some of my colleagues—that there is not sufficient combination among us; that we each know too little what the other is teaching. In the ascending scale of studies which I sketched, each teacher should teach with a view to his successor, and each should be able to refer to what his predecessor has taught. Although I admit my deficiencies, I have for a long time tried to do this. I omit some chapters of physiology almost completely, which, although long and difficult, have but little bearing on medicine, in order to devote myself to those subjects whose understanding is essential in almost every clinical case. And many of you know that, in the hospital, I am never tired of showing how every case, from the simplest to the most severe, must be investigated by reference to the teachings of physiology.

You live, gentlemen, in a transitional period of medical education. You are compelled to take advantage of the modern practical methods of instruction, while you are not as yet emancipated from one of the great abuses of the old system, namely, the excess of lectures you have to attend. When, on this day, twenty-two years ago, I became a medical student, and heard my first lecture in this school, there was no practical instruction given here or elsewhere in Dublin, except practical dissecting-room anatomy. We used to hear the professor of chemistry sometimes speak of the chemical laboratory; but, so far as I know, no medical students ever worked in it. There was not a microscope in the whole school; nor, with the exception of Dr. Bennett, then himself a student, any one in the school who could use one. There were, practically speaking, no museums; and the extent to which anatomy was studied may be judged of by the fact, that the anatomical lectures were given to a scanty audience in the room which is now Dr. Aquila Smith's museum. Yet in those days we found the number of lectures quite enough to leave us little time for independent work. Now, while the number of lectures has increased, you have, in addition, two laborious and long courses of laboratory work to attend. Everyone admits that the present lecture system requires to be completely recast; but the practical question how this is to be done—to what branches of this overgrown tree the pruning-hook is to be laid—is not so easy to settle. Before long, however, I think some change must be made; and, although I cannot tell you what this change will be, I can pretty confidently tell you the direction in which it will tend. It will diminish the number of hours you are compelled to sit on benches listening to lectures, and will increase the number of hours you will be required to spend in using your eyes and fingers and brains in practical work in the dissecting-room, in the laboratories and museums, and in the most important practical department of all—the wards of the hospital. Whenever this change comes, it will be seen that Trinity College is not unprepared for it. Our school is now well provided. It has an unrivalled chemical laboratory, a noble dissecting-room; its pathological and anatomical museums are well housed, well arranged, easily accessible, and stored with ample and constantly increasing material; while this building, whose opening we celebrate to-day, will bear comparison with any similar institute in the kingdom. Of the work which has already been done in the other practical departments of the school it is unnecessary to speak. Here we are only beginners; we are only putting on our armour, and have our spurs yet to win; but we will endeavour not to fall behind our elder comrades. And I hope that throughout all time our three great practical departments—anatomy, chemistry, and physiology—will flourish side by side, and be a credit to the university and to our country.

HEBBURN.—This district has now lost the services of Mr. John Spear, through his appointment to the Local Government Board. The present officer of health, appointed, as it would appear, without being required to devote himself exclusively to public-health work, cannot be expected to rival Mr. Spear's sanitary activity; but it may be hoped that he will follow as closely as possible in the steps of his predecessor, and guide a reluctant authority into wise ways of thinking. Before he quitted office, Mr. Spear presented to the local board a report on the sanitary history of 1879, which records the lowest death-rate for Hebburn on record—20.1 per 1,000. The birth-rate (50.6 per 1,000) is unusually high; but Hebburn has a large youthful population. Doubtless to this high birth-rate must be ascribed in part the very large proportion (59 per cent.) of deaths under five years to the total deaths—a proportion which Mr. Spear records without comment. Zymotic diseases were by no means prevalent last year, the only one seriously fatal being whooping-cough. Twelve deaths were registered from diarrhoea, six in the summer quarter. The deaths from tubercular diseases numbered 34, of which 17, or 50 per cent., were caused by phthisis. Much still needs to be done in the direction of sanitary improvement. The question of the removal of refuse is especially one needing the early attention of the local board.

THE COMMUNICABILITY OF PUERPERAL FEVER BY THE MEDICAL ATTENDANT.*

By ANGUS MACDONALD, M.D.,

Physician and Clinical Lecturer on Diseases of Women in the Royal Infirmary, Edinburgh.

It is difficult to imagine a subject within the whole range of practical medicine, on which it is more necessary that there should, if possible, be general agreement and acquiescence in reliable rules of action among professional men, than that which is the theme of the present paper.

According as correct views are accepted and acted upon by him, depend the peace of mind and comfort of the conscientious obstetrician in that sad hour of trial, which sooner or later falls to the lot of every experienced accoucheur, when it is his misfortune to meet with puerperal fever in his practice. Only by an intelligent and clear apprehension of the problem under consideration can just rules of conduct be established for the guidance of the obstetric practitioner, who is charged with the management of puerperal fever cases, so as to enable him to protect the interests of his patients on the one hand and prevent himself from being unnecessarily blamed or punished on the other.

It is accordingly of the highest importance that only correct and uniform ideas on the communicability of puerperal fever should be current throughout the profession. But it is more than doubtful whether at the present time such is the case. There appears to exist a kind of ill-defined dread of the ailment as a something whose degree of communicability is of terrible intensity, but the principles regulating which are very uncertain, so that the practitioner in a difficulty scarcely knows for certain what to do or not to do; what to avoid or not to avoid. With the exception of the writings of Dr. Matthews Duncan on this subject, the literature of puerperal fever contains few records of any efforts made to determine the limits in practice that may be allowable without risk, or to point out the value of modern antiseptics, when conjoined with strict caution, to deprive this calamity of at least a portion of its terrors.

As uncertainty of action on this head leads to much inconvenience and even danger, it is the duty of the physician-accoucheur, especially as on him centres the burden and the blame associated with an outbreak of puerperal fever, to do what in him lies to acquire and disseminate sound and reliable doctrines upon the subject of its transmissibility. This is all the more necessary, as there exists in the profession an undoubted tendency to press with undue severity upon the obstetrician in this matter, and to surround his path in practice with so many rules and restrictions as to remind one of the commands made by the Egyptian monarch of old to his Hebrew subjects in the matter of bricks.

The restrictions put upon the obstetrician, as a rule, take some such form as that he ought never to see a scarlet fever patient, or, in fact, any fever patient almost. As to erysipelas, he would hardly require to remain in the same district of the county in which a case of the disease is to be found; and if he happens to have a case of puerperal fever in his practice, then, in the interests of his patients, he must leave his practice for a period, varying according to his various advisers from eight days to eight weeks, or even more. It has been otherwise urged; and I have myself listened to a well-known physician, now deceased, when he argued that he believed the poison of puerperal fever entered the fluids of the medical attendant, and was evolved in his perspiration. Accordingly, such a person could not purify himself by any kind of ablation or disinfection, and unless he suspended the practice of his profession for a period sufficient to allow of his secretions removing the whole poison from him, his patients were not safe. Such a view is as grotesque as it is incorrect. I merely refer to it as an aggravated example of the kind of ill-defined ideas that float in the minds of the profession regarding the communicability of this disease.

But in passing we ought to ask whether the rules put upon us by some physicians and surgeons are acted up to by themselves, when placed in similar circumstances.

Suppose a physician is called to see a case of scarlet fever, does he for the next week or a fortnight decline to attend any case of pneumonia or of phthisis, except in the case of patients protected by a previous attack of scarlet fever? Or suppose a surgeon meets in his practice a case of phlegmonous erysipelas or of pyæmia, does he at once announce his determination to relinquish his practice for a week, or even a day, and hand over his amputations and resections, etc., to another? The answer to such questions is, that no one ever dreamed that such was necessary.

But I reply that those who are so fond of imposing hard terms upon

* Read in the Section of Obstetric Medicine at the Annual Meeting of the British Medical Association in Cambridge, August 1880.

the unfortunate obstetrician forget that they are themselves doing with impunity every day the very things they try to prove it wrong for the obstetric physician to do. There is no one, who is acquainted with the misery and misfortune which a single case of puerperal fever brings with it to all concerned, who will not be ready to make any sacrifice, and, to undergo any loss or privation, which is necessary in order to avoid the occurrence of such cases.

But the whole question turns upon what is necessary.

It appears to me that the time is come, when, in the light derived from a better knowledge of what puerperal fever is, and what is the exact nature of the infecting agent, we ought to reconsider our opinions regarding its communicability and endeavour to determine with greater accuracy what is lawful, and what is unlawful, in regard to our intercourse with patients suffering from this disease. I do not mean that we are to restrict the physician and surgeon in the direction of limiting their attendance upon patients. I mean to say that with proper care and cleanliness it ought to be the rarest occurrence possible for a physician or surgeon to be the means of communicating disease from one patient to another. But I mean to claim the same privileges, or rather rights, for the obstetrician in the performance of his duties. A physician, a surgeon, or an obstetrician, who neglects the strictest rules of cleanliness and the necessary precautions against being a carrier of infection, may become a circulating focus of infection any day. But such is not necessary and is, I hold, in every case avoidable.

The uncertainty in regard to what is admissible or inadmissible in regard to the obstetrician's dealings with puerperal fever, seems to me largely attributable to the existence of incorrect views regarding the essential nature of the ailment.

It was long customary, and in some quarters where one would expect clearer views is still so, to regard puerperal fever as a specific fever of very terrible intensity due to the creation of a specific virus, analogous to the poisons believed to give rise to zymotic fevers, but the laws regulating the contagiousness or infectiousness of which are very ill-known. In consequence of this, a state of great uncertainty and of unnecessary alarm has arisen in connection with the spread of the disorder. People have spoken and written of the disease becoming epidemic here and there, and have ascribed its spread to atmospheric and telluric influences, which are as little satisfactory as consoling in dealing with this important subject. The question of its epidemic nature has, it appears to me, been set to rest by means of the various statistical and argumentative contributions of Dr. J. Matthews Duncan to this subject—more particularly by the able statistical paper of that author published in the *Edinburgh Medical Journal*, March 1876.

In the famous, but eminently disappointing, discussion on puerperal fever in the London Obstetrical Society in 1875, it is sufficiently observable that certain of the speakers exhibited a leaning towards the specific fever idea, though the great preponderance of opinion was clearly towards regarding the disease as essentially a wound-fever. But I am anticipating.

The occasional occurrence of the ordinary zymotics in the lying-in woman, when they almost invariably assume specially severe forms, has again led to their being confounded with true puerperal fever. It would serve no good purpose to refer to authors specially, but it is fair enough to assert that in many quarters it has been held that those zymotics might give rise to true childbed fever, when they seized upon the woman in the lying-in period.

I need scarcely remark that I do not believe there is sufficient ground for such a view. Careful observation of such unfortunate coincidences as the occurrence of any of the zymotic fevers in the lying-in woman, will, I believe, satisfy any unprejudiced observers that those specific fevers retain their essential characteristics then as elsewhere. The only real peculiarity presented by them is the liability of some of them at least, more particularly of scarlet fever and of measles, to assume a specially severe character, and to be very fatal. But still, the illness is clearly generic in character, the involution of the uterus, etc., going on in scarlet fever undisturbed, as has been well pointed out by Olshausen and others, and has been confirmed again and again in my own experience.

But, leaving this negative consideration of the subject matter, it may safely be asserted that as a result of careful investigation of the phenomena involved, modern opinion is now very nearly at one in regarding puerperal fever as in all cases a septicæmia.

The essential element in our idea of this hitherto rather vaguely defined ailment is, that it is always due to the absorption through or from a wounded surface of some septic material by which the blood becomes poisoned. It is therefore in all cases a true septic intoxication—a fever of resorption—and differs essentially in no wise from surgical fever. I say essentially, because in regard to its entire phenomena puerperal septicæmia is extremely varied in intensity, and protean in aspect. But

these peculiarities are undoubtedly due to the special anatomical and physiological conditions of the lying-in woman. Nor need we wonder at this, when we examine the very numerous causes that are present in every lying-in woman, calculated to facilitate the absorption of putrid matters into the patient's blood, and to favour its reproduction within her system.

I need only refer, in passing, to one or two of these. Thus the numerous fresh wounds in the genital tract, especially at the perinæum and the cervix, the imperfect condition of the mucous membrane of the body of the uterus, which is such as to reduce a considerable proportion of its entire area to the state of a freshly wounded surface, whilst the rest of it is covered over with a very rudimentary barrier of epithelium, not to speak of the large gaping sinuses in the placental area, combine to give too ready access to any putrid substance.

Then, without predicting any distinct imperfection in the blood of the pregnant woman, the metamorphic changes that commence with delivery, and are in full action during the lying-in period, are calculated to task the full energies of the secreting organs in health; and, if the action of the latter chance to be lowered in any way, even though temporarily, the blood is certain to become rapidly impaired and thereby to prove itself specially susceptible to the operation of septic influences.

When to these considerations is added the possible presence of putrescent materials, such as portions of membrane decomposing, or of placenta, or of retained clots, as also the natural lochia, which is so apt to become putrid, we need not wonder that any septic element should find ready access to the blood of the puerperal woman, and, when there, develop symptoms of very great intensity.

While the view that puerperal fever is always a septicæmia, and that only, presents us with a simple conception of the disorder, sufficient cause for its very varied degrees and intensity is afforded by a consideration of the various circumstances modifying the entrance and action of the septic matter.

Thus the virus introduced is often found to vary in degree as well as in intensity at the time of its introduction. The effects on the general organism and on special organs are found to vary according to the channel by which it enters—venous or lymphatic—as also by the rate at which it travels. The poison, when reproduced within the system, has been proved to become vastly more intense. Various patients present different degrees of resistance to the action of any morbid agent, and this is true also of puerperal patients. Consequently we have endless varieties of the disorder. Many cases, I believe, are not diagnosed with certainty, even by the most careful attendants. A considerable number of such cases are not even suspected of belonging to the proper category. For we know that acute cases die before time is afforded for the production of the grosser changes of pathological anatomy; and also that even in subacute cases the pelvic and abdominal organs may be left intact, the poison passing along the large veins without involving these parts, whilst death results from septicæmia acting on distant organs, such as the lungs.

But while the conception of puerperal fever as a septic wound fever supplies us with a view of the disease which is at once simple and comprehensive, there has been great advance in the same direction made within the last decade to determine the nature of the *materies morbi*, and to determine the means that can be adopted to prevent its spread from patient to patient.

The observations of Mayrhofer, Orth, Heiberg, Haussmann, Spillmann, Pasteur, Doléris,* and others, have within the last ten years accumulated such an amount of evidence, that it seems to me next to impossible to refuse credence to the belief that the septic changes which take place in the lying-in woman are dependent upon the action of certain micrococci, which can always be detected in the fluids of the dead, and usually also in the blood of the living who are affected with puerperal septicæmia.

At any rate it seems proved that these minute organisms are an inseparable accompaniment of septic inflammation, and that, whether they cause the destructive changes themselves, or merely, as Burdon Sanderson† believes, give rise to the septic material which directly effects the destructive alteration in the blood, if we could destroy these organisms, we should destroy the self-formative power of the septic matter that is introduced into the patient's system.

Thus, if we regard the micrococci as causing the disease directly, to destroy the micrococci is to prevent the disease. If we regard the micrococci as producing the septic fluid, this septic fluid is not capable of multiplying itself without living micrococci, and there could be no great danger to our patients from the introduction of a small portion of this product, as the resisting powers of the organism could not fail to

* *La Fièvre Puerpérale et les Organismes Inférieurs*, p. 45, et seq., Paris, 1880.

† BRITISH MEDICAL JOURNAL, December 22nd and 29th, 1877.

rapidly destroy it. The condition that makes a small amount of the septic material dangerous is, clearly, because the living germs of the evil are introduced and proceed to multiply within the organism.

From that point of view, a single micrococcus may be sufficient to induce a case of puerperal fever, as, finding an appropriate cultivation ground in the patient's organism, the single bacterium very quickly becomes millions.

But important advance has meanwhile been made in the collateral regions of pure surgery, which have the most important bearings on this subject. The investigations of Lister in this country, of Koch,* etc., in Germany, the experiments of Dr. Alexander Ogston† of Aberdeen, have combined to advance our knowledge a very long way in this department, and have given us a footing on which we can with a considerable degree of assurance rest our therapeutic or rather prophylactic action in dealing with puerperal septicæmia. Dr. Ogston, in a series of beautiful experiments, appears to have demonstrated conclusively that acute suppuration is always associated with the production of micrococci in the pus; that the severity of the general symptoms, and of the suppuration, vary with the number of these minute organisms, which, in severe cases, are found freely circulating in the blood; and also, that by the addition of a five per cent. solution of carbolic acid to the most putrid and virulent pus, its activity is entirely destroyed, and, when injected into the living tissues, it is without any irritating influence whatever.

According to Dr. Amédée Doléris the power for evil is taken away from septic micrococci by the addition of an equal part of a one per cent. solution of carbolic acid to a solution in which they are found.

It would thus appear that a solution of carbolic acid, which will not injure the hands of the physician, is sufficient to destroy the minute organisms on which, according to the modern idea, the poison of puerperal septicæmia depends for its existence and activity.

I therefore believe that we have now arrived at the time when we are able to state that, with the employment of extreme care and cleanliness, coupled with the use of proper antiseptic precautions, we may, without danger, attend patients suffering from puerperal fever, and do all that is required of us as doctors, without the slightest risk of communicating the disease to our other patients.

But a distinction ought to be made regarding what is the duty of a doctor as compared with what is incumbent upon a nurse. A medical man, to avoid carrying the infection, must not stoop to perform the work of a nurse. He must be careful not to expose his clothes to any discharge that comes from the patient's genitals, or that is about the bed—or indeed to allow them to touch the bedclothes.

I have seen medical men lifting puerperal patients by pushing the coated arm below their legs, and then carefully disinfecting their own hands. Such conduct can only end in disappointment. Before such an act were admissible the coat should be removed, and only the bare arm used; after which, the arms and hands should be thoroughly disinfected.

In proof of the practicability of what I maintain, I may mention an experience of my own in the spring of 1879. It was then my misfortune to meet with a case of puerperal fever in my practice. The social position of the patient, and certain other considerations, rendered it necessary that I should do more than is usually required of the medical attendant. The case was a well-marked one of septicæmia, and ultimately terminated fatally. I watched the case very closely for ten or eleven days; and twice daily, with my own hands, washed out the vagina with a disinfectant solution, and dressed a vulvar ulcer which had formed. Being at the time on duty at the Royal Maternity Hospital, and satisfied in my own mind that my disinfectant applications were sufficient to prevent any harm to my patients, I continued my services there. It so happened, that during these ten days there were several specially interesting cases treated in the maternity. Accordingly, I had to perform craniotomy on an out-patient for obstructed labour. The operation was tolerably difficult, and took up considerable time. The patient never had a bad symptom, and made an excellent and rapid recovery. I also had occasion to employ forceps in a case in the hospital in which the head was arrested high up. That patient also presented no febrile symptoms, and recovered uninterruptedly. I likewise performed version in another case, in which the head and arm presented. This patient, like the rest, did well. In consultation I saw and examined a patient suffering from hæmorrhage after abortion. In this case I passed my finger into the interior of the body of the uterus to make certain that no portion of membranes had remained behind. No bad symptom of any kind followed this manoeuvre.

* *Untersuchungen über die Ätiologie der Wundinfektionskrankheiten*, Leipzig, 1878.

† *Über Abscesse*. Separat-Abdruck aus von Langenbeck's *Archiv*, Bd. xxv, Heft 3.

Had the remotest bad symptom appeared in the first case, I certainly should have at once desisted. But the success in it emboldened me more and more to trust to the disinfectant measures I had adopted. Besides, I had before me the experience of Dr. Thos. Keith, in his statement that with due attention to antiseptics he felt at liberty to perform an ovariectomy operation, although half an hour previously he had had his hands in the filthiest mess possible.

When called to treat such cases, however, it is my invariable rule to attend to the strictest antiseptic measures. If I have to examine or lift a patient suffering from puerperal septicæmia, I always take off my coat and roll up my shirt-sleeves. After doing this, I wash my hands in turpentine or rub them with carbolic oil. After examining the patient I again wash my hands, and, if need be, arms, in turpentine and soap and water, using a nail-brush freely. Then I wash my hands in a five per cent. solution of carbolic acid, and finally pour a stream of running water over them from a tap. Considerable importance appears to me to be attached to the latter proceeding, as the running stream makes it certain that everything is carried away as well as washed off the hands. If a basin be employed, the hands are brought from time to time into contact with any septic matter that might remain undestroyed in the basin.

In common with every consultant obstetrician, I am from time to time summoned to see and examine cases of puerperal septicæmia with my professional brethren. I am in the habit of doing as I have indicated in such cases, and I am satisfied that thereby no harm has resulted to any of my own patients. Nothing could be further from my intention in this contribution than to inculcate carelessness or do anything which could bring danger to patients or disgrace to obstetricians. But on the other hand, I am anxious that everything should be done for unfortunate patients suffering from puerperal septicæmia, which is consistent with fairness to the unaffected and to the obstetrician in charge.

I am further certain that the rules as to abstinence from seeing infectious cases, and as to suspension from professional duty so loudly preached by many heads of the profession, are not acted upon by them. Indeed, I believe, they are neither necessary, nor would they be effective for the purpose indicated. I agree entirely in the object, I disagree with the proposed means.

For aimless and haphazard suspension from professional duty, I would substitute the most thorough cleanliness and disinfection, believing that in the latter means the real safety of the patient lies. I have published my experience in this matter, in the hope that it may encourage others to trust to and practise disinfectant appliances in similar emergencies.

I must here enter my protest against the senseless and heartless proposal frequently made to the obstetrician to flee from such cases. He cannot do so if he would. A better object, surely, is to so regulate one's conduct as to enable him to attend and care for such sufferers; and at the same time do what is right for his other patients.

The exaggerated idea as to the communicability of puerperal septicæmia, or rather as to the great difficulty of getting rid of the infecting element, when once it gets attached to one, leads, I am persuaded, frequently to the spread of the disease.

The terrible consequences to a practitioner of having a case of septicæmia in his practice make him very unwilling to allow, even to himself, that a doubtful case belongs to that category. In that way, I believe, many cases are treated by the medical attendant, under the idea that he has to deal with simple metritis, parametritis, or other non-infecting ailment. In many cases, no harm results. In others, it may lead to the spread of the disorder to other patients.

Where the practitioner is convinced that, with due attention to cleanliness and antiseptics, no harm would result, he would not be so unwilling to recognise a case as septicæmia and take the necessary precautions to prevent communicating it to any other patient.

The principle of thorough, rapid, and complete disinfection ought also to be practised by nurses and midwives. If such measures were intelligently adopted, we should find less need to place nurses on a lengthened period of probation after attending a case of septicæmia than we do at present.

Their case is no doubt different from that of the medical attendant, if the latter restrict himself to his legitimate duty of superintending the nursing only. The intercourse of the nurse, and even of the professional midwife, with the patient, is more constant and more close than that of the doctor. But this fact only implies that the nurse and midwife should exercise greater care and thoroughness in disinfectant applications after attending a case of septicæmia. The disinfection should include both the clothes and person of the nurse. But, provided such measures are adopted, there appears no good ground for suspending a nurse for a series of weeks or months. Indeed, in the mere suspension there appears to be no real safety, as, unless measures of dis-

infection are duly adopted, it is impossible to say how long the person and clothes of a nurse may remain a source of danger. A very valuable contribution on this subject is made by Ahlfeld in the *Centralblatt für Gynäkologie*, 31st May, 1880. In this article the author protests against the State regulation adopted in Saxony, which compels a midwife, when a case of puerperal fever occurs in her practice, to cease from it for a stated period, instead of insisting upon rapid and thorough disinfection.

Nurses and midwives deserve great consideration at the hands of the obstetrical branch of our profession. It is our duty to provide them with sound instruction in regard to the use and application of disinfectants and antiseptics. By our so doing, not only will they as a class be greatly benefited, but lying-in women will be placed in much greater safety than by unjustly punishing unfortunate midwives.

It is to be regretted that the mover of the famous discussion on puerperal fever in the London Obstetrical Society in 1875, should, in his summing-up speech, have met the protest against the special act of injustice practised in the name of law upon midwives at that time, by something very like a sneer. It would be much more to the purpose to meet the question boldly as Ahlfeld does, and demonstrate to nurses that time is only a very uncertain element in disinfection, and that cleanliness and antiseptic applications are the only measures by which safety can be secured.

In regard to the prevention of puerperal septicæmia in maternity hospitals by strict application of antiseptics, and more particularly of carbolic acid, I may be allowed to refer to the successful practice of Dr. Lucas-Championnière in the Cochin Hospital of Paris, as recorded by Dr. Amédée Doléris, p. 294, *et seq.*, in his work entitled *La Fièvre Puerpérale*, Paris, 1880. There seems to me no good reason to doubt that with proper care as to hygiene and antiseptics, patients treated at a maternity hospital ought to be as free from septicæmia as those attended in the best private houses.

In conclusion, I would wish to express my belief: 1. That the diligent and intelligent employment of antiseptic precautions and appliances, good ventilation, and extreme cleanliness are capable of very largely diminishing the occurrence of septicæmia, both in maternity hospitals and in private practice; and 2. That, if antiseptics are carefully and systematically employed, there should be no case of communication of the disorder from one patient to another by the medical attendant, even when he performs to the sick person all the duties that are incumbent upon him as a medical adviser. It is always, however, to be understood that the doctor restricts himself to his own duties, and does not encroach upon those of the nurse; and that his measures to secure perfect antiseptics shall be thorough in all cases, when there is the slightest suspicion of septicæmia. It follows also, if these views are correct, that the recommendation so frequently given to an obstetrician, to leave his practice in case he meets with puerperal fever, is both unnecessary and unsatisfactory, inasmuch as it tends to the neglect of the most reliable measures of safety, namely, constant attention to cleanliness, and thorough and complete disinfection; whilst it puts upon the obstetrician a burden that is too heavy for him to bear, and which, indeed, is not borne by the very people who are loudest in recommending its necessity.

ON TOBACCO-AMBLYOPIA.*

By JOSEPH NELSON, M.D.

THAT cases of tobacco-amblyopia are not of unfrequent occurrence in Austria, is evident from statistics collected by Drs. Reuss and Fuchs; for, according to them, no fewer than 153 cases, all men, are recorded from Dr. Arit's *Klinik*, during the eleven years ending 1876. During the past twenty months, I have had the opportunity of seeing nearly all the cases which presented themselves at this *Klinik*; and, through the kindness of Professor Arit, I was allowed to investigate and have the charge of those cases which were able and willing to undergo treatment. Unfortunately for observation, however, many of these people lived far from Vienna. Of those who did remain in town, twenty-six afforded me material for the following notes.

Inasmuch as the bulk of my notes only confirm what has already been written on the subject by Leber, Hutchinson, Nettleship, and other authors, I shall confine myself as much as possible to a statistical *résumé* of my investigations. The patients were all men in good health, and between forty and sixty-six years of age, except three. The youngest was twenty-seven years old.

Failure of sight was, in most cases, about one-tenth, never so reduced

that the patient could not go about readily. In the majority, sight failed in from two to six months; in a few, from two to six weeks. The duration of failure bore no constant relation to the degree of amblyopia, or to perception of colours. They saw better in the evening, or in subdued light. During daylight, especially bright sunlight, everything appeared fogged or clouded. All smoked; the smallest amount of tobacco consumed during the day being over half an ounce, the largest quantity an ounce and a half. In only four instances were patients free drinkers, in addition to the abuse of tobacco. I may remark that these four cases were the longest in seeking advice; or that their sight failed perhaps more gradually than in other cases; thus bringing to my mind Hutchinson's suggestion, that alcohol may have a balancing or counteracting effect upon tobacco (*Royal London Ophthalmic Hospital Reports*, vol. viii, p. 458). I should state that these people drank wine and spirits, not beer.

Failure of sight was generally equal in both eyes; there was never any marked difference that could not be accounted for by refraction, opacity, or other evident cause.

On testing with Holmgren's wools or colours of large surface, the colour-sense was, as a rule, normal; but, on testing with small coloured discs of six millimètres or less, on a black background, like Donders's discs, more or less impairment of colour-sense was present in the majority of cases. For the different colours, especially red and green, the perimeter almost invariably revealed a scotoma between the blind spot and the fixation-point, and embracing both. In all cases in which I tested, I found much the same shape and size of scotoma in both eyes. The peripheral portion of the field for colours was normal, and the field for white neither contracted or concentrated; thus differing from other forms of amblyopia—ataxic, cerebral, hysterical, glaucomatous, neuritic, and hereditary. The scotomata were negative, and differed much in intensity. In some, the square (of ten millimètres) would change colour or become indistinct, whilst in others the colour would be completely lost.

We have here a number of central scotomata, embracing both the fixation point and the blind spot; and I would ask, How do these scotomata begin? There are two possibilities: 1. They may begin at the fixation-point, and extend outwards to the blind spot; or, 2. They may begin at the blind spot, and extend towards the fixation point. The former, according to authors (Leber, Nettleship, Treitel) is that generally accepted; but, from the cases which came under my observation, I would hold that the scotoma begins at the blind spot, and extends towards the fixation point. The points upon which I rest are five: 1. The gradual failure of sight; 2. The shape of the scotoma; 3. The density; 4. The manner of receding; 5. The fact of seeing cases where fixation-point is free.

1. If the scotoma began at the fixation-point, the central vision would probably become at once so bad as to oblige the patient to seek medical advice, and we would then very often find a scotoma which had not yet reached the blind spot. But, as the scotoma sets in at the disc, it will at first not much interfere with sight, and the patient will only seek aid when the scotoma has extended to and embraced the macula. For this reason, we always find the scotoma so large, as to include both fixation-point and blind spot.

2. The scotoma was often of an irregular pear-shape; but, of whatever shape, the base or broad part was always at the blind spot, whilst the apex or narrow portion was at the fixation-point.

3. The scotoma was invariably denser at and towards the blind spot than at the fixation-point; in fact, the density increased from the fixation-point towards the blind spot.

4. It is a matter of fact that a scotoma may disappear, especially under judicious treatment, such as leaving off smoking, rest of the eyes, etc. I have myself had four cases, all well-marked scotomata, in which they completely disappeared. As regards the manner of receding or disappearing, I would direct attention to three cases where second and even third observations were made, and the changes—the decrease in size, position, shape, and density—are noted. These changes are, I think, very suggestive; so much so, that I am inclined to think that, if we could only examine a tobacco-case at an early stage, we should find a young scotoma growing from the blind spot—its base—and extending towards the fixation-point, but stopping short of it. The opportunity of examining such a case is, however, very unlikely to occur, inasmuch as the patient will not complain until the macula is reached, and sight interfered with.

5. One of the perimeter charts shows a scotoma of the usual shape and position, except that it does not reach the fixation-point. This was in a case of six months' failure, and was probably a receding scotoma; at any rate, it is a case where the fixation-point is free.

The density of the scotoma does not seem to hold any proportion to acuity of vision, for, in Case XIV, with dense scotoma embracing the

* Read in the Section of Ophthalmology at the Annual Meeting of the British Medical Association in Cambridge, August 1880.