respects, as yet immature and incomplete. We allude to the investigations or to the potency of septic poisons, connected with the names of Davain, Panum, Kehrer, Thin, and many others; the great and actively progressing researches as to the production, diffusion, and influence of bacteria of various kinds, by Lister, Klebs, Waldeyer, Sanderson, Billroth, Winge, Heiberg, Orth, and many others; the new researches of Sanderson on the infective product of all acute suppurative inflammations; researches also into the distinctive characters of the noxious or septic, and the innocuous bacteria.

Besides all these, many valuable results have been elicited from the analysis of experience in hospital and in private practice. In this way the influence of age has been ascertained, and especially of immaturity and of advanced life; so also the baneful influence of primiparity and of excessive childbearing, and of twin-bearing; so also the baneful influence of severe and of complicated labour; so also the bearings of the duration of labour; so also the influence of earliness or lateness of attack after delivery; so also the baneful influence of inclemency of season; so also the dangers attending hospitals and the dangers of communications between the sick and the healthy.

But all of these researches, whether finished or still imperfect, do not, when combined, complete our modern view of this great subject. It is a subject in practical medicine, and we are practitioners. The great object of our work is to prevent or to remove the disease in our patients, and we have to inquire what fruits our knowledge produces for the comfort or healing of the sick.

Many remedies for puerperal pyæmia have been proposed, and their successful application loudly proclaimed and widely believed. Doucet was even rewarded by the French Government for his discovery of the curability of this disease by ipecacuanha. In our own day new cures do not fail to make their appearance, and the advanced knowledge of our times would lead us to expect that they should be more rational, as the phrase is. But who is there of weight in the profession, now, who believes in any cure or in any system of specific treatment? All have been found wanting. Yet the wise physician of this formidable disease does not despair of guiding his patient through it, although he well knows its very dangerous character. Experience has shown him the utility of several means for relieving sufferings; and the favourable progress of a case may be encouraged, though not secured, by those invaluable directions which he may give as to diet and stimulants, as well as to more direct medicinal treatment of the genital passage, of the skin, of the bowels, and of the system generally.

But in this disease the physician has long been saying, not that prevention is better than cure, but that prophylaxis or prevention is to be chiefly looked after, and not cure. Great credit is due to Semmelweis for the good he has done, especially to hospital patients, by his enlightened zeal in this cause; but the records of hospital practice sufficiently show that much more has yet to be accomplished. Prophylaxis is still farther to be carried out, by attention to stop injurious communication between the sick and healthy, by disinfection, and by architectural arrangement, subjects which are all at present receiving much attention from the profession.

In the course of my remarks I have repeatedly referred to scientific researches, as to the poison producing pyæmia, and as to the effects of its concentration, and as to the connection of this poison with the presence and diffusion of bacteria. These researches have been carried on mostly by observations and experiments on the lower animals, and of their very great value there can be but one opinion. But there is a variety of circumstances which seem to indicate that the lower animals are not subject to exactly the same laws in these matters as man is, and certainly there must be great caution exercised in arguing in human pathology from the analogy of the lower animals. The most important

of the researches referred to, however,—those of Lister and his followers,—have been mainly carried out in man, and consist, in a great degree, in the attainment of results in practice equally wonderful and valuable,—results that can, so far as we at present know, be attained in no other way. These results go far to justify the belief that pyæmia is a septic disease, and that puerperal pyæmia may be almost, if not altogether, prevented by the application to delivery of a practice based on antiseptic principles. We know how much has already been, and is, I am happy to say, daily done with success in this direction. But the rules of Semmelweis, or any other washing of the hands, however carefully conducted, do not constitute treatment according to the manner of Lister. Such imperfect antiseptic precautions, by use of antiseptic gauze and otherwise, I have used with apparent advantage; but we have yet a long way to go, in order to secure complete antiseptic delivery and subsequent treatment. To reach this desirable object, the efforts of several good minds are, I know, directed, both at home and abroad; and some recent unpublished cases of successful antiseptic treatment of wounds of the penis, where periodical discharges of urine have to be permitted, supply a sketch in miniature of plans that might be applied to ordinary confinements. To say more about them I have no right; but I conclude by calling upon you to give your best aid to forward the grand cause of the increased safety of lying-in women.

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF MEDICINE,

*At the Annual Meeting of the British Medical Association,
in Norwich, August 1874.*

By PETER EADE, M.D. Lond., F.R.C.P.,

Physician to the Norfolk and Norwich Hospital; President of the Section; etc.

GENTLEMEN,—This is the forty-second anniversary of this great Medical Society, but only the ninth of the meeting of its special Section of Medicine. The work to be done at the annual gathering, which at first could be comprised in a combined general meeting of the whole of the members, has gradually increased to such an extent, that three and even four Sections, all meeting and working at once, are found to be almost inadequate to the due consideration of all the matter offered; and, on this occasion, it also seems doubtful whether the time allotted will be sufficient to do justice to the value of the promised contributions and the talent of their authors. But, however this may be, I hope we shall all feel that we have met here to-day for work; that, as far as in us lies, we shall endeavour to promote the first object of the Association, which is the advancement of medical science; and that, although the Society only assembles in the capital of the Eastern Counties, instead of, as last year, in the metropolis of all England, we shall feel, when this gathering shall have separated, that we have not met in vain, and that some solid results have accrued as the outcome of this week's work.

I do not, of course, desire that the other objects of the Association, viz., the promotion of good feeling and the cultivation of professional harmony, the wielding of its undoubtedly great political influence, and the lesser objects of the exercise of hospitality, and the opportunity of investigating the special natural features of the district, should be in any way neglected; but I trust that each of these will have its proper place, and that, at all events for the present, we shall give ourselves thoroughly to the work before us. It is only once a year, and in this way, that representatives of professional thought from the various portions of this kingdom—I might almost say of the world—meet together; and therefore the opportunity of mutual interchange of opinion, if lost now, cannot be regained for another twelvemonth.

I have said that the time at our disposal is inadequate for the work to be done. Even were it not that we have already this morning listened to a long and admirable address on Medicine from an acknowledged leading thinker of the profession, I should, on the former account alone, refrain from taking up very much of your time by observations of my own. As, however, it has become an honoured custom for the President of a Section to preface the commencement of the ordinary work in his department by a few introductory remarks in his own line of thought, I will claim your attention for a few brief moments; and if, in doing so, I should make my observations too local or personal, I would ask you to remember that disease is cosmopolitan, and that local experience may be of world-wide applicability.

During the preparation of this little address, it has struck me very forcibly that the time available for it is either too long or too short. It is too long for the mere enunciation of a few introductory common-

places, and quite too short for the proper discussion or estimation of almost any one of the many great questions now prominently engaging professional thought. Much has been written of late (and by none better than by Dr. Parkes, in his address to this Association last year) on the great strides which medical knowledge has made in recent times. Perhaps, there is no better way of realising this than by carrying our minds back twenty or thirty years, to the time when many of us were students. If we turn to our note-books of that day, we shall not fail to see in how many most important respects the lectures of the then professors of medicine are wanting in much that is deemed essential to the simplest acquaintance with the theory and practice of our art as understood at the present day.

To mention only a few of the more purely medical subjects. Since the period alluded to—

The causes of fevers have been greatly elucidated, and their varieties more exactly differentiated.

The influence of poisoned air and poisoned water in producing disease has been fully recognised, and as strenuously combated by anti-septic and hygienic measures.

Very many local and parasitic diseases have been discovered or distinguished from each other.

Life, in its earliest and simplest manifestations, has been minutely examined, and new doctrines of inflammation and other diseased actions derived therefrom.

Degenerations, natural and induced, have been inquired into.

The great questions of diathesis and hereditary influence, as well as the effects of alcohol and other articles of diet, have been critically examined.

The thermometer has been (medically speaking) almost invented afresh, and generally applied to practice.

Anæsthetics have come into general use as the solace of suffering humanity.

Thrombosis and embolism have become familiar to us as causes of disease.

And, last, not least, nervous action and nervous influence have been carefully and unceasingly studied.

These are, indeed, great results and great changes. They have necessitated almost a relearning of our profession, and a continual reconsideration of our position as physiologists and physicians. But, great as they are, the field for inquiry, so far from being exhausted, seems to be ever widening, as our stand-point of observation is changed or enlarged. I am glad that there seems every prospect that in the future, as in the past, the members of this great Association will contribute their full share to this tide of advancing knowledge; and the experience of the brilliant meeting last year, and the promise of the present one, give us in this respect good cause for congratulation.

I have thus ventured, at the risk of being tedious and of telling a twice-told tale, to enumerate a few of the many great questions which have of late received such great attention. But, besides these, occupying, as it were, a class by themselves, and almost forcing themselves upon our notice, there remain those three great subjects which have been so fully discussed during the past year in our great metropolitan medical societies, and which, from their frequency of occurrence, their great fatality, and their consequent enormous importance, imperatively demand a solution, if such solution is to be obtained. Need I say that I allude to

The nature and varieties of tubercular disease;

The nature and mode of development of cancer;

The causation and spread of pyæmia, and suppurative fever.

Tubercle! Cancer! Pyæmia! We are now meeting almost at the close of the nineteenth century of the Christian era, and medical science has been cultivated for long periods beyond this; and yet tuberculous and cancerous diseases stand out, not certainly as unknown as to their nature and causes, yet quite as certain as to their fatality, as before men bent their modern energies to the elucidation of the scientific elements they involve.

By the improved practice of the past few years, the duration of life under tuberculous conditions would appear to have been materially lengthened; and, by the inoculations and experimental investigations of Villemin and his English followers, Sanderson, Wilson Fox, and others, some increased insight into the nature of the tuberculous process would appear to have been obtained. So, also, in regard to cancerous disease, our acquaintance with the anatomy and histology of its growth has infinitely increased. But the very length and breadth of the discussions above alluded to, show, I think, how much there is yet to learn on these subjects, and how vast an amount of fruit yet remains unplucked on this tree of knowledge. Even so, we may at least hope that we have thereby taken stock, as it were, of our knowledge, that we have ascertained the exact steps of the ladder of their unravelment to

which we have climbed, and that we have been able to discard once for all, many false ideas which the previous uncertainty of our position had allowed to accumulate and remain.

Some few years ago, at the Oxford meeting of this Association, I attempted, in a short paper, to trace the etiology of phthisical disease to pulmonary paralysis (a subject which has since been ably and thoroughly examined by Dr. Richardson), but I am sorry to be obliged to say that I have now no special reason for dwelling longer upon this or either of these subjects; even though tuberculosis and struma are as common here as elsewhere; though cancerous disease unfortunately affords us a very constant and copious field for investigation; and, though the whole subject of hospital hygiene, and the aggregation of the sick, has recently forced itself upon us in this city, through the occurrence of many cases of pyæmia and erysipelas in our hospital, and by the fact that this building is likely soon to be subjected to considerable modification and rearrangement.

On looking over the list of papers promised for this year, we cannot but be struck with the preponderance of those bearing upon the direct action of the nervous system; and there is no doubt that this preponderance does not inaccurately express the prevailing tendency of medical thought at the present moment. The more exact knowledge which we now possess of the brain and its subdivisions, as well as of its vascular supply and distribution; also of the functions of the ganglionic and vaso-motor nerves; together with the ascertainment of the great principle that vascular supply is regulated by the inhibitory action of the nerves, and that removal of this governing influence and control admits of ungoverned local action, *i.e.*, disease—have been sufficient to stimulate to fresh and active inquiry in this direction.

And well it may be so, when we see that many previously obscure diseases (notably chorea and epilepsy, and many varieties of paralysis), have been already rendered more intelligible through this knowledge, and that the whole process of inflammation itself is in a fair way of being at length referred to its evidently proper position of a paresis or functional paralysis.

Amidst all the investigations of our nervous physiologists, there is one direction which has comparatively seldom been taken, and that is the more exact elucidation of reflex nervous disorders. Reflex action is a word scarcely ever out of our mouths, and reflected derangement is recognised as the commonest of functional affections, and yet but little effort has hitherto been made to systematise our knowledge of these actions, or to reduce our chaotic facts to anything like exactness. It would seem that in this direction lies a great future for the patient worker, and that the doctrine of reflex action—expounded by Prochaska, Todd, and Marshall Hall, and expanded by Brown-Séquard and others—is yet destined to great and remarkable development in its application to practical medicine. For not only is it certain that many of the disorders we have to deal with are but reflected expressions of distant disturbance of function, but it is also certain that many of these reflex actions are as distinct and constant as those of more direct disease. If this be so, it is equally certain that there must be definite channels through which the nervous current passes; in other words, there must be distinct reflex arcs through and along which the morbid actions travel, and at the extremities of which the influence so exerted is respectively started and felt. In many cases these arcs are evidently long (and it is noticeable how frequently the extremities of the body are mutually interaffected rather than intermediate parts); but it is also pretty certain that many of them are very short, and that the diseased action is propagated by the nerves, afferent and efferent, merely from one vertebral segment to another. In this view doubtless lies the most easy explanation of the relations of many of our concurrent disorders, but it also illustrates the way in which disease may be spread by contiguity; the new morbid action being in this latter case the result of lesion propagated in a nervous channel through such a short reflex arc, rather than from particle to particle of adjacent tissue. In illustration of this theory let me quote the following cases, which have quite recently occurred in my practice.

CASE I.—Mr. A. had congestion of the kidneys, with prolonged hæmaturia; and then albuminuria. After a time came obscure, silent, latent pneumonic consolidation at the base of each lung. This pulmonary consolidation would yield to no treatment directed to the lungs; but, as the kidney-condition improved, so the lung-affection gradually and *pari passu* passed away.

CASE II.—Mr. B. was suddenly attacked with bilious diarrhœa, which lasted a day or two, and left him greatly enfeebled. Then, for a few days, the liver was quite inactive, and the stools white. At this stage, there came on (without any exposure) passive congestion of the base of the right lung. This congestion lasted a few days, then gradually passed away, at pretty nearly the same rate as the evacuations regained their colour and normal appearance.

CASE III.—Mr. C. suffered for a week from hiccup, sickness, dislike to food, and general stomach-discomfort. At the end of this time, it was found that the base of the *left* lung was becoming congested, although no cause for this was apparent, and the symptoms of lung-disorder were almost absent. Here, also, removal of the stomach-disarrangement at once relieved the pulmonary disease.

It may fairly be asked whether all these three cases were not examples of disease propagated to neighbouring parts in such a short reflex nervous arc as I have spoken of; and, though in the two latter the pneumogastric nerve may be supposed to have been the nervous channel engaged, yet in the first such could not have been the case; and, on the whole, it is at least as probable that in all of them the reflex circuit was the same, and that the contiguous ganglionic and spinal nerves were the agents for conveying the morbid influences from the liver, stomach, and kidneys respectively, to the thoracic viscera.

It is, of course, well known that Frerichs and other writers on the liver have noted a co-occurrence of disease of this organ and of the base of the right lung; also that such coincidences as these have often been observed. But, as this doctrine is certainly not yet received as truth, and is as yet by no means removed from the region of hypothesis, I have thought it worth while to make this brief allusion to it.

Again: arising out of this view of reflex pathology, and connected with it, I would allude to the *segmental* character of many nervous diseases, as well as their power of *alternating* from segment to segment of the body. By the first term, I mean that, in a more or less exact sense, the human body may be divided, like that of insects, into the three great segments of the head, thorax, and abdomen, with, for convenience sake, some lesser subdivisions or subsegments, and that many nervous symptoms may well be considered with reference to their relation to these segments. It is true, the exact separation of the spinal cord (and therefore of its nerves and their ganglia) into such typical segments cannot be shown; but a broad line of demarcation, in a physiological sense, does nevertheless exist. And not only so, but there is no reason to doubt that the individual vertebrae, with their related parts, do bear a close analogy to the rings and annular segments of the bodies of the Annelida and others of the lower animals.

This method of classifying and grouping symptoms into the large sections of the body is often of the greatest value for diagnostic and nosological purposes, and in simplifying the multifarious phenomena of nervous disease. And it is curious to note how naturally (when habitually regarded in this way) many vague and irregular symptoms arrange themselves in one or other of these localities, and how much their ready appreciation is facilitated by such grouping and localisation.

Again, looking at nervous disease in this "segmental" light, it is worth while to notice for a moment its power of alternation from one of these arbitrary divisions of the body to another. We talk of the metastasis of disease, and especially of such blood-disorders as gout, rheumatism, and the like. Such metastases are at least doubtful; but there is no doubt that a change of localisation of disordered nervous action does really, and not unfrequently, take place—as from the chest to the abdomen or head, or *vice versa*. Who has not witnessed a sudden cessation of an asthmatic paroxysm, and, after a brief space of time, the development of oppressive flatulence and other troubles referable to the abdomen? Or, again, who is not familiar with the alternations of dyspeptic troubles and cardiac palpitation or dyspnoea, or of palpitation with neuralgia or other discomforts referred to the head or cephalic segment? The best and most interesting study of functional nervous disorder is of course seen in women suffering from spinal irritation or uterine disturbance, in whom these principles are often well illustrated; and perhaps the most remarkable case I ever saw was in a young woman, not previously hysterical or nervous, who, after a severe blow on the loins, with apparent pelvic shock, developed disordered muscular action of the whole body; and it was curious to watch the nervous excitation alternating along her whole spinal column, causing rapidly varying play of action as it passed up and down, and reminding one irresistibly of the effect produced by running the fingers quickly over the strings of a musical instrument, or still more of the flickering and rapidly changing appearance of a linear row of gas-lights exposed to eddying currents of air.

There are no papers promised in this Section on that subject which has such a special interest in this county—viz., that of calculous disorders; although I doubt not Mr. Cadge will have something to say to-morrow upon the question of vesical calculi. These calculi are, however, quite as interesting to the physician as the surgeon, and, indeed, ought to be more so, as our speciality is to prevent them; and, if we were able to perform our duty properly, the surgeon would have no part in their treatment, as they would all be prevented, or dissolved ere they had time to form concretions large enough to require surgical procedures. Much has undoubtedly been done of late to increase our

knowledge of the chemistry and pathology and causes of urinary deposits and concretions, yet still much of certainty is wanting in this direction. Forty-six years have passed since Dr. Yelloly of this city wrote in the *Philosophical Transactions* that he was not "aware of such differences of air, water, soil, or habits of life, having been yet detected, as can justify us in attributing the prevalence of stone in the Norfolk district to any of these causes"; and, although habits of life and special qualities of drinking-water have been ascertained to have some influence on their production, yet it is clear that many other factors are at work, and that much of this sentence still holds good, and is as correct as when it was written, nearly half a century ago. We are all doubtless familiar, too, with that most valuable work on calculous disorders published by the late Mr. Crosse in 1835. I refer to it, as well as to the papers of Dr. Yelloly, in order to call attention to the comparative frequency of occurrence in this district of the oxalate of lime calculus. My own later experience shows that this so-called oxaluric condition of urine is still very common, and a very frequent attendant upon the disorders of the general poor and of the hardworking artisans of this city and neighbourhood.

The chemistry and mode of production of this oxalate of lime deposit are now pretty well understood, thanks to the investigations of Dr. Basham, Dr. Beale, Dr. Roberts, and others; but, though, in consequence of these, we must give up speaking of oxalate of lime apart from its derivative, uric acid, yet there is no doubt that a condition of urine so well described by the late Dr. Prout does exist, which is so definite, so distinct, often so continuous and so constantly associated with the same constitutional symptoms, that, if we could be allowed to speak only clinically and practically, instead of chemically or physiologically, we could verily speak of nothing but the oxalic *diathesis*. In these cases, the oxalate of lime is the only deposit found in the urine after cooling. The density of this fluid is always high, often (I have observed) exactly 1027; and there is constantly an excess of earthy salts in solution, with a preponderance of symptoms of depressed health. Alkaline remedies are rarely of benefit, but acid and other tonics, and improved hygiene, are usually beneficial.

To conclude with one word about our old city. Norwich does not yet contain much more than half the number of inhabitants which frightened Queen Elizabeth into commanding that no more houses should be built in London; and, unlike London of that day, it stands upon a large space of ground in proportion to its population. Yet, in spite of this and many other advantages, I fear it must be said that it offers its full quota of the diseases incident to aggregations of humanity. In former times, the plague ravaged it often and severely. It has had its fatal epidemics of small-pox and scarlet fever, and, of late years, typhoid and diphtheria have afforded only too abundant opportunities for studying their nature and peculiarities.

Not that our old city is comparatively unhealthy. Its average mortality per thousand is not above that of other large towns, and, indeed, not so high as that of many; but still, like most others, it loses annually the six or seven per thousand of its population who, as elsewhere, are computed to fall victims to imperfect hygienic arrangements and an insufficient command of the essentials of life.

It is probably known by report to most of those now present that the Norwich drainage is still imperfect, for the failure so far of the Norwich sewerage works, in spite of large expenditure, is notorious throughout the kingdom; but, notwithstanding this, we have happily had to record no great epidemic of typhoid or drain-poison diseases, and, though the river which flows through our midst is foul, yet no plague has in consequence attacked our houses. In one respect, Norwich is extremely fortunate in a hygienic sense. It has a copious supply of excellent water, well filtered, stored in covered reservoirs, and supplied on the constant service system. And I am happy to call your attention to the fact, that arrangements have been made to enable all who may wish it to inspect the works in connection with this true preventive of disease and warder off of many of the evils incident to a crowded population.

Gentlemen, this old city of Norwich in which we are met is a city immortalised, in a medical and philosophical sense, by having been the residence of Sir Thomas Browne. Almost within a stone's throw of this spot he lived and died, and wrote his famous essays.

He wrote in the seventeenth century, when superstition and traditional ideas still kept their hold on men's minds, and when the great light, which the science and continuous observation of the last two centuries has thrown upon all things medical, had not been shed abroad to aid and enlighten the mind of the honest inquirer. Many of the "vulgar errors" which were realities in his day to many, and which he did his best to expose, have ceased to be anything more than curiosities of the past. But I fear that very many equally important unrealities and misconceptions remain or have grown up; and I trust it may hereafter be said that, in this city, and with his philosophic spirit, by our work in

this Section, we have helped to cut down some of the scientific weeds of our own age, and not only so, but that we may have added something to the great store of knowledge which it is one of the great objects of this Association to accumulate.

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF SURGERY,

*At the Annual Meeting of the British Medical Association,
in Norwich, August 1874.*

By SIR JAMES PAGET, BART., F.R.S.,
President of the Section.

I OWE to being a Norfolk man the honour of being President of this Section, and certainly I owe it to that fact that when the honour was proposed to me I accepted it, for there was no other ground whatever on which I could have held such a position as this; but when on that ground it was proposed to me, I felt I was bound by a debt of gratitude to show myself, and endeavour, if I could by any means, to promote the success of the meeting of the British Medical Association in my own county. I very soon found that the honour brought with it, as honours usually do, a great responsibility, which was that I should give some reason for my being here, and say something of Norfolk or of what I had to do with it. And, naturally, I turned back to think of what Norfolk surgery was when I left it to be a student in London; and I soon found reason for having yielded to any invitation that could come to me from my county in the feeling of profound gratitude that I had to bear to those who were my first teachers, and among whom were found my earliest and now my oldest friends. I turned to my first case-book, commenced in 1830 and finished in 1834, when, just forty years ago, I left Yarmouth, where I was apprenticed, to proceed to the larger field of study in London. I had spent there four years, being apprenticed to learn the "art and mystery of a surgeon", whereupon let me say that the lawyers do us great injustice in the spelling of the word, which always implies that we have in our art some mystery or secret thing which other men should not dare to look into; whereas it is not the mystery but the *mestieri*, the *metier*, the mere business and art of the surgeon and apothecary. In my own case-book, I found nothing that in the present day would be worth recording—no more than the cases that might well be taken by any other boy, from sixteen to twenty, at his first sight of a large practice. But I found there some things which made me still fear (falling in very much with the tone of that most admirable and wise address that we heard this morning), lest in our progress we should have let fall some of the things which we had better have held, and are now regarding some things too lightly that then seemed very grave, and, perhaps, are so still. For among the records which I found are records of methods of practice now almost completely disused, and yet in which I feel confident there was much right. I find, for example, a large and almost uniform practice of bleeding for all acute diseases, for many chronic, for most of those that were unknown or uncertain, and for a large proportion of cases in which there seemed to be nothing the matter. Trying to learn something from these things, I came to one or two conclusions which I am sure the younger members of the profession need to have much impressed on them. One of them is that at the present day we undoubtedly overvalue the blood, and estimate too cautiously the loss of it. We heard after the address to-day of the impossibility of performing vivisections upon men. It was not impossible at the time of which I speak; for then there was a practice of vivisection performed on nearly all market days on those who wished to be bled; and the early part of my life was spent in this practice. On market days patients came, and I really scarcely know for what reason, except that it was a good old custom, and the county was then Conservative. They had some discomfort, and wished to be bled, and I bled them usually till they fainted, and the charge for bleeding was one shilling.

Now, looking at that as I can with a very clear memory of its results, and with a more scientific tone of mind than I then possessed, I can regard those as a series of vivisections fairly performed for the determination of what is the influence of the removal of blood up to the point of

syncope upon a comparatively healthy person. I think I can say surely that not one of these persons suffered harm. I kept my cases with much care, and I have a clearer recollection of that period of my professional life than I have of an equal period of any part of it which has since passed, and I verily believe that not one person suffered harm. There were a few who came afterwards with trivial inflammation about the wound, a few with light attacks of what might be called adhesive phlebitis: clots formed in the wound, and obstructed the vein for a time; but beyond that no damage, to the best of my belief, ever was sustained. Amongst these was a large number of pregnant women; for, in the eastern part of the county at that time, it was the general belief that pregnancy might not pass through its whole course without some mischief, unless a patient were bled; and in these cases, too, no harm whatever came. The custom seems like a comedy, but there are in it, and in the records of it, a series of facts of very great importance. They prove to my mind as clearly as anything of the kind can be proved, that the loss of blood up to fainting, and, in some cases I remember, with those epileptiform convulsions that come with the loss of blood, is absolutely harmless. I will venture to say, that there are few persons in this room who might not be bled to fainting, and tomorrow be almost unconscious of it, and, perhaps, in this week of hospitalities, might even be the better. That fact, I take it, may be held by the practice of former times as perfectly assured; and we, therefore, probably take vastly more care than we need of the saving of blood in healthy persons. I am far from suggesting that any good followed this method of proceeding. I speak of it literally, as if it had been simply a course of vivisections to determine the influence of the loss of blood to the point of fainting upon a large series of healthy persons. Among the cases recorded, I also find that all who suffered from acute symptoms, especially from acute pain, with the ordinary signs of fever, were largely bled. That was a custom scarcely without fail. As soon as the hot skin, and a rapid pulse, and a flushed countenance, were found with local pain or any other acute symptoms—as delirium, for example—at once bleeding was resorted to. I will not venture to say how much of real good was done by these cases; but this I will say, that I believe there is at present no remedy employed from which the immediate relief derived is so great or so complete as it was from bleeding. From time to time, I have the record, and many more times I have the clear memory, that after every bleeding there was a distinct expression of the very greatest relief, and commonly following that a firm belief that the bleeding had cured the disease. And this I remember—and I tell it as a guard against any suggestion that I should at once propose the same manner of treatment—that this relief of suffering followed in those who quite certainly were suffering from profound anæmia. Persons who were bloodless, pale, and faint, who yet complained of the continuance of the local symptom, were again and again bled, and again and again experienced temporary relief.

Call it again vivisection, if you will; at least the fact is worth recording, that, for nearly all the most pressing symptoms of acute disease, bleeding was at the time an almost complete remedy. It would be impossible, from those records, or from any others much better than mine of the same time, to determine now by any method of analysis what cases gained real and what gained only passing or apparent relief; but I cannot for a moment believe that there were none among those cases that were not thoroughly and fully relieved by the proceeding. For what have we to believe, if we suppose they were not? Why, that all those whose names we hold in honour as the most sagacious teachers of that time, the men whose acumen we praise, whom we speak of as the keenest observers of their time, were at this point profoundly and guiltily mistaken. Can we suppose that those who were teachers then—to mention only those who were the great leaders of practice in this city at the time—men like Crosse, and Dalrymple, and Wright, and Lubbock, and the rest, were keen in all their observations of the diagnosis of disease, could forecast what would be its course, and judge exactly of its effects, and yet, in respect of that to which they gave the best portions of their lives and minds, the treatment of it, were even stupidly in the wrong, and doing grievous damage to those to whom they were seeking to do good? I will take any hypothesis of pathology rather than believe that. That there were cases greatly benefited I have no doubt, and one of the chief inquiries we have to enter upon even now is which of the cases suffered harm, and which were really benefited by the treatment which we now neglect.

In another line was the almost constant use of mercury. For nearly all the chronic affections, for nearly all cases that were obscure, mercury was given to the point of producing some degree of salivation; and, amongst all the prescriptions that I dispensed most frequently, I suppose there was none nearly so frequent as a quarter of a grain of calomel and a quarter of a grain of opium every four hours. For that there was a traditional respect, which had been derived from a physi-