delicacy. It is far from rare to meet with such peculiarities as I allude to. They may be due to circumstances quite independent of the existing malady; or they may be allied with it, but without possessing any intrinsic value in determining the diagnosis in the case. They constitute for the most part the category of what I may term the "remarkable features" in the early cases, and especially the operations, of young surgeons,—features which cease to be remarkable, and which are estimated at their true value as experience ripens. Yet, on the other hand, some of these accidental or occasional concomitants are fraught with a deep and peculiar meaning to those who know how to read them aright; they are as the floating feather, showing the course of the current of air or of the silent stream.

The coexistence of various signs or symptoms suggests the consideration whether they are consistent with each other, and how far they are diagnostic of the injury or disease which is suspected. In this investigation it becomes the duty of the surgeon to unravel the tangled web, and to ascertain the simplicity or complexity of the case before him. The concurrence of dislocation, with fracture near to a joint, may give rise to such a complication; and the diagnosis of such injury is thereby rendered more obscure. Many opportunities will be afforded me of exemplifying this rife source of difficulty in the diagnosis of visceral lesions. These obscurities are sometimes intensified by the more urgent symptoms assuming such a predominance as, in part or entirely, to mask those of less prominence, though not of inferior importance in arriving at a correct conclusion. The sensations of the patient,—the pain he suffers, or the swelling of an injured part are rife sources of obscurity in such circumstances, and necessitate delay in acting, on account of the difficulties they entail. Or, again, the surgeon may be misled by deception on the part of the patient; and this source of embarrassment, as I have already observed, is not always wilful. His ignorance of the relative value of this or that indication may induce him to give prominence to one symptom, whilst he conceals another; or he may have some unrevealed motive for misguiding his medical attendant. Hysteria often perverts symptoms, by the substitution of those which are its peculiar atrributes; or the acute or chronic effects of intoxicating drink may entirely alter the phase of indications, which would otherwise stand out in bold relief.

Another source of obscurity is the functional derangement consequent on the sympathy of different organs; such, for example, as arrested or excessive secretion, irritability of the stomach or bowels, obstinate constipation, dyspnœa, convulsion, paralysis of the bladder, and a host of other sympathies, some familiar in their interpretation, others unintelligible to us. The influence and effects of prior treatment also often call for the scrutinising attention of the careful practitioner; for in them we find an explanation of modified or qualified symptoms, which would be otherwise a prolific source of fallacy in diagnosis; and which sometimes suggest the wish that the case had been transmitted to us without this additional source of obscurity. The earlier treatment of strangulated hernia, of retention of urine, of misunderstood joint injuries, present us with many and painful proofs of the importance of investigating all that the patient has had done for him by those who have had the earlier management of the case.

These are the chief sources of fallacy or misdirection which occur to me to notice in connection with surgical diagnosis. It would not be difficult to extend this list, or to enlarge upon its details, by further illustrative commentaries on the different subjects to which I have alluded; but this would be inconsistent with the necessarily condensed style of these lectures; and the details of the outline I have sketched may be readily filled in. I shall next proceed to make a few remarks on certain symptoms and signs which have a more special import and significance in surgical diagnosis.

(*To be concluded.*)

## THE PATHOLOGY OF SKIN-DISEASE.

THOSE interested in the cryptogamic diseases of the skin may be interested to know that arrangements have been made for Mr. Tuffen West, F.L.S., to deliver two lectures, in connection with Mr. Hutchinson's course, on Parallel Diseases in Plants. The lectures will be given in the Library of the Hospital for Skin-Diseases, New Bridge Street, Blackfriars, on Friday, July 17th, and Saturday, July 18th (this week), at 8.30 in the evening. Mr. Hutchinson also wishes us to state he has arranged for the attendance of several patients suffering from the curious malady known as Morphœa (Addison's Keloid), on Tuesday next, at half-past three. He will be glad to submit them to the examination of any one interested in the subject. They are supposed to illustrate the important proposition, that the disease has its primary seat in the nervous system, and not in the skin.

# AN ADDRESS

ON THE

# ANTISEPTIC SYSTEM OF TREATMENT IN SURGERY.*

*Delivered before the Medico-Chirurgical Society of Glasgow.*

BY JOSEPH LISTER, F.R.S.,
Professor of Surgery in the University of Glasgow, etc.

MR. PRESIDENT AND GENTLEMEN,—In order that the antiseptic system of treatment may confer upon mankind all the benefits of which it is capable, three things appear to be indispensably requisite. First, that every surgeon should be convinced of the reality and greatness of those benefits, so that he may be induced to devote to the antiseptic dressing of a case the same kind of thought and pains as he now, if at all worthy of the name of surgeon, bestows upon the planning and execution of an operation; secondly, that these efforts on his part should be directed on sound principles; and thirdly, that, for carrying out these principles, he should have thoroughly trustworthy practical means at his disposal. I venture to hope that the illustrations which I propose to bring before you this evening may promote in some degree each of these essentials.

In speaking of the antiseptic system of treatment, I refer to the systematic employment of some antiseptic substance, so as entirely to prevent the occurrence of putrefaction in the part concerned, as distinguished from the mere use of such an agent as a dressing. The latter has long been practised in many parts of the world. The former originated rather more than three years ago in this city (Glasgow). The material which I have generally used for the purpose is carbolic (or phenic) acid, which, when I first published on the subject, was new to most British surgeons as an external therapeutic agent. This circumstance, while it had the effect of attracting greater notice to the matter than might otherwise have been the case, was perhaps on the whole a misfortune, since it tended to distract attention from the essential principles of the treatment which I advocated, and to lead many in this country to look upon carbolic acid in the light of a specific. On the other hand, continental surgeons visiting our Infirmary, familiar with the use of carbolic acid as an ordinary antiseptic dressing, have invariably formed a just estimate of the advantages derived from its employment upon the system to which I have alluded.

So far from carbolic acid being a specific, it owes its virtues to properties which it possesses in common with various other substances; and results similar in kind to those obtained by its means might be got by disinfectants long familiar to British surgery, provided always that the same principles guided their employment. This statement is not made on theoretical grounds alone. About nine months after I had first treated compound fracture with carbolic acid, Mr. Campbell De Morgan published a paper "On the Use of Chloride of Zinc in Surgical Operations and Injuries," and was kind enough to send me a copy of it. By means of this salt he had obtained highly satisfactory results, though led to employ it with a very different object in view. Mr. De Morgan used chloride of zinc in the first instance in cases of cancer, upon the idea that the frequency of return of the disease after operation might depend on the dissemination of its germs on the cut surface, and he hoped that, by applying a strong solution of the chloride to the wound so as to destroy any cancer-germs that might be scattered over it, he might diminish the chance of recurrence. Having treated cases of cancer in this way, he found that the wounds healed unusually kindly, while there was, at the same time, an absence of "animal odour," and he expressed his surprise at the small amount of "action" in the part. To myself it appeared perfectly natural that, if chloride of zinc prevented animal odour, implying that putrefaction was avoided, the wound, protected from the irritating influence of the products of decomposition, should exhibit little inflammatory disturbance. But it struck me as very remarkable that a single application of chloride of zinc to the raw surface should

* This address does not profess to give a complete account of the antiseptic system, but was based upon some illustrations which happened to be at my disposal. One of these, an experiment in support of the germ-theory of putrefaction, was dwelt upon at considerable length, in accordance with what I believe to be the great practical importance of the subject to which it refers.

have the effect of preventing all odour of putrefaction for days afterwards; for I knew that in the case of carbolic acid a renewal of the antiseptic to the exterior was essential in order to prevent decomposition. Hence it appeared likely that chloride of zinc would answer better for my purpose than carbolic acid, and I determined, on the first suitable occasion, to give it a trial. It was not long before an opportunity presented itself.

*Case of Compound Fracture Treated with Chloride of Zinc.*—A labourer was admitted into the Infirmary with compound depressed fracture of the *os frontis*, caused by violent impact of the handle of a winch. I applied chloride of zinc solution thoroughly to the interior of the wound, and, with the view of preventing the spreading of decomposition inwards, adapted to the forehead a plate of clean block tin overlapping the sound skin for a considerable distance, a means which, as I had before ascertained, prevents the occurrence of putrefaction in the discharge from a healthy granulating sore. The tin plate was kept in position by a piece of strapping, and over all was laid a damp folded rag to absorb discharge, with directions that it should be frequently changed. The tin was not disturbed till about a week had expired, the patient meanwhile remaining free from any unfavourable symptoms, and not a drop of pus appearing. On removing the plate of metal, I found that the wound, instead of being hollow as when I had last seen it, was on a level with the surface of the forehead, being occupied by a chocolate-coloured mass which I supposed to be a clot, altered by the action of chloride of zinc. But when I scraped the surface of this material it bled, shewing that it was in reality alive and vascular. This exactly corresponded to the most striking and peculiar of the results I had seen to follow the use of carbolic acid in compound fracture (see the *Lancet*, March 16th, 1867), and the most likely to be mistaken for the effect of a specific action of that substance, viz., that the blood acted on by the antiseptic, though greatly altered by that action, remained susceptible of organisation. Or, speaking more strictly, the product of the action of chloride of zinc upon the blood, like that of the operation of carbolic acid upon it, so soon as the irritating antiseptic material with which it was at first imbued had been withdrawn from it by diffusion into the surrounding circulation, proved a suitable pabulum for the growing elements of living tissue in the vicinity, which accordingly absorbed and appropriated it.

In the case just related, nothing could be more satisfactory than the effects of chloride of zinc. Subsequent trials, however, proved it to be very inferior to carbolic acid except in one class of cases, those, viz., in which, from the circumstances of the part concerned, it is impossible to maintain an efficient external antiseptic dressing, so that the application must be made once for all at the time of the operation. Here the permanence of the effects of chloride of zinc renders it highly valuable, as, for example, after the removal of portions of the maxillary bones. Every surgeon is familiar with the highly offensive character of the discharge for the first few days after such operations; and there can be no doubt that the fœtid state of the wound, besides being a great inconvenience to the patient and his attendants, involves a certain amount of danger. By means of chloride of zinc this complication is nearly, if not entirely avoided. In the first case of this kind in which I used it, I had to remove a considerable portion of both superior maxillary bones, on account of epithelial cancer which had spread to them from the face. I applied the chloride of zinc solution freely to the raw surfaces at the time of the operation, and afterwards examined the breath daily, when the only smell perceptible from first to last was an occasional odour of tobacco. [Since this address was delivered, I have used chloride of zinc with great advantage after the removal of the tongue by Mr. Syme's method, in a case of epithelial cancer.]

But in ordinary cases carbolic acid is very superior to chloride of zinc, and, so far as I am able to judge, to any other antiseptic agent with which we are at present acquainted. It presents, indeed, a remarkable combination of advantages. In the first place, it possesses the essential requisite of being a most potent poison for the low forms of life which determine putrefaction, and it retains this power even though diluted to such a degree as to be almost entirely unirritating to the tissues of the human body. In the second place, it is volatile, and its vapour is quite efficacious as an antiseptic. This gives it a great advantage over chloride of zinc or any other non-volatile substance, enabling the dressings impregnated with the acid to exert their influence not only upon objects in actual contact with them, but also upon the air in their vicinity. Again, carbolic acid is a local anæsthetic, and exercises a most soothing influence upon a painful wound. Lastly, carbolic acid is soluble in a variety of liquids of very different properties, so different, for example, as water and the fixed oils; and each of these solutions has its own special value in practice, a point to which I shall have occasion to allude further on in this communication.

And now, before speaking of some cases treated with carbolic acid on the antiseptic system, I wish to direct your attention to an experiment illustrating the germ-theory of putrefaction. It is on this theory that the antiseptic system of treatment is based; and I venture to say that, without a belief in the truth of that theory, no man can be thoroughly successful in the treatment. If any one believe that putrefaction, through atmospheric influence, is due to the operation of the atmospheric gases alone upon the putrescible materials, he will be perpetually meeting with the most perplexing anomalies, and will be liable to commit the most serious practical blunders; the truth being that, on the one hand, the complete exclusion of the gases of the air affords no security against the occurrence of putrefaction, and that, on the other hand, the freest admixture of air with the putrescible contents of a wound or abscess will fail to induce putrefactive changes, if the germs of that air have been removed by filtration or deprived of vitality by a germ-poison. Of this I might, if time permitted, give several very striking illustrations from practical surgery.

The experiment which I wish to bring under your notice is a modification of one described by Pasteur (see *Comptes Rendus*, vol. l, p. 306), not, indeed, as originated by himself, but by M. Chevreul. It is so simple, and, at the same time, so conclusive, that it should, I think, if believed, carry conviction to the minds of all. To myself the statement of Pasteur, confirmed as it is by the report of the Commission of the French Academy, before whom this, as well as various others of his experiments was performed, was perfectly satisfactory. But there was one reason that made me anxious to repeat the experiment as bearing upon the antiseptic system of treatment; and this was that, so far as I read Pasteur's papers, he had performed it only with reference to the fermentation of a saccharine solution, and I wished to make sure that it applied equally to putrefaction. The experiment was performed in the following manner.

*Experiment in support of the Germ-Theory of Putrefaction.*—On the 26th of October, just half a year ago, I introduced portions of the same specimen of fresh urine into four flasks, of which these are two. [The flasks, which were capable of containing about six fluid ounces each, were about one-third filled.] After washing the urine from their necks, which were then wide and straight, I drew out the necks by means of a spirit-lamp into tubes about a line in diameter, and in three of the flasks bent these elongated and attenuated necks at various acute angles, as you will see in one of the two before you. In the remaining flask, the neck was cut short and left vertical in position as you see it here, but its orifice was reduced to even smaller calibre than in the others. Each flask was then boiled over the lamp, and the fluid maintained in a state of ebullition for five minutes, the steam issuing freely from the orifice. The lamp was then withdrawn, and atmospheric air was permitted to rush into the flask to supply the place of the condensed steam. The flasks were then left undisturbed in the same room, the ends of their necks being still open so as to permit free exit and entrance of air as a consequence of the diurnal changes of temperature which, of course, involved alternate expansions and condensations of the contained gases. Sometimes on a cold night I have raised the temperature of the apartment considerably, and then putting the fire out, have thrown open the windows so as to occasion a depression of temperature of 20 degrees, involving the entrance of about a cubic inch of fresh air into the body of each flask. But, independently of any such exceptional treatment, a perpetual daily interchange took place between the air inside the flasks and that of the room in which they stood. And what has been the result of the action of the air upon the urine? In the flask with straight and short, though narrow neck, I observed after ten days a minute filamentous object at the bottom of the glass. It grew larger from day to day, and was evidently a kind of minute vegetation; and on applying a pocket magnifier, it was seen to consist of delicate branching threads. Four days after this growth first appeared, I observed an object floating on the surface of the liquid, evidently also a minute fungus; but this in the course of a few days clearly shewed itself to be of a different kind, consisting of straight radiating filaments much more closely packed, while to the naked eye its appearance was much denser than the other, which was beautifully feathery and delicate, and its colour bluish-grey instead of perfectly colourless like the first. The two differed also remarkably in their rate of growth, that at the bottom of the vessel springing up with rapidity, so that a month after the commencement of the experiment it occupied about half the mass of the fluid, while the floating kind, though it had been steadily enlarging, had attained only about the size of a pea. Meanwhile the urine had been undergoing a change in chemical constitution, as was indicated by an alteration of its colour from a pale straw to a deep amber tint. But in the meantime, what was the condition of the urine in the three other flasks, with bent necks, of which this is a sample? You observe it is perfectly clear and bright, free from cloud, scum, or sediment, and it retains its original straw colour, contrasting strikingly with the amber tint of the other. In short, it has precisely the same appearance as it had at the outset. I may add that, on the day after these flasks were prepared, having another similar one

at my disposal, I introduced into it some fresh urine from the same source, drew out the neck and bent it into angular form, and treated it like the others, so that I have thus four flasks of boiled urine communicating with the air though bent tubes; and in all of these the urine has remained with unchanged colour and undiminished transparency. It can hardly be doubted that this completely unaltered appearance of the fluid is associated with absence of putrefaction. I shall take an early opportunity of ascertaining whether such is the fact or not; but in the meantime, suppose we assume that it is so. [Since the delivery of this address, viz., on the 2nd inst. (May 1868), I poured out about half an ounce of urine from one of the flasks with bent neck into a wine-glass, and examined it. Its odour was perfectly sweet, and its reaction faintly acid to litmus paper, while under the microscope it shewed not the slightest appearance of anything possessing vitality. I then covered the wine-glass with a piece of sheet gutta-percha to prevent evaporation, and left it at a temperature of about 70 degrees. Three days later it had already lost its brilliant transparency, and a distinct change had occurred in its odour, which had assumed something of the smell that urine has when evaporated to dryness. And under the microscope, organic forms of different kinds were present in abundance, some of them motionless delicate elongated rods (bacteria?), others with wriggling movements, apparently of vibrionic nature, while there were also numerous amorphous and faint granules, probably also organic. Nine days after the urine had been placed in the glass, two little woolly balls of fungus were visible in it to the naked eye. In correcting the proof, I may add that the urine is now thronged with fungous growths of at least three different species; while the odour is highly offensive. But the hot summer weather of the last two months has produced no change in the contents of either of the flasks with bent necks.] Observe, then, what inference is to be drawn from this remarkable fact. There has been nothing in the bent tubes that could by possibility interfere with the transit of any of the gases of the atmosphere. At first, indeed, they contained some drops of condensed aqueous vapour; but these in a few days disappeared, the tubes being dried by the air passing through them, and I beg you particularly to observe that, in the instance before you, the tube is open and dry from end to end. Every atmospheric gas, therefore, in whatever proportion it may exist, must have daily passed unchanged into the flasks to exert upon the putrescible urine any influence of which it was capable; yet no putrefaction has occurred. The urine has remained absolutely free from putrefactive changes for half a year, though exposed during the whole of that time to the action of all the gases of the atmosphere, perpetually renewed. Surely we are safe in drawing the inference that, in the case of this putrescible substance at least, the atmospheric gases alone are incapable of inducing putrefaction. What is it, then, that is essential to putrefaction of urine by atmospheric influence which the bent tubes have arrested? It cannot be any of the gases; but it may be, it must be, some particles suspended in them, some dust, which the angles of the tubes might arrest mechanically. And this conclusion, inevitable as it is from the consideration of the flasks with bent necks, is confirmed by comparison with the other in which the orifice, though narrower, was purposely so arranged as to afford a better chance for the introduction of particles of dust, and in which accordingly chemical changes soon declared themselves in the contained liquid.

This experiment has an equally clear bearing upon the question of equivocal generation, essentially involved in the germ-theory of putrefaction. It illustrates strikingly what appears to be the truth; viz., that even the lowest and most minute forms of life with which we are conversant, do not arise spontaneously in organic substances as the result of the operation of the atmospheric gases upon them, but take their origin from definite particles or germs, the offspring of pre-existing organisms. For, on the one hand, we have seen that this liquid, which is a most favourable nidus for such development, has remained for half a year free from any change in its appearance such as even microscopic organisms would produce, though exposed freely during that long period to the influence of air unchanged except in the circumstance that it has been filtered of suspended particles. And, on the other hand, this same liquid similarly situated in every respect, except in the fact that particles floating in the atmosphere might gain access to it, soon presented, even to the naked eye, two distinct kinds of vegetation, each springing from a definite point, and growing steadily from that point, but incapable of taking origin in any other part of the liquid. [The facts subsequently ascertained, of the absence of any living organism which the microscope could detect in the liquid from one of the flasks with bent neck, and the speedy appearance of abundance of such minute objects as well as of others visible to the naked eye, when the liquid had been removed from its protecting chamber, afford, of course, most satisfactory confirmation.]

There is one circumstance in this experiment which may appear difficult to comprehend. Admitting that the angles of a narrow bent tube might arrest the progress of even the finest dust of air when in very gentle motion, is it conceivable that such particles could fail to be driven into the flasks during the first rush of air into them on the withdrawal of the lamp at the time of the original boiling? This difficulty is met by Pasteur in the following way. He says doubtless germs are carried in, but they pass into a liquid so hot as at once to destroy their vitality. Now, though I feel much diffidence in expressing dissent from so high an authority, I must say I do not feel satisfied with this explanation; inasmuch as Pasteur has himself related experiments which show that the mere raising of urine to the temperature of 212 deg. Fahr. is not sufficient to ensure the destruction of the vitality of the tough-lived germs which it may contain; but that it is essential for that purpose to maintain the liquid for some minutes at the boiling point. (*Comptes Rendus*, vol. l, p. 306.) But, if this be so, the germs introduced on the withdrawal of the lamp, being under the same circumstances as those in urine simply raised to 212 deg. Fahr., and at once allowed to cool, should retain their vitality and give rise to organic development. The explanation which has occurred to myself is as follows. Immediately that the steam ceases to issue from the tube on the removal of the lamp, moisture is deposited upon its interior from the condensation of the aqueous vapour in it; and this moisture remains clinging to the interior of the tube, and tending to form drops at its angles, however rapidly the air be driven through it. And it seems to me natural that this water in the tube should arrest the particles in the air transmitted through it. Conversely, I am inclined to think that the germs of the two growths visible to the naked eye in the flask with straight and short neck entered with the first rush of air, but retained their vitality in the hot liquid, as in Pasteur's experiments, with urine heated to 212 deg. Fahr. and at once cooled. These two fungi had already grown to a sufficient size to be distinguishable by the naked eye, within a few days of the commencement of the experiment, but no other points of growth appeared during the ensuing month; implying that the germs of such fungi, though admitted at first, when the air entered rapidly, were excluded by the narrow, though straight, neck during the slow movements caused by the gradual diurnal changes of temperature.

Believing that there must be germs of various organisms adhering to the interior of the narrow neck near its orifice, I thought that if I were to seal the orifice, and then allow some of the liquid to pass up to its immediate vicinity, I might wash down some of them into the body of the flask, and so induce other growths in the urine. Accordingly, on November 20th, nearly a month after the commencement of the experiment, I sealed the end of the tube with the blow-pipe, protecting the neighbouring parts of the neck from the flame as well as I could with a bit of wet lint wrapped round it. I then tilted the flask so as to cause some of the urine to pass into the neck and back again; and you will observe that there is still a drop in the immediate vicinity of the sealed extremity. A few days later, I imagined that I had attained my object, as several minute points of growth were seen upon the surface of the liquid, distinct from the original floating mass, which by this time had assumed a really beautiful appearance, its upper surface being a circle of three quarters of an inch in diameter, composed of concentric rings of blue mould. But, in the course of a few more days, it became evident that the new growths were of identically the same species as the original floating one; and, on the other hand, that the drop near the end of the neck remained perfectly transparent, instead of exhibiting fungous developments as I had anticipated. Hence I inferred that the germs, which I could not doubt must have existed near the orifice, had been arrested so close to it as to be destroyed by the heat of the flame. Whence, then, did the new growths in the body of the flask take their origin? The answer is obvious enough. The blue mould covering the surface of the original growth teemed with myriads of sporules of the fungus, and, like larger plants, was ready to shed its ripe seeds when shaken; and the tilting of the flask, which had up to that time been carefully kept from disturbance, scattered some of these ripe germs, which grew into organisms like their parent. About a month after the sealing of the tube, all further growth of the fungi in the flask ceased, and its contents have remained unchanged for the last four months, except that the fungi have become shrunk and unhealthy in aspect. This I attribute to the cutting off of the supply of oxygen by the sealing of the tube. [This view has since been verified. On the 2nd inst. (May 1868), I broke off the sealed end of the neck after scratching it with a file, leaving the flask otherwise undisturbed. In four days, I detected the first indications of return of the growth which had been so long suspended; and, a few days later, the dwindled and discoloured original growths were abundantly covered with fresh vegetations of the same nature as before, while the surface of the fluid presented multitudes of new points of development of the same species; the unavoidable motion of the liquid in conveying the flask to and from the meeting, which indeed greatly marred the beauty of the fungi, having evidently scat-

tered other germs about, which remained latent till fresh air was admitted.]

Looking at this experiment as a whole, we see that the atmosphere was rendered incapable of inducing in that specimen of urine either putrefaction or the formation of even the lowest and most minute known organisms, by merely depriving it of its suspended particles; or, conversely, that the "air-dust" is the essential cause both of organic development and of putrefactive changes in such a liquid; while the experiment further illustrates what seems to be a general law; viz., that the low forms of life to which the atmospheric particles give rise, so far as we are able to observe them, resemble higher plants or animals in springing only from pre-existing organisms. Any one who bears these facts in mind will have little difficulty in admitting the truth of the germ theory of putrefaction; and I venture to recommend to any of you who may hereafter feel perplexed by the contradictory and bewildering statements of various authors upon this subject, and be tempted to regard it as hopelessly obscure, that he should recall to his memory the clear evidence respecting it which has been brought before you this evening.

*Emphysema and Pneumothorax from Simple Fracture of the Ribs.*—This mode of experimenting, as described by Pasteur, besides charming me by its simplicity and conclusiveness, had a further special interest for myself, because, before knowing of it, I had explained to my own mind on the same principle the remarkable fact, previously quite inexplicable, that, in simple fracture of the ribs, if the lung be punctured by a fragment driven inwards upon it, the blood effused into the pleural cavity from the wound in the highly vascular organ, though freely mixed with air which enters the pleura through the same orifice, undergoes no decomposition, as is clearly implied by the absence of any symptoms of pleurisy in such cases. The air is sometimes pumped into the pleural cavity in such abundance that, making its way through the wound in the pleura costalis, it inflates the cellular tissue of the whole body; yet this occasions no alarm to the surgeon, unless the opening in the parietal pleura become insufficient to permit free egress for the air, which then becomes pent up in the serous cavity, and, distending it far beyond its natural dimensions, encroaches on the other lung so seriously as to embarrass or even abolish its functions. Thirteen years ago, I had the opportunity of making a *post mortem* examination of the body of a man who had died under such circumstances ten days after the receipt of the injury which caused his symptoms; and I was much struck to find the enormously distended pleura free from effusion, and perfectly smooth and healthy. Why air introduced into the pleura through a wounded lung should have such totally different effects from that entering through a permanently open penetrating wound from without, was to me a complete mystery till I heard of the germ-theory of putrefaction, when it at once occurred to me that, though we could not suppose the gases of the atmosphere to be in any way altered in chemical composition by passing through the trachea and bronchial tubes on their way into the pleura, it was only natural that they should be filtered of germs by the air-passages, one of whose offices it is to arrest inhaled particles of dust, and prevent them from entering the air-cells. In truth, this fact in practical surgery, when duly considered, affords as good evidence in support of the germ-theory of putrefaction as any experiment that can be performed artificially.

Another remarkable example of the same thing, though brought about by different circumstances, occurred recently in my practice at the Infirmary.

*Case of Penetrating Wound of the Thorax and Abdomen.*—On the 1st of October last, a butcher, aged 18, was admitted on account of a most serious wound of the chest, inflicted by a comrade who, angry at having a dirty bladder thrown at him by the patient, threw in return his knife, with a blade nine inches long, and keen edged, half of which buried itself in the patient's infra-axillary region, between the ninth and tenth ribs. He himself drew out the knife, which was followed by a fearful gush of blood. Being accustomed to see blood flow, he said "there was a spout of four inches before the fall." He was immediately taken to the hospital, where my then House-Surgeon, Mr. Hector Cameron, found him blanched, his clothes drenched with blood, which was still pouring from the wound, venous in colour, and with a tendency to regurgitate during inspiration, implying that it proceeded from a wound in the lung, which was further indicated by the occurrence of hæmoptysis. There was also protruding from the external wound a piece of omentum five inches long, shewing that the knife had passed through the diaphragm into the abdominal cavity. No time was to be lost, as death from hæmorrhage was imminent; and Mr. Cameron judged it best to plug the wound, but at the same time to introduce an antiseptic as in compound fracture, in order to destroy any atmospheric germs that might have been drawn in during inspiration. With this object, after cutting off the protruding piece of omentum, which he kept to show me, he soaked a piece of lint with a solution of carbolic acid in four parts of boiled linseed oil, and by means of dressing forceps passed it as far as he could in every direction in the pleural cavity, repeating the application several times. He then took two strips of lint steeped in the same solution, each about a foot long and an inch in breadth, and pushed them into the pleura one upwards, the other downwards, as far as possible consistently with keeping their ends protruding externally; and, the wound being thus plugged antiseptically, he applied a sheet of paste composed of whitening mixed with the oily solution of carbolic acid before-mentioned, taking care that it was large enough to overlap the skin around the orifice by several inches in every direction, retaining it in position by strapping and bandage. It may, perhaps, be said by some of you, "Surely it was heroic practice to introduce irritating carbolic acid so freely into that important serous cavity. Would it not have been a milder and more prudent course to have plugged the wound with a piece of dry lint?" But any one who argues in this way forgets what would have been the inevitable result of such a procedure. The mass of blood accumulated in the pleura would necessarily have been soon decomposed through the agency of the germs contained in the lint; and the putrefying mass, growing from day to day more acrid in the cavity in which it was confined, would undoubtedly have soon caused the death of the already prostrated patient. On the other hand, carbolic acid, being a local anæsthetic, is much less irritating, even when first applied, than the products of decomposition; and it also differs from the latter in this all-important point, that it soon becomes dissipated by diffusion and removed by the surrounding circulation, when, the blood on which it has acted being still amenable to organisation and absorption, the part is as favourably situated as if affected only with a subcutaneous injury. Next day, when I saw the patient for the first time, I cautiously withdrew the plugs, under the protection of a large piece of lint dipped in the oily solution of carbolic acid, and continued the use of the paste. For about ten days the patient progresssed admirably, the pulse descending, the laboured rapid respirations growing less laboured and less rapid, and altogether his condition becoming so much improved that he could not be prevented from sitting up in bed, singing songs and conducting himself otherwise in an imprudent manner. Meanwhile, examination of the thorax disclosed signs of the presence of both blood and air in the pleura, such as dulness of the base and preternatural resonance of the upper and anterior part of that side of the chest, and metallic tinkling, which was well-marked. And to such an extent had this accumulation of blood and air proceeded, that the heart had been pushed over towards the right side, so that its apex beat below the right nipple. And yet this mass of blood, freely exposed to the influence of air, had not decomposed. Any putrefactive germs introduced through the external wound had been destroyed by the carbolic acid, and the air, entering the pleura through the wounded bronchial tubes, had deposited its floating organisms upon the slimy mucous secretion of those tortuous canals. Hence the patient remained free from any symptoms of irritation, and suffered only from loss of blood and the embarrassment of the respiration which was the mechanical result of the injury. But, thirteen days after the accident, profuse hæmoptysis appeared, which I was disposed to attribute to tearing open of the wound in the lung through his imprudent exertions; and this, continuing for several days, threatened entirely to exhaust his weakened frame. The expectorated blood assumed also a putrid odour, like that from gangrene of the lungs; and I was apprehensive that the putrescence might spread to the mass in the pleura. Fortunately, however, this did not occur. The bloody expectoration gradually became purulent, and then diminished in quantity till it ceased entirely. With regard to the external wound, it furnished no pus so long as the original mode of dressing was continued. In the first twenty-four hours, there was a free discharge of bloody serum; but this grew less from day to day, till, six days after the receipt of the injury, it amounted to less than a minim in forty-eight hours; and when the piece of lint, which had been kept permanently on the wound beneath the paste, was removed, between three and four weeks after the accident, a superficial sore was found, which afterwards cicatrised kindly. On November 18th, seven weeks after his admission, the apex of the heart was observed to be again beating below the left mammilla; and, finally, I may add, that he was seen a few days ago by Mr. Cameron, engaged with another butcher in driving a herd of unruly cattle through the streets, when our former patient, though still pale from anæmia, proved the more vigorous of the two in turning the animals; while his lusty exclamations, though not couched in the most decorous language, gave satisfactory evidence of the soundness of his lungs.

[*To be continued.*]