ADDRESS IN OBSTETRIC MEDICINE,

BY

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PUERPERAL PYÆMIA, ETC.

NOW-A-DAYS, as compared with even a quarter of a century ago, obstetrics have flourished and grown so rapidly, that he who is called upon to give an address such as this cannot but feel embarrassed in the selection of the matters to which he would call attention. If he is not to present his audience with a mere catalogue of details, he must exclude great departments; if he is not to be a mere registrar and exponent of the opinions of others, he must further restrict the scope of his review.

At one time, and that so late as last century or early in this, the whole field of medicine could be with comparative ease occupied by one mind. The same man practised and also taught all its branches, medicine, surgery, and their subdivisions. If one man proposed to do this now, we should look on him as a foolish pretender. The great medical genius of the scientific Helmholtz already has narrow limits which it cannot overpass; the great practical talents of Thomas Keith and of Spencer Wells are restricted in their beneficence by close boundaries.

Rarely and at long intervals in the course of the ages a powerful mind is produced, such as that of a Newton in physics, who, using the scattered knowledge accumulated by his predecessors, succeeds by his own discoveries and generalisations in restoring simplicity where before there was complexity, and in making for his successors a sort of royal road to the science of his day. But such a master mind, while on the one hand it simplifies and builds by generalisation, does, on the other hand, increase complexity and number of incoherent details by stimulating progressive inquiry. We hope we shall never again see the return of those dark days when one man could dare to profess or to practise the whole science and art of medicine.

It is by division of labour that we secure growth, and it is vain to regret the feebleness of our powers. As each department of medicine grows, it is, like its parent stem, doomed to subdivision. He, however, who brings the widest attainments and the most varied talents to bear on his chosen branch, will, *ceteris paribus*, be the greatest in his department. But who is equal to the demands of any department? To tell the truth, there is not among us a single man who has mastered all the details and work of even one single individual topic. One finds that the histology of his branch fully occupies his powers; another is similarly placed in regard to its chemistry; a third finds full occupation in its physical relations; a fourth, gleaning behind all these, finds his hands full in trying to be practically useful to his patients.

Partly as a result of the subdivision of labour, greater progress is being made now than at any previous period in the history of medicine; and there is no greater exhilarant and stimulant of enthusiasm than that very progress which is the result of well-judged zeal. In the field there is work for minds of every kind, for talents of every description: for him who truly observes and accurately notes details, for him who arranges or classifies, for him who experiments, for him who suggests and demonstrates conclusions, for him who confirms or overthrows by the process of verification. Every one of us should take some part in this work of advancing medicine, a more important part than that humble and useful one in which all should, in addition, take a share, the attentive listening to and applauding the diligent and the successful.

There can be no doubt as to what is the paramount cause of our more rapid progress in recent times. It is the adoption of proper method; it is the modern attempt to found or still further build up a science of medicine on what is known as the Baconian plan. Imagination and ratiocination are not even now excluded from the operations, but nothing is held as proved by mere reasoning; and when the philosopher strays far from the solid shore of rocky facts he loses his intelligent audience, who have no interest in his occupation of beating the air. The medical man proceeds nearly in the same manner as the physical philosopher. He observes by the naked eye, then by the microscope, and this instrument he aids by numerous experimental plans of hardening, cutting, tinting, and staining. He applies so far as possible physical laws to explain natural and morbid processes. He institutes new experiments to elicit truth. He makes the same appeals to chemical science, and to the practical chemist, as he makes to physics and the physicist. He reasons on what he obtains, and tests the truth of his conclusions, verifying by further observations and further crucial experiments.

Everyone who knows the history of midwifery must admit that it is by these methods alone that progress has been made. The greater part of what has been attempted, and the greater part of what is being attempted, is not according to this method; and the labour is vain. The substantial progress, often seen at the time only by a few, and appreciated by a small circle, is achieved by scientific method, and is like the grain of mustard-seed which grows and grows till it is a great tree. Obstetricians can justly boast that, in some departments, mere obstetrical knowledge is ripening into a science of obstetrics, a boast which happily some other subdivisional branches of medicine can truly make.

The first subject to which scientific obstetricians, as a matter of course, direct themselves, is natural labour in all its varied aspects; and it is in some parts of this that the greatest advances have been made. The great idea of "mechanism" so loudly proclaimed by Levet is in process of being worked out. The first stages have been nearly completed by the writings of W. Hunter and Smellie, of Ould, Solayres, Saxtorph, and Nægele. But, now that their inquiries are completed, we are landed in a more difficult and higher stage of this investigation, a stage demanding much more than the good powers of observation and description which sufficed for the former. Standing side by side with contemporary workers at these outposts of science, we are not good critics of the comparative value of their works; but among them we may signalise names familiar to you all—Hecker, Kehrer, Schroeder, Poppel, Houghton, Schatz, Küneke, Schultze, and many others.

The majority of practitioners, however, take far more interest in the management of morbid labour than in any other department of obstetrics; and this for the evident good practical reason that there lie the questions of the highest, immediate, most pressing urgency—their difficult cases and how to manage them—questions which, in the interests of the poor patients and of the anxious practitioners, brook no delay, which cannot wait for the slow decisions of science, however valuable these may be when at last they do come. But, even though science is not at hand to offer the solution of difficulties in practice, it is the duty of every obstetrician, with a view to the advantage of his confiding patients, to watch the progress of science, if not to contribute to it, as well as to select more or less empirically among modes of treatment. He should at once contribute to build up science, and at the same time be ready to meet in the field of practice the great difficulties that suddenly come in his way. Like the Jews of old, he should work with one of his hands in the permanent work, while with the other hand he holds a weapon wherewith to fight against Sanballat and Tobiah. He who directs his professional life after this manner will certainly be the best practitioner, the most useful to his immediate patients, and peradventure happily useful indirectly to the patients of all instructed practitioners in all coming time.

There is no department of obstetrics from whose scientific progress more is to be expected by the mere practitioner than that of morbid labour. This is a branch in which a great deal of valuable work has been expended with invaluable results, but much more still is required before we can style our practice rational or scientific. Morbid labour in contraction of the pelvis is a well known and typical part of this practical department; and, to permit brevity as well as to give point to our remarks, let us be supposed as referring to it alone meantime. Now, where is the practitioner to look for guidance in this matter? He can nowhere find a secure resting place. What views is he to adopt as to mechanism? What treatment is he to pursue? What instruments is he to use? Whom is he to adopt as his guide—Collins, or Simpson, or Dubois, or Hodge, or Barnes, or Hicks, or Spiegelberg, or Schroeder, or Leishman? He must act, and to the best of his judgment and ability. To decide among these able and ingenious but discordant advisers he is unable, and there is no hope of his being very soon in a position to do so. However dogmatically the teacher may write on these subjects, he settles nothing for anyone but himself and his disciples. The elementary data necessary for arriving at conclusions on these subjects are not yet acquired; but they are being rapidly elaborated. When they are worked out, then there will be a vast diminution of the area over which there is at present mere difference of opinion; and many practical directions will flow from such knowledge which, not then resting on opinion, will command, nay compel, general consent.

To secure this position for the practice in contraction of the pelvis, there must be, first of all, a nearly complete view of the whole mechanism of natural labour, not merely of what is popularly called mechanism in our text-books, and we know that we are very far off from this complete view. Then pelvimetry must be improved, and even uterine cranio-metry. Then the modes of progress of labours in contracted pelvis of

different degrees and kinds, and both when assisted and when unassisted, have to be made out. Then we have to study the explanations of these modes of progress. Then we have to accumulate experience and laboratory experiments of complexity and variety in order to discover what is the best direction of treatment. Then we have by similar means to find the tools best suited for effecting our varied purposes.

In every one of these departments, we are proud to say, most valuable labour is at present being expended, and light is being converged on this great practical matter—delivery in cases of contracted pelvis. Till this light makes the subject clearer than it is at present, we can have no final adjustment of our differences and difficulties. Mere dogmatic empirical teaching must be allowed to prevail. We must, as people do in matters of politics and religion, choose our authorities and do our duty under them as we best can. It would be tedious to enumerate the names of men who have recently contributed to the progress of this subject; but, at the risk of making omissions which are to be regretted, we may signalise among foreigners Michælis, Litzmann, Hecker, Kehrer, Dohrn, Spiegelberg, Olshausen, Fehling, Cohnstein, Pajot, Joulin, Breisky, Braun, Martin, and von Haselberg; and among ourselves Radford, Churchill, Simpson, Barnes, Hicks, Playfair, and Kidd.

After natural and morbid parturition come subjects of far more difficult investigation, natural and morbid puerperality. As their secrets are far more deeply hidden, so our progress in bringing them to light has been less and is more recent; yet it is made in a way strictly analogous, if not identical, with that which has conducted us to so much acquaintance with parturition. The steps of natural healthy recovery from childbearing are first investigated; and here much has been attained through the labours of Kölliker, Farre, Priestley, Robin, Friedlander, Turner, and many others. Then, in logical order, we come to morbid puerperality, the subject more immediately interesting to accoucheurs, one of the most important topics, not in obstetrics only, but in the whole of medicine, one which has been discussed with an excessive copiousness, but with very little advantage till recent times. There is in the whole large library of puerperal diseases not a work of considerable value before the early part of this century. We have to come down to the names of Dance and Cruveilhier, and Robert Lee, and Virchow, and Pasteur, and Sanderson, and Lister to find the indistinct bridle-path which is quickly becoming an evident broad and great line of beneficent scientific progress, a progress whose practical results will overflow, not obstetrics merely, but surgery also and medicine proper. Old books on puerperal diseases are full of disjointed facts and mere discussions. It is only now when we have resorted to statistics, to anatomy, and to researches by the microscopist and the chemist, that our way clears up towards a comparatively full intelligence of the awfully important puerperal diseases. The old works of Puzos, of Willis, and of others, are nearly as valuable as most of those of the last generation of authors. But since the times of phlebitis and lymphangitis began, this department has ceased to move in a circle of no progress, and almost every day we are reaping new increments of knowledge, not only valuable in themselves, but indispensable to still further advances.

In connection with this subject, there is a preliminary inquiry whose importance is self-evident, and which, remarkable to relate, has only recently been discussed formally, and with sufficient means—the mortality in childbed, or total mortality of childbirth and in childbed. How many women die from all causes during childbirth and in the puerperal state? Of course, such a question, lying on the surface, has been considered and answered, but the responses have been most insufficient and erroneous. The ordinary belief seems to be that there is, in connection with childbirth and lying-in, no mortality in a well conducted practice. Miss Nighingale says that deaths from puerperal diseases ought never to arise after delivery in a properly conducted and managed institution for lying-in women. In a late number of one of our principal medical journals, appears a report of one of these properly conducted and managed institutions. The hospital is a military one, and not a death is reported; and the article is evidently written with a view to show benighted civil obstetricians what is the result of proper conduct and management. I have often heard sanguine medical men say that in the course of a long and large practice they had not had a single fatal case. Now all such beliefs, reports, and statements, are mere encumbrances of the inquiry, and are to be thrown overboard, if not more ungracefully dealt with. We have no time to trifle with such nonsense, for we are everywhere surrounded by awful deaths in childbirth and in childbed, where there has been, so far as can be discovered by ordinary mortals, nothing but proper conduct and management.

Another response to this great question is familiar to all; it is derived from Merriman, and has often been repeated, not only as evi-

dence of the mortality of childbirth, but also to show the successful progress of obstetrical therapeutics by the rapid diminution of the maternal mortality. It requires such a stretch of credulity to place the slightest confidence in Merriman's tables, that I do not regard myself as justified in taking up your time any longer with them. More recently, and on much higher, even on official authority, somewhat similar statistics have been proclaimed as evidence of the progress of obstetrical therapeutics from decade to decade; but, alas for us, the evidence will not bear inspection, and we shall not inspect it. Even now, in 1874, we are only striving to reach a sound conclusion as to this mortality; and after all our labour, official and private, have no statistics to be relied on for the comparison of the results of successive decades or of longer periods. The importance of the question needs no demonstration, and it must be solved in a scientific manner. We are seeking not what we fancy or wish, but what is. There can be no doubt that the death-rate does represent marriage and childbearing as a most perilous ordeal for a young woman to encounter; and it is not good reasoning to use this, as a great author has done, to prejudice us against receiving what may be proved. During the whole life, including intrauterine existence, the female half of mankind has a great advantage over the male in point of mortality, except that for a considerable time childbearing brings the female nearly to the level of the male; and that, for a less time, the risks of primiparity sink her below him. Childbearing is in these kingdoms the special, and, so far as known, the only special great cause of enormous increase of female mortality above what it would otherwise be. Primiparity produces a great exaggeration of the childbearing risks. It is this mortality that we seek to estimate, and you observe it is of the highest human interest; but it is for medicine of special importance, being a cardinal element in the solution of the question of the value of hospitals. These noble institutions, the lighthouses of practice, have had their reputations tampered with on the most insufficient grounds. If we are ignorant of what may be called the normal mortality of childbirth and lying-in, how can we justly judge the hospital mortality? If we do not estimate the excess of special, and, in a sense, just causes of mortality in hospitals over those acting in the country generally, how can we fairly measure the salubrity of maternities? It is impossible to do so, yet there has been a lamentable and injurious amount of such mere cavilling with institutions whose reputations should be too sacred for any but the most solemn and logical consideration.

Among the Chinese, puerperal mortality is held, according to Dr. Jamieson, to rise as high as from 1 in 12 to 1 in 20—an alarming and scarcely credible statement; yet Dr. Thin, lately of Shanghai, believes it to be true. Some statistics of the city of New York, recently published by Fordyce Barker, yield a puerperal mortality of 1 in 35, a terrible result; and on the surely extravagant admission that a quarter or even half of the births were not registered, we have here a very high figure. Faye states the puerperal mortality of Prussia as 1 in 84; and that of Finland, according to Pippingskjöld, as 1 in 106; and that of Norway as 1 in 131. I found that in Edinburgh and Glasgow, in 1855, the mortality of married women within six weeks after delivery was 1 in 107 at least. From a large collection of data, and trying, however rudely, to get an approach to exactness, I estimated the mortality within four weeks after delivery as about 1 in 120.

Hervieux says there are places where for a long series of years this rate has not risen above 1 in 1,000; but he must derive his information from some other planet than this, for certainly there is no such abode of the blest known among the inhabitants of this earth. Le Fort estimates the rate as 1 in 212; but his figures and reasoning are such as to render this determination unworthy of any reliance, as has been sufficiently demonstrated. Farr has carefully estimated this rate, and arrived at the conclusion that it is 1 in 190. But, however much we may be disposed to bow to his authority, we are bound to scrutinise his method; and, on doing so, it turns out to be very unsatisfactory. He is dealing with a system of registration which is not compulsory; he seeks to verify the returns relied on by appeal to the returners, which is something like trying to correct an error by itself; he made no independent search for the deaths of the delivered women; he made no correction for twins, nor for still-births. On all these accounts I regard his result as being not only out of keeping with the best of the others, but as not especially reliable. The data of foreign countries which I have given may be very good for aught I know, but then I have no positive knowledge of the care or of the circumstances under which they were compiled; yet we have always been led to regard the Swedish and Prussian returns as very valuable.

In this state of matters I was not disposed to allow the point to remain unsettled for this country, and I recently undertook the somewhat onerous task of thoroughly searching official returns, with a view of getting a figure that could be relied on. The determination which

I am about to give can be erroneous only in the way of making the rate too low.

I found that there were registered in Edinburgh and Glasgow, in 1869 and 1870, about 52,000 births, and I found that within twenty-eight days after delivery at least 1 in 139 of the mothers had died. Now, several mothers additional may have died, and their deaths have been elsewhere registered, they having left their original residences. These would slightly increase the rate if they were found; but the rate is too low for another reason—namely, that all births of dead children are omitted. Now this very serious omission of a large proportion of the most dangerous labours leads to this rate of 1 in 139 being far too low. How very far too low, we may to some extent conceive when I call to mind that, among Collins's 16,414 women delivered, 164 died; and of these 164, nearly one-half had dead children! It may be said, then, as the result of this investigation—the most careful and complete, so far as I know—that at least 1 in 139 died; and I add, for the reasons above given, and for others, that I have no doubt that at least 1 in 120 died. These terrible results, gentlemen, or something closely approximating, we must accept meantime, however forcibly they may demonstrate that marriage and childbearing are a fearful ordeal for a young woman to encounter.

My estimate, gentlemen, of this lamentable mortality of lying-in women is 1 in 120 within four weeks; and it is useful to have a fixed period of four weeks for various reasons. But we must not allow ourselves to be misled into thinking that puerperal mortality is over in four weeks. You are well aware that many bad cases linger on beyond the month of four weeks, to die beyond the reach of these restricted statistics; and that many others owe their deaths to puerperality, although the occurrence is later than four weeks after the labour. I have already mentioned that I have most carefully prepared statistics showing a mortality of at least 1 in 107 within six weeks after delivery. Further, I have statistics analysed which do, I believe, show that the mortality of puerperal women does not again fall to its ordinary level till a period not of weeks, but of months, after delivery. It would be a grand work for our young statisticians to show the wave of special mortality, beginning with conception and ending some months after delivery. Statistics have already shown the great rise of mortality, or the great wave of it that passes over the sex during the childbearing age. But we want much more than this, and especially the wave for the average individual pregnancy, labour, and lying-in.

I must conclude this already too long discussion by saying that I believe that in this country nearly 1 in every 100 women delivered at or near the full time dies in parturition, or before the puerperal state and its effects have passed over. This is, no doubt, an awful statement for women and for men. Whether it will deter them from marriage or not when they come to know it I cannot say, for I have no analogy to guide me. The risk from railway accidents is comparatively a mere bagatelle, when taken in any point of view, and I have made no inquiry as to its influence in deterring from travel.

Even the fear that women may be deterred from marriage and childbearing must not deter us from unmasking the real extent of the dangers they encounter; but I must only spend a few words on puerperal morbidity. Besides dying at a rate of nearly 1 in 100, women have to encounter a vast amount of disease and suffering which does not end fatally. This has been called morbidity, in contradistinction to mortality. Miss Nightingale and Dr. Farr, besides having very favourable views of mothers' chances of survival and recovery, go a great deal further than this, and enunciate a doctrine to which it is difficult to believe they have ever given a moment's reflection. They regard ordinary women as having no need of long nursing after lying-in; for it is all over, say they, in a few days after retirement and delivery in the rude compartment of a hut. It is unnecessary to take up the time of any one of the most moderate experience in confinements and diseases of women among the poor or the rich, among the civilised or uncivilised, with a deliberate demonstration of the tragical injustice of their statement. I only refer to it here as it forms a contrast with the truth as to puerperal morbidity. This subject has been illustrated by many authors, among whom are Späth and Landau, who confine their researches to morbidity shortly after delivery. The latter, taking temperature as a criterion, estimates puerperal morbidity as affecting 1 in 6. His valuable experience was in an obstetric hospital, and it requires corroboration. Besides this, he takes no account of the many ulterior diseases coming on after so-called recovery.

Deaths during parturition or the puerperal state are often conveniently arranged in three sets:

1. Childbirth deaths.
2. Puerperal or metria deaths.
3. Accidental deaths.

A woman dying during *post partum* hæmorrhage undergoes a childbirth death; a woman dying of puerperal fever undergoes a puerperal or metria death; a woman accidentally poisoned by laudanum shortly after delivery undergoes an accidental death. In cases such as these the placing of the death in its class is easy, but there are a large number of cases regarding which there may be just difference of opinion as to which of these three groups should receive them: hence the classification as used in practice cannot be relied on as embodying a scientifically accurate statement of any point—a circumstance which, for the conduct of various important discussions, is much to be regretted.

But though this is so, there is unanimity in placing puerperal fever deaths in the second category, that of puerperal or metria deaths, and in giving it the horrid pre-eminence over all other causes of mortality in the three combined categories. For obstetricians and for the world, then, this is the subject of first importance in midwifery, and it has attracted a corresponding amount of attention, and never more than at present, and certainly never with so much advantage.

The oldest writer on puerperal fever, Willis, whose book was published two hundred years ago, propounded a theory of this disease which is remarkably like that now in vogue among the best pathologists. He regarded it as having some connection with an uterine wound; but then he did not know the nature nor the anatomy of the uterine wound, and he did not think any of the other wounds or injuries of the lying-in woman important in the matter. He regarded the disease as a fermentation in the blood, but his notions as to fermentation, its causes and results, were imperfect in themselves, and extremely unlike ours. He attributed to fancied sulphureous particles somewhat of the importance and place that are now given to the bacteria of Mayrhofer, of Lister, and of Heiberg, or to the micrococci or globular bacteria which Orth, and I believe Heiberg also, regard as holding the supremely baneful position among these noxious beings. Our new theories of puerperal fever must be tolerated because, as philosophers, we delight to frame them; and they have a certain utility which this is not the place to discuss. The new theories far surpass the old ones, which deserve more thorough displacement and rejection than the profession has yet awarded them. The new theories are based on an accumulation of facts whose collection is most creditable to modern science. These valuable facts have been the fruit of the ceasing to philosophise, and the struggling to observe and to experiment which are the characteristics of modern pathology. As in parturition, so in the puerperal state, the progress has arisen from discovering mechanism, although we do not speak of these advances in such terms. What are our discoveries in the anatomy of the lymphatics but the mechanism of puerperal fever or a part of it? What are thrombosis and embolism? What is the conveying and diffusion of bacteria, or of any septic poison? What are our antiseptic precautions? By statistics, by observation, by experiment, we accumulate facts more or less pertinent to the subject, and we arrive at theories having a wider and more solid basis than those of our predecessors, who long and till recent times, misled by the *ignis fatuus* of an essential puerperal fever, were destined to make little real progress till they threw that notion aside and began working at the matter again according to better method.

No theory of this subject can be regarded as final or sure. But the time has come when obstetricians should try to leave off the use of the convenient term puerperal fever, because it embodies error. There is nothing essentially puerperal known in it; nor is there anything of the nature of a fever, as that term is generally understood. A new name, already widely used, is to be found in the already comparatively old term, *pyæmia*. This new name can be of only temporary utility, but that utility will be very great, and continue till advancing science displaces it by a better, as it should now displace puerperal or childbed fever. It will then have served its time by carrying the ideas of generations of practitioners away from the old, flimsy, and extensively erroneous speculations of the past to the more substantial of this day. It is not to be supposed that *pyæmia* is a term to be analysed into its component parts and held as implying purulent blood. That was once the meaning of *pyæmia*, but it is not so now. The crude pathology of Piorry is already almost forgotten, and his term *pyæmia* is used extensively among the best pathologists as a comprehensive word, identical with or including the septicæmia and ichorrhæmia of certain others. It sounds like an adoption of humoralist views, but in it there is as much of solidism as of humoralism, and there is in it vastly more of modern science than in the term puerperal fever.

Pyæmia occurs in several forms, which are characterised each by more or less peculiar symptoms, but most distinctly by the pathological appearances discovered *post mortem*. There is that most widely known when you have septic embola, and scattered abscesses caused by them, and perhaps otherwise also. There is that where you have inflamma-

tion of the peritoneum and other serous cavities, including the synovial and endocardial. There is that where the mucous membranes are chiefly affected—the muco-enteritic. And, lastly, there is that where the only results found after death are—alteration of the blood, enlargement of the spleen, the liver, and degenerations of their most important tissues, with similar degenerations in other organs. It is this last which, often rapidly fatal, was described by Helm, and is now often called acute septicæmia. These are the cases which the superficial pathology of our young days described as having no *post mortem* appearances at all. An autopsy in those days was made by any practitioner, occupied only a few minutes, and the observations made were of corresponding value. Now an autopsy is a matter understood to demand the labour for a long time, often for hours or even days, of an expert. On these fruitless necropsies, where no appearances were discovered and none supposed to be discoverable, was founded, as you will remember, an argument supposed to be of clenching potency in favour of the essential fever character of the disease. But I confess I have never been able to discover either the logic or the power of the demonstration.

Easily getting rid of this old argument, we come sharply into contact with a new difficulty. When I say that Weber, Bergmann, Billroth, Hüter, and Verneuil support the doctrine, that Olshausen holds it an open question, and that Sanderson, in his essay on the *Infective Product of Inflammation*, demonstrates truths which seem at least to favour its pretensions, you will see that the matter has already occupied great minds. The doctrine is antithetical to the essential fever notion, for it states that pyæmia, or so-called puerperal fever, does not essentially differ from ordinary inflammatory fever, such as is called healthy, except in degree, and that the modes of induction of these feverish states are identical, or nearly so. From the slightest pyrogenous effect or merest evidence of morbidity, as discovered by the thermometer, up to the most rapid of Helm's cases of acute septicæmia, we have one disease in different degrees or forms, all depending on a chemical poison of Schmiedeberg and Panum, or on the bacterium of Mayrhofer, of Lister, of Klebs, of Waldeyer, of Heiberg, and of Orth, whether this bacterium be the poison, or only its carrier. That there are weighty reasons for entertaining this view must be admitted, and among them not the least are the wonderful results of the antiseptic system of treatment, as not only preventing pyæmia, but as preventing ordinary inflammatory fever. But practitioners of my own age, or greater, will find it difficult to get rid of, or controvert, the *primâ facie* evidence in favour of the old views afforded by the great array of facts and ideas which forms the basis of our daily reasonings in the guidance of practice in healthy and unhealthy inflammations, and which furnish a set of arguments which have been well stated by a reviewer in a late number of our medical quarterly journal.

The disuse of the term puerperal fever, and the replacement of it by puerperal pyæmia, is a change which has already been carried out by many of our best obstetric authors. The old designation is so impregnated with erroneous and misleading theory, that it cannot, within a reasonable time, be purified, and will probably be most advantageously subjected to destructive cremation. Fordyce Barker, a recent American author of great intelligence, still upholds the old banner—"an essential fever peculiar to puerperal women, as much a distinct disease as typhus or typhoid". He well knows how pathologists believe they have torn this view into tatters, and he ought to have given us good evidence of its being reparable, if not actually rehabilitated, but he does not even attempt the difficult task. When we are asked for evidence as to the specific characters of typhus or typhoid, we can easily produce them, and defy the farther destructive analysis of these diseases. It would be a waste of time to go over the special history of the causes, progress, and results of these diseases. They are well known, clear, and convincing to all. For puerperal fever, we have no such characters—no such evidence. All the evidence brings the disease into the closest alliance or identity with surgical pyæmia. The grand modern history of pyæmia is, in fact, at every step of its progress, the history of the elucidation of puerperal pyæmia, or of so-called childbed or puerperal fever.

One error is sure to bring another in its train; and so we have the widely prevalent belief that this disease is like cholera, or small-pox, or typhus, in occurring sporadically, but chiefly epidemically. Here it must be observed that many authors use the word epidemic carelessly, or as synonymous with endemic; a common error, which should never be committed. But, knowing this, we find them almost universally believing in real epidemics of puerperal fever, describing them as sweeping over a country or devastating a continent. It is well known, and indeed needs no further proof, that the disease has often the appearance of being endemic in an hospital, that it attaches itself to and follows certain individuals in their practices; but I have not been able to find anything worthy of the name of evidence to prove its epidemic preva-

lence at any time or in any large district. You are all, no doubt, familiar with the long descriptions and marvellous statistical compilations adduced as evidence of this doctrine by our best writers, especially by medical historians, among whom Hirsch is pre-eminent. But when these statistics are subjected to scrutiny, they are all found wanting, as may be made plain by one example, and the examples are all from old times. We have few epidemics described in recent times, and these few do not produce respect for the doctrine implied in their description. Yet though there are few descriptions now, there is still everywhere the erroneous belief. Epidemics are described as having occurred in Edinburgh in 1772, 1814, 1825, 1833. But it is mere assertion. There is not a tittle of proof that the disease was not as prevalent in every year as in the years of the so-called epidemics. When a physician, struck with awe by a few cases, writes a description of them, down it goes in the statistical tables as an epidemic; and the year of it is not always that of the cases, but sometimes that of the publication of the pamphlet or book. When the horrid mortality rises in an hospital, from overcrowding or other causes, down it goes again as an epidemic; and with this supply of burlesque evidence the manufacture of epidemics never ceases. Registration arrangements are not required to show the epidemic character of cholera, or of small-pox, or of other fevers; and, when we do get the valued registration statistics, we get the proof in proper form. But if puerperal fever is to be shown to be ever epidemic, it must be by regular modern registration statistics. The prevalence of it is happily never very great, comparatively speaking, and, again, it is never extensively or long absent; and, when we appeal to such statistics of metria as are accessible, we do not get proof of epidemic character. We find it always present, in every county, in every community. It is easy to get proof of the epidemic characters of fevers, but not of puerperal fever. He who studies this point will find metria to vary in its ravages as pneumonia does; he will find cholera or scarlatina varying in their ravages according to a quite different law.

As the disease is erroneously believed to be a fever and to occur in epidemics, so we have a corresponding erroneous theory of its origin or causation. Many authors delight to speak of cosmic or of telluric influences or of miasma as producing the disease, and such subjects are favourites with a certain class of minds which find it most agreeable to enter at great length upon those topics of which they know very little or even nothing. For such there should be a puerperal Zadkiel. It is very difficult to find any evidence for the existence of a miasma even in the air of a pestilential hospital, for puerperal pyæmia prevails in such a manner as is scarcely reconcilable with the miasma hypothesis, and, on the whole, easily reconcilable with more or less direct communication with diseased neighbours, as Veit has ably shown. Among the circumstances of prevalence to which I allude are the comparative immunity of women brought into the hospital already delivered, the special liability of primiparæ, the special liability of those who have tedious and difficult labours.

Again, as the disease is believed to be a real or essential fever, so, of course, it is believed to be contagious and infectious, or both—whatever these terms may mean. In recent times, this mode of communication has come to be regarded as so certain and yet so subtle and mysterious that many teachers and a great body of practitioners have been terrified out of their senses by it. One cries out that the obstetrician must not wear gloves, and it would be just as rational to say he must not wear clothes. Perhaps he might be permitted to paint, and go about his practice, as the ancient Britons fought. Many say that the practitioner who has a case of puerperal fever must give up his practice and go through various processes, and not return to his avocations for a period varying from a fortnight to six weeks. Others, on this point speaking logically, go further, and say the accoucheur should give up his practice, not only if he has on hand a case of puerperal fever, but of many other contagious diseases, such as scarlet-fever, typhus, small-pox, measles, erysipelas, fetid abscess, etc. Of the many who propound or teach such doctrines, I have never known one who practised them, and I cannot say their feelings on reflection are to be envied. If such be good doctrines, they are, of course, equally good for physicians and surgeons as for obstetricians, but the poor obstetrician is laden with restrictive burdens which his medical and surgical brethren do not recognise or touch with one of their fingers. Yet, the old proverb tells here, that what is sauce for the goose is sauce for the gander, and accordingly physicians and surgeons must follow the rules they inculcate on the accoucheur. In truth, these extreme practical doctrines of contagion are absurd, for they render all practice, whether medical, surgical, or obstetrical, an impossibility, or at least reduce the number of patients cared for at a time to one, which amounts to the same thing. In this matter much error and evil are, I believe, introduced by confusing the duties of the practitioner with those of the nurse, two quite different and almost, if not altogether, incompatible occupations. A practitioner

must always, in such circumstances as we are now considering, carefully eschew undertaking the functions of a nurse, for, if he do, he must submit himself to the code of rules that regulates the conduct of nurses. If, avoiding a nurse's duties, a medical man of any kind cannot make himself medically clean in hands and person and dress, all kinds of medical practice as at present carried on are impossible with due regard to the safety of patients. Everyone who knows the safety of actual medical practice must see that such views of contagion end in absurdity. But it is not, despite all this, to be supposed that practitioners are not bound by the most solemn considerations to take most scrupulous care against being disseminators of disease; and there is no disease with which they deal, where such care is more imperative on them than puerperal pyæmia. The puerperal woman presents in her contused, lacerated, and inevitably wounded passages the most favourable nidus for the reception of morbid material; and the woman suffering from puerperal pyæmia in any of its forms, and patients suffering from some of the allied diseases, present this morbid material in its subtlest and most potent essence. A well-demonstrated communicability arises from this source. No other has been demonstrated, but it is possible that in an ill managed hospital there may be some other. For the existence of another source, several of the best recent authors offer slight evidence; but, on the other hand, its existence is rendered very doubtful by the alleged absence of pyæmia in those surgical hospitals or parts of hospitals where the antiseptic treatment of Lister is properly carried out.

Another result of this extravagant and superstitious dread of contagion is what I deliberately call the slandering of our noblest and most useful institutions—hospitals—and in that word I include all hospitals for the sick, not those for lying-in women only. Although it is against the latter that most of the foolish talk is directed, it is vain to suppose that they alone suffer. If one kind of infirmity be indefensible, so are all kinds; they must stand or fall together. The laws of pathology are not varied in nature with a view to the misfortunes specially of lying-in women. No doubt, the slandering is done with an excellent intention, under a good motive, but it is none the less what it is called, censuring injuriously and falsely, or without sufficient evidence. There have been, and there may be now, maternities which are justly calumniated as injurious, but that fact is no excuse for calumniating all. After abundant evidence has been adduced to show the directly and indirectly erroneous character of Le Fort's statement, that while the mortality of hospitals is 1 in 29, that in private practice is 1 in 212, a recent lecturer on pyæmia repeats it as if it were uncontested, and weakly appeals to authority on a point capable of scientific demonstration. We have reliable and large statistics to show what a moderately good hospital is, and we have no thoroughly reliable evidence that better results are anywhere obtained, whether within an hospital or in private practice. Among the good, I place the Rotunda of Dublin, the reports of whose recent condition you may have seen from the pen of Dr. George Johnston, its present master. So great is this superstitious dread of hospitals and reliance on imperfect statistics, that one eminent author believes he has made out that amputation of the forearm performed upon a poor man in his cottage is thirty times less fatal than if it were performed in an hospital. The paradox is not to be received because its basis is inadequate; and, considering whence come most of our hospital patients, I wish we had from this author some theory of the healing virtues of the concentrated and various filth of a highland bothy or cottage, or of a den in the Edinburgh Cowgate, or the London Ratcliffe Highway, accompanied, as it often is, by every abomination physical and moral.

As hospitals are in this facile manner made out to be bad, so, of course, large hospitals are worst, and statistics are again appealed to in support of this view. Were it not that a recent lecturer on pyæmia repeats this statement, I would not here allude to it, for it has been shown to be groundless by demonstrations as good as can well be imagined, but which seem not to have reached the eyes or mind of this eminent surgeon. Siebold believed that lying-in hospitals were so useful that shutting them up would bring far more serious evils than an occasional outbreak of puerperal fever. Whatever soundness there may be in Siebold's judgment, I prefer, with Steele, to adopt a different view of maternities, and to look forward to the time when there will be no endemics of puerperal pyæmia, but only sporadic, or what are called autogenetic, cases.

Other errors connected with the old and still prevalent opinions regarding puerperal fever, though worthy of comment, must be passed over; but one, from its importance, demands notice. It is, that this disease is a kind of cholera, or a kind of typhus, or a kind of scarlatina, or owns the same or similar causes. In this country, it is almost exclusively the identity with scarlatina that has found supporters. But at home and abroad the doctrine has, in some form or other, been extensively entertained. It is to be carefully distinguished from that reasonable view of Pouteau, Alison, Sidey, Nunneley, and Tilbury Fox, that

the disease has close alliance with erysipelas, and, I might add, with the diffuse inflammation of Duncan, a view which has been partially adopted and signally illustrated by Virchow in his now celebrated paper on Diffuse Puerperal Parametritis. Scarlatina is a source of terrible danger to lying-in women, and scarlatinoid rashes are seen in some of the worst cases of septicæmia; and herein probably lies the attractiveness of the theory. But, unluckily for its supporters, and especially for the most recent, the theory has been disposed of by the researches of Hirsch and of Veit, who, by statistics of the comparative prevalence of scarlatina and of puerperal fever at different times and at the same time, have shown that there is no relation between the two. This argument against the theory is far stronger than any in favour of it, and must meantime be held as conclusive. It had been urged by these authors and by Späth before the *Lancet* did the good service of bringing Farr into the field on the same side.

Every change in doctrine or in name does not necessarily indicate progress; often, indeed, it indicates retrogress. Such unfortunate changes arise more frequently from error in philosophising than from error in observing. Among such, there is one in the history of our present subject. It has by some pathologists been proposed, not only to continue the name puerperal fever with all its adherent errors, but to go far towards introducing like errors into surgery by describing the allied diseases there as surgical fever. Had there not been the erroneous use of the word fever in childbed diseases, there would undoubtedly have been one hindrance less of the general acceptance of modern views. Progress in surgical pathology runs no risk of being now impeded by the false name, surgical fever; and its adoption will certainly not do any good. By adopting, instead of puerperal or childbed fever, the term puerperal pyæmia or some similar one, we, by the mere use of words, enforce the argument for a great medical generalisation, making the gains of surgical and obstetrical science mutually beneficial.

It must not be supposed that there is in the mere change of a name any real progress, however much such change may foster right views. Nor is this change of name to be held as even tantamount to a change of theory, from a false to a true one. The present state of our knowledge is not such as to justify a sense of great security in any theory. Yet, theories are very attractive, nay useful, and the tendency to frame them, whether prematurely or not, is in vain repressed. All the more ought we to maintain our minds in an unprejudiced attitude, ready to throw away the worse and cling to the better. Willis, who introduced the name febris puerperarum two hundred years ago, held that it was the result of sulphurous particles and fermentation. This has a faint resemblance to the new pyæmic theory, with its bacteria or micrococci, its septic and infective materials; but Willis's theory and every recent modification of it is a mere shadow compared with the pyæmic theory. The former can scarcely be said to explain or embrace any of the chief details of the subject, while the latter explains and embraces a vast number, is incompatible with none, and is the most promising road to still further explanation and generalisation. It is the last philosophic production of a vast array of modern observations and inquiries. It helps to increase the distance between us and the mere philosophers, and to join us with the advanced guard of modern medical investigators, whose weapons are observation and experiment, not learned talk, however clever. When the physicist finds a theory of light propounded by the greatest of his kind to be inconsistent with his more advanced researches, he not only adopts a new one, but deserts the old nomenclature connected with the misleading hypothesis.

Many researches of different kinds have contributed, and are contributing, to converge scientific light on this greatest of practical, obstetrical subjects; but scarcely one of them can be regarded as being even yet completed, while some are only well begun. In drawing to a conclusion, it will be well to take a glance at these various investigations, with which we may class some more general discussions, such as that of Spiegelberg, on this topic.

Van Swieten, Willis, and many old authors on puerperal fever, regarded it as a wound-fever, and Eisenmann's well known work on the subject, published in 1837, is called "Wound-Fever and Childbed-Fever". The wound which these, and even most recent authors, have in view, is that produced by the separation of the placenta; but it is now well known, and has been often found clinically exemplified, that the disease may begin in a contusion or laceration of any part of the genital canal. Many of you have recognised the beginning of a fatal disease in a diphtheritic state of a slight recent perineal laceration, with surrounding redness and boggy swelling. I have already alluded to the recent advances of our knowledge of the anatomy of the placental wound, but already we know where to look for much more light on this subject. The anatomy of the lymphatics, to which recently Recklinghausen, Klein, Thin, and many others, have contributed, is not yet completed for the uterus. We all know the fine-looking and distinct,

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-