2. These affections do occur in puerperal women, and the result is a disease which does not constitute a continued fever "communicable by contagion".

3. When either of these affections complicate puerperal fever, they modify the clinical phenomena by symptoms which can be distinctly appreciated and described by any close observer.

I will not take up your time by giving the evidence on which these propositions are based, because I think that this has been done in a work *On the Puerperal Diseases*, which it will be indelicate for me to refer to in more definite terms.

I will add only a few words in regard to the pathological anatomy of the disease now under discussion. On the first evening of the discussion, Dr. Richardson argued that there were no local lesions "which would lead us in the dissecting-room in any case to say this was a case of puerperal fever, as we should say this was a case of scarlet fever or of typhoid or of typhus fever". I think it would be impossible to prove by the anatomical lesions that scarlet fever or typhus fever, or relapsing fever, are distinct diseases. A child is put in bed at night apparently well. A few hours afterwards, it is awakened by vomiting, it is very delirious, its pulse is very rapid, and the temperature is six or seven degrees higher than is normal, and the child dies in the early morning without the slightest irruption on the skin. No anatomical lesions can be found to characterise the disease. But scarlet fever is epidemic in the neighbourhood, or another child in the same family is taken ill with well marked scarlet fever within a few hours after the death; and any physician would feel warranted in registering the cause of death as scarlet fever. Such cases are not very rare, and, I dare say, have been met with by several gentlemen now present. Would the most able of the recent authorities on the continued fevers—would Sir William Jenner, or Murchison, or Hoffman, or Lebert—insist that no one would have a right to register a death as resulting from typhus fever or relapsing fever, unless certain characteristic anatomical lesions are found in the dissecting-room? All of these writers declare that these diseases have no pathognomonic lesions.

It was asserted by the author of the Address in Obstetric Medicine, that "pathologists believe that they have torn to tatters the view" that the disease we are now discussing "is an essential fever peculiar to puerperal women, as much a distinct disease as typhus or typhoid".

When? how? where? Show us the proof. Is there evidence that such pathologists have studied the disease anywhere except in hospitals and large cities, where the disease is very probably complicated with septicæmia or pyæmia?

Of what value would be the researches of the ablest pathologists of Germany or England on questions relating to the pathology of yellow fever, if such pathologists had no opportunity of studying this disease, except in Germany and England? Have Spiegelberg, or Schroeder, or Schmiedeberg, or Mayrhofer, or Orth, or Heiberg, or Olshausen, or Fehling, or Cohnstein, or Breisky, or Von Haselberg, ever studied puerperal fever as it is found in epidemics in rural districts, where pyæmia and septicæmia are rarely met with? German is a difficult language to acquire; and it gives an attractive appearance of learning to introduce such names, even if nothing be quoted from them. I am familiar with their writings, and would not undervalue the merit of their researches; but so far as the elucidation of the question now under discussion is concerned, I think it of equal importance that we should carefully study for the clinical phenomena of the disease your English classical writers—Hulme, Leake, Kirkland, Clarke, Gordon, Hey, Armstrong, and Robert Lee. The hint on this point thrown out by Dr. Farre at the last meeting seemed to me most appropriate and timely.

What progress is made in science? Now do "we free ourselves from error", or gain in scientific precision or accuracy of description in giving to this disease a new name—pyæmia, as Dr. Duncan would call it; while he confesses that it has no etymological significance in this disease, and, in fact, has no definite positive meaning?

What propriety is there in giving to an obstetrical disease a name significant and appropriate to a surgical disease, unless it can be demonstrated that the two diseases are identical in their clinical phenomena and their anatomical lesions? The burden of proof to show this identity belongs to those who would call the disease puerperal septicæmia. Some think that the negative has already been established.

I have neither the time nor the voice to discuss the other questions which have been proposed. With my warmest thanks for your courtesy in listening to me so patiently, allow me to close with the suggestion that it may be well for all of us who discuss puerperal fever to remember the exhortation of Oliver Cromwell, when he lost patience with a Scotch Assembly: "I beseech you, brethren, by the mercies of God, conceive it possible that you may be mistaken."

THE RELATION OF PUERPERAL FEVER TO THE INFECTIVE DISEASES AND PYÆMIA.

Address delivered at the Obstetrical Society of London, July 7th.

By CHARLES WEST, M.D., F.R.C.P.,

Physician to the Hospital for Sick Children, etc.

I CAME here to-night because I felt that honour implies duty; and, having now for some years had the honour of being one of your Fellows, I felt that, if on the occasion of so important a discussion as this, I stayed away, it would seem either as if I cared nothing about the exercise of my profession, or about the furtherance of it; or as if some other motive—I know not what—had kept me away from your assembly. What I have to say, I will say in as few words as possible.

And, first of all, I have but one criticism to offer, and that bears on one of the questions which have been suggested with reference to the share which certain microscopic organisms bear, or may be supposed to bear, in the production of the disease called puerperal fever. It appears to me—with all respect to the gentleman who in his able paper proposed that question—that it is somewhat out of time to raise it. We have not as yet a sufficient amount of knowledge on the subject to be able to apply it, or to draw from the observations that have already been made any correct and useful inference. No one can, I think, have read with care the very interesting discussion on the subject of the "Germ-Theory of Disease" without being struck by the way in which men of the greatest talent and the sincerest love of truth differ, not merely with reference to the conclusions that they draw, but also with reference to the very facts that they thought they had observed. I think, if this matter is to be settled, it must be settled in the first place in the case of some disease simpler in its nature than puerperal fever. I pass, then, from that subject in these few words, in which I would still wish to convey the very high respect I feel for a kind of knowledge to which I can make no pretension, and to assure you that what I have said means no disparagement to study carried on by means of the microscope, and no doubt as to the great results that hereafter we may arrive at from it; but I refer to the subject from the conviction that the question so raised is, in the case of puerperal fever at least, premature.

I now approach the other and more practical question. It may be allowed me, perhaps, to remind the Society of what, I doubt not, they have not forgotten: that some thirty years ago a physician, now taken from amongst us, but whose professorial chair it was my high honour for a short period to occupy, wrote a work on the very subject which we are now discussing; and I do not myself see that we have advanced far, if at all, beyond the conclusions which Dr. Ferguson laid down, that the phenomena of puerperal fever depend upon a vitiated state of the fluids, and that, in the case of a woman in the puerperal state, such vitiated state of the fluids is specially apt to arise. I do not think, therefore, that we can in any way assimilate the condition of what is called puerperal fever to typhus, typhoid, or scarlet fever. The disease wants those characteristics which more or less distinctly prevail in the case of the specific fevers. It has not the same regularity of course, the same regularity of incubation, the same regularity of outbirth. It has not the same diagnostic marks which we find in the case of the specific fevers; such, for instance, as the pustules which appear, even though in small number, and characterise even the slightest case of small-pox. Nor is this all. But with reference to these fevers we know, further, that for the most part, at any rate, they produce disease like to themselves; and that, further, though with many exceptions, they have a tendency not to recur where once they have happened. Now, all these characteristics are absent in the case of puerperal fever; while the very circumstance that a far greater proportion of cases of puerperal fever occur in primiparæ than in those who have given birth to many children, shows that there is a marked connection between the liability to an outbreak of the disease and the difficulties attendant on the process of labour. If, further, we expose a man to the contagion of puerperal fever, he does not get any disease as the result of it. If we expose a woman in the puerperal state to the contagion of scarlet fever, measles, or some other disease, she may have that disease, even though its characteristics may be masked and altered in some degree by the puerperal state; but it does not necessarily follow that she will have that same disease. Cases have been related by Dr. Collins of Dublin, with reference to epidemics of puerperal fever, where the introduction of a case of typhus fever into the hospital was followed on each occasion by an outbreak of puerperal fever.

So it would seem that a cause distinct from anything which we can identify as the special poison of puerperal fever may yet, by the influence which it exerts upon the woman in this peculiar condition, produce a disease not distinguishable by any of its phenomena from that assemblage of symptoms which we ordinarily call puerperal fever. It seems as if the puerperal state itself was the condition of the development of this set of symptoms. Moreover, we must remember that there is not any one single solitary cause to which we can refer the symptoms of puerperal fever. It occurs now from one cause and now from another. I remember at this moment the case of a lady in a state of perfect health, who was delivered at a time when puerperal fever was prevalent. She was a person in that position of society in which she was shielded from every possible danger. Soon after her delivery, she was exposed to a current of air, and caught cold. In a short time, the symptoms of puerperal fever developed themselves, of which she died. There was a case in which there was no special morbid poison existing—nothing but what could be derived from her own fluids, or brought about by some impression on the system. Moreover, we know that mental shock will have the effect of producing the symptoms of puerperal fever, leading to a fatal issue. I remember the case of a lady who progressed perfectly well after an easy labour; but, owing to some violent scene of domestic strife with her husband, she was seized almost immediately with symptoms of puerperal fever at a time when no such disease was prevalent, and of that disease she died.

I think, then, we are to look for the cause of puerperal fever rather to the condition present after delivery, than to any special poison. Poisons of various kinds may produce it. We know that, in Vienna, puerperal fever was exceedingly prevalent, owing, as was discovered, in a great measure to the fact that the students left the dissecting-room and went with their hands imperfectly clean to attend the women in their confinements; and when greater care was taken, when careful ablution and disinfectants were insisted upon, puerperal fever diminished in its frequency to a very great extent. We know that local violence may produce it: in short, we know that a great number of causes may give rise to it, and that the only one factor in the production of that class of symptoms consists in the special state of the woman after delivery. That state I need not recall. It is a state in which the fluids are laden with old, effete, and useless materials; laden also with materials out of which the old uterus is removed and a new one is to be built up. They are laden also with those elements out of which eventually the new secretion of milk is to be established. None of us except those few who, like Dr. Richardson, have given great attention to these inquiries, can describe at all wherein the alterations of the blood consists; yet, without indulging in any wild theory, we may say that there is a something. People used to call it a ferment. I do not care what it is called, providing we use the name not as defining the thing exactly, but as a name only to which we attach no other meaning than that of a something by which we are to distinguish this or that from the other, while as yet we know not its real nature. For instance, in the time of the old alchemists, calomel and corrosive sublimate, the chemical composition of which was unknown, were respectively called *Draco magnus* and *Draco mitigatus*; and, if we use names as the old alchemists used theirs, we shall be much more likely to keep from the error of hasty conclusions or rash generalisations, than if we use a name to which we tack a definite scientific meaning, while as yet we have no scientific grounds upon which to rest that meaning.

These were the chief things which it occurred to me might perhaps be worth the saying. Take a woman in the puerperal state checked in any manner, whether by poison from without, by mental shock, by cold, or by injury to the regular performance of those functions that ought then to go on, and you have the condition thereby produced out of which the assemblage of symptoms that we call puerperal fever—and all the world is pretty well agreed to give it that name—arises. The only other word I would utter is to quote a saying of a great wise old physician, who said that the highest aim of our art must be the greatest possible generalisation of diseases and the greatest possible individualisation of our patients; so that we have in each case to look, in the prevention of puerperal fever, not simply to the removing of possibility of contagion, not simply most carefully watching the local condition of the uterus and removing from its inner surface any possible decaying matter, not simply to the taking care that the air that surrounds the patient is good, but we are bound also to consider the patient's mental state, to regard minutely her individuality, and, if we wish to put a stop to the disease, to remove from around or from within our patient anything which can interrupt those important processes, the interruption of which gives rise to these symptoms, which we designate by the name of puerperal fever.

CONCEALMENT OF VENEREAL DISEASE IN THE ARMY.

By JOHN AMBROSE, B.A., M.D., Surgeon A.M.D.,
"Attached", 58th Regiment.

THE discussion which followed the reading of Mr. Acton's paper before the Royal Medical and Chirurgical Society, the recent Parliamentary debate, and numerous other signs and tokens, indicate the avidity with which the opponents of the Contagious Diseases Acts would add to their other objections to these statutes that of failure to compass all the good, in the way of disease-prevention, which might reasonably be expected of them. It, therefore, behoves those who are interested in the efficient working of these Acts to leave no means unsuggested of ensuring for them as large an amount of success as it is possible to achieve. At present, our efforts to control the propagation of venereal disease consist chiefly—I might almost say solely—in the care taken to bring under examination and, when necessary, hospital treatment, the prostitutes with whom our soldiers consort in certain military stations. This, obviously, is the step best calculated to strike directly at the root of the evil to be overcome, and the one without which any other measures would be likely to be of little avail. But, at the same time, what I may call subsidiary measures should be taken into account, and I should like to draw attention to one or two such, which I venture to think may be profitably considered.

During the past year and a half, a soldier admitted to hospital with primary syphilis or gonorrhoea (relapses excepted) loses the whole of his pay for the time he is under treatment for either of these diseases. Before considering the influence which this deprivation of pay appears to me to exert upon the amount of venereal disease in the army, I shall mention the other point of which I desire to speak, because the two subjects may be, I think, conveniently discussed conjointly. The other point is this: it was formerly the duty of the regimental medical officers to subject the men of the corps to which they belonged to a periodical inspection for the detection of venereal disease; but for some years this duty has been relinquished. If now we glance at our proceedings with reference to our mode of dealing with these contagious complaints for the last ten years or so, we find that, while, with the one hand, we have imposed certain salutary, if irksome and disagreeable, periodical inspections upon prostitutes, we have with the other hand exempted our soldiers from these very inspections to which, be it remembered, these same prostitutes have been subjected purely and solely for the benefit of the army and navy. Further, in the same decade, a measure has been introduced by which a soldier who confesses himself to be the subject of gonorrhoea or syphilis forfeits the whole of his pay while in hospital. There is now no substantial reason why a soldier suffering from a comparatively trivial, though none the less contagious, form of disease should not conceal and impart it to one or more of the women who, on his account, are submitted to a periodical examination. In this way, some of the good effected at the Lock Hospitals is, I fear, undone; and the drawback is not likely to be favourably affected by the fact, that the soldier frequently prefers avoiding the restraint of a residence in hospital, and devoting some of the pay of which he would be deprived in the event of his being admitted there, to remunerating a neighbouring chemist and druggist for the treatment he requires. The soldier, by so acting, no doubt transgresses the regulations, but he retains his liberty and his money. I say nothing here of the after-effects that irregular, and often insufficient, treatment of this kind must exert upon the man's constitution. The following brief narrative of facts will illustrate the remarks I have made, and prove that I have not been conjuring up imaginary difficulties.

The 58th Regiment came home from India in March of last year, and occupied the Anglesea Barracks at Portsmouth—a "protected" station—until April of this year. The amount of venereal disease in the regiment during 1874 was comparatively small, which gratifying circumstance I, in common with others, accepted as an indication of the satisfactory working of the Contagious Diseases Acts at Portsmouth. This explanation was, I believe, to a large extent, correct, and I do not consider that what I am now about to say in any way invalidates the comparison drawn by Dr. Parkes, in the *BRITISH MEDICAL JOURNAL* of February 20th, between the 58th and 106th Regiments, at Portsmouth and Parkhurst respectively, since the same condition of things which I am now about to describe must have existed in the 106th as in the 58th. When, however, month by month the admissions to hospital, in the 58th, on account of venereal disease, became still fewer and fewer, until, in January and February of this year, they numbered only three; and when, in the last week of February, there was not a case of either gonorrhoea or syphilis under treatment, I could not