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THE LISTERIAN IDEA IN THE YEAR 1939*

BY

RENÉ LERICHE, M.D.Lyon, LL.D.Glas., Hon. F.R.C.S.Eng.

Professor of Surgery, University of Strasbourg

In reading Lister's papers, clear, precise, clean-cut, and bright, like the metal in our operating theatres, it has appeared to me that nowadays the fullness of the Listerian conception and its permanent application have not always been understood as they ought to be. Toying with the words "antisepsis" and "asepsis," many surgeons of the present day see the letter only and not the spirit of Lister's doctrine. They are inclined to the idea that Lister halted on the threshold of Pasteur's work. His ritual would then have been only one stage, soon exceeded. His method would then be only a momentary phase in a moving practice. And of Lister there would remain only a great name.

Such a conception would show a very imperfect knowledge of his work and thought. And indeed such an erroneous judgment has already been corrected in this place. The late Lord Moynihan put it thus:

"We know that the old quarrel as to the relative merits of the antiseptic and the aseptic methods was senseless and jejune. For Lister was indubitably the parent of both. . . . Aseptic surgery is the wise practice of antiseptic surgery. There is a difference in detail, not in ideal or in fundamental truth."

It is true that boiling and dry heat have in great measure done away with carbolic acid, but these are only antiseptic means for the attainment of asepsis. The objective still remains the coagulation of the protein element of a microbe. Heat? Chemicals? In truth there is no essential difference between them. Heat is safer, easier to handle. We have adopted it. The principle remains the same—the achievement of asepsis by antisepsis. Again, it is necessary to note that the most rigid partisans of asepsis by heat remain faithful, in certain respects, often without knowing it, to the attainment of antisepsis by chemical means. They disinfect the skin with tincture of iodine, with alcohol, or with picric acid. They apply a chemical substance to appendix stumps and to the lines of intestinal section. Involuntary disciples of Lister, they are unable to escape from the dominance of his teaching.

Our interest then, in considering the present position of the Listerian idea, does not consist in the reopening of an old academic quarrel. The question is already settled. Our entire surgical practice of to-day is the direct outcome of Lister's research and teaching, in the light of the

demonstrations of Pasteur. Lister was the first to understand, and in the beginning he fought alone, and alone withstood the blows directed against his teaching. Such glory as his is not to be shared.

Having said this, and once for all, our present interest in the subject is to see whether nowadays we do not require to get back to Lister's first idea, for it is just possible that, with all the improvements and apparent advances we have made in our methods of destroying germs, we may have too much overlooked one of the essential elements in the problem of sterilization.

Have the Germs in the Air a Pathogenic Quality?

In other words, is infection of wounds from the air, and by the germs contained in it, of any substantial importance? After his memorable conversation with Thomas Anderson, who told him of Pasteur's experiments, Lister conceived the idea that infection of wounds was conveyed by the air. He believed that it was the living particles in the air that were to be feared, and therefore to be destroyed. That is why he invented the carbolic acid spray. Nowadays we no longer trouble much about the air in our operating theatres. We mainly fear direct involuntary contagion, contact with everything that the surgeon employs and everything that is around him. Germs in suspension, floating in the air around, do not worry us. Are we right in so thinking? At first sight, everything seems to point towards an affirmative answer to the question. Under present surgical conditions infective complications of the suppurative type have become exceptions.

As a rule our operative wounds heal by first intention. They are neat, free from redness, and without obvious inflammation. And yet we know that they are not entirely free from germs. Our operation scars generally form without incident, but they are not aseptic, even though we speak as though they were. Bacteriological tests have long since proved this. For this very reason, as we are generally desirous of further improvement, recent years have seen a notable renewal of researches on the subject.

Petri dishes, brought into contact with our instruments, or even, as I have proved, with swabs used in the abdomen for packing the intestine out of the way, are always contaminated. Culture discloses the presence of colonies of staphylococci and of streptococci. The colonies are few in number if no one moves in the operating room. They

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are numerous if there is much movement, if there are many onlookers, if the latter are not duly masked, and if they approach close to the operative field. If Petri dishes are placed near the spectators the dishes all show numerous germ colonies. The experiments of Flüge and those of Trillat have shown us why.

On the other hand, germs are few in number if the operating theatre is kept absolutely closed, if the floor is constantly wet, and if the air is conditioned and filtered. This all means that the air of our operating theatres is usually heavily charged with germs when we are operating, and all the more heavily charged the more it is disturbed. The ordinary matter-of-fact surgeon, who is not readily disturbed and is easily satisfied, will say perhaps: "What, then, does it matter? Our patients do quite well, and nothing untoward happens." But it is indeed true that, although Petri dishes exposed in proximity to us furnish colonies of microbes, our operative wounds pursue an undisturbed and absolutely apyretic course. They heal by first intention, and without a doubt give us smooth linear scars.

But do none of them, in the course of a few months, widen out somewhat, become exuberant, perhaps a little blue under the influence of cold? Do not some of them become painful and more or less keloid? Do not some of them have a hard, sclerotic substratum? Are there none that give way—it may be months afterwards—causing, perhaps, eventration; although in the beginning they seemed quite firm and secure? Are there not a number of so-called aseptic operations that result in peritoneal adhesions, even though no visceral cavity has been opened and there is no question of endogenous infection? And when some operative procedure has apparently cured an old septic lesion of only moderate virulence, only to be followed by a fresh and sudden infective attack, are we not entitled to suppose that there has been a fresh infection superimposed on it? Finally, do not vascular complications—phlebitis and embolism—still occur, even although otherwise the post-operative course has been uninterruptedly satisfactory, and the process of healing apparently normal?

In view of all this, it seems justifiable to raise once again the question as to what deleterious influence may be exerted by infection having its origin in the air of our operating theatres. Though not actually pyogenic, may it not perhaps be capable of proteolysis, of modifying local protein structure, of slowly dissolving new-formed elastic architecture, of perverting local tissue metabolism? To put it another way, may not this masked or imperfect infection, incapable of clinical detection, and which we regard as actually of little account, have effects on the tissues which hitherto we had not ascribed to the real cause?

One must not forget that the word "infection" has come nowadays to have a much wider meaning in microbiology than that which we surgeons give it—especially since the peculiar effects of ultraviruses and of certain protein molecules have become known. If this is so, might we not be wise to return to the Listerian idea, and to endeavour, from now on, to eliminate even the least possibility of air infection?

Role of the Constitutional Factor in the Post-operative Development of a Wound

Personally I am inclined to think that the quality of our operation scars, their aesthetic value, their firmness, and the future of their development depend, above all, on the constitution—local and general—of the patient's tissues.

Under similar operative conditions, every patient heals in his own particular way, because each has his own tissue values, which may be regarded as a reflex of his hormonal personality.

Without any doubt, attention to detail and the development of the most perfect technique have, up to a point, a definite influence on post-operative healing; but no given surgeon obtains, always and at all times, the satisfactory scars he may deserve. Some of our patients have linear, soft, and satisfactory scars, while the scars of others are coarse and exuberant; and we are powerless to determine the issue.

Every repair depends very much on the vitality of the tissues—a factor which sometimes, of itself, defeats the best of technique. Our scars are composed mainly of connective tissues, and connective-tissue vitality is regulated normally by circulatory and hormonal phenomena that have a different tonality for everyone. It is true that there is only one general plan of wound evolution, but, from one man to another, what variations there are! During the process of healing, these circulatory and hormonal values come into play, each in its own way, and always with its individual characteristics. Quick-acting and well-balanced vasomotor reactions generally assure the development of perfect scars. Sluggish reactions—as when there is a degree of stasis—lead to hypertrophic scars. The subjects of certain hypophyseal conditions have elastic tissues of poor quality, while people with excess of parathyroid have satisfactory collagen. Many other factors, still unsuspected, doubtless influence tissue repair. One day, perhaps, we shall have "hormonally directed healing," and then our patients will be spared many discomforts.

These reflections indicate to you that I am personally inclined to turn in quite a different direction from the one I discussed previously. But I readily admit that even these reflections are not sufficient to justify the setting aside of the other, the microbic, hypothesis. It is first of all necessary that we should study for a little the means that have been suggested to protect our patients against air infection. Two of these may be considered. They are: sterilization of the air by physical agents, and some arrangement such as the complete outfit devised by Gudin, which has for its purpose the attainment of what he has called "integral asepsis."

Are Physical Agents Antiseptic?

Is it the case that certain forms of irradiation are able to exert an effective action on the microbic flora of the air in the vicinity of a wound? Some writers have affirmed that this is so as regards *ultra-violet rays*, the most precise and consistent observations on the subject being those of Hart and of Santy.

Hart employs special lamps that irradiate the wound throughout the whole operation. He has found that, exposed in this way to the rays, Petri dishes containing staphylococcus cultures are sterilized in a few minutes. Sterile Petri dishes placed near the operative field remain sterile if irradiation is carried out, while in the absence of irradiation they become contaminated.

Santy's operating theatre is so arranged that the air is made to pass in front of a powerful generator of ultra-violet rays, and is then sent back through a charcoal filter. He has found that, under such conditions, Petri dishes which would ordinarily show thirty to fifty colonies at the end of an operation show hardly any (perhaps four to five colonies per dish), even if there are many onlookers. It seems that, with a few improvements, a complete

sterilization might be reached. We may have here the promising beginnings of further great surgical progress, and we shall probably be able very soon to reach a new stage in aseptic technique.

Are ultra-violet rays the only ones to act in this way? For the present, apparently one must answer the question in the affirmative. *Infra-red rays* do not seem to have similar germicidal properties. At the instigation of Paschoud I have during the last two years operated quite a lot under infra-red rays—over a thousand times—and it has not appeared to me that, clinically, infective conditions calling for operative intervention have behaved in any way differently, according as they were or were not irradiated. And in aseptic surgery I have met with cases of phlebitis and embolism in patients who had been operated on under adequate infra-red irradiation.

I am therefore convinced that infra-red irradiation makes no essential change in the ordinary evolution of healing of surgical wounds. This does not necessarily imply that exposure to infra-red rays makes no change at all in post-operative development. Patients certainly suffer less, and seem to be more comfortable after their operation. This is probably to be explained by the fact that infra-red rays cause hyperaemia, and this hyperaemia modifies the quality of the post-operative sympathetic reactions, though it appears to have no real action on the infection itself. I have placed Petri dishes actually in the wound whilst operating under the infra-red rays, and the usual microbic developments have taken place.

We have to be content, therefore, with the ultra-violet rays, which, unfortunately, have no analgesic effects. A combination of infra-red and ultra-violet rays might perhaps be efficacious; but, on the other hand, it might have some drawbacks, both for our patients and for ourselves. For this reason it appears necessary for us to explore other possible lines of action.

Gudin's Integral Asepsis

Is it not possible to operate, as Pasteur would have wished, in a completely sterile atmosphere, in a theatre where everything is sterile, into which no one should enter unless clad from head to foot in sterile garments; a theatre where anything could be touched without the slightest risk, and anything could be moved about without danger because air, walls, furniture, floor—in fact, everything—would be germ-free? That is the question that Maurice Gudin of Rio de Janeiro put to himself a few years ago. Long before his time the air in operating theatres had quite often been sterilized by steam or by antiseptic spraying. I remember visiting, thirty-three years ago, an organization of this type in Kraske's clinic. But, as soon as operator and assistants entered the theatre, germs entered with them, and absolute asepsis was little more than an empty word. This attempted effort at improvement came to be regarded as an unnecessary complication, and the idea was soon abandoned.

Gudin's objective is altogether different. To him the credit is due of being the first to realize the necessity for a really complete outfit. For this purpose he established a group of sterilized rooms, so disposed that patient, surgeon, and assistants shall enter a completely sterile theatre only when they themselves are identically aseptic, dressed from head to foot in sterile garments, the operators having only a glass-protected opening for their eyes and the patient an opening for the operative field, suitably antisepticized. In order to obtain complete sterilization of the different rooms and their interior fittings Gudin uses, by means of a special apparatus, formaldehyde

vapour, which finds its way into every corner, and which is later neutralized by ammonia. Bacteriological tests have shown him that under such conditions complete sterilization can be achieved. Door knobs may be handled, swabs may be taken from shelves, without there ever being any possibility of contamination: asepsis is "integral."

I have myself operated in Gudin's sterile block, and in spite of the smallness of the operating theatre, with its roof of glass enabling the assistants to overlook the operating table, I was completely at ease and operated without tiring. The air is conditioned. The temperature is pleasant. Silence is complete. And I had really a feeling of great security.

Here you have, then, actually in being, and after the lapse of three-quarters of a century, a revival, in greatly improved form, of the Listerian idea of sterilizing the air by chemical means; a revival in strict conformity with the ideal of Pasteur, with whose views on such a matter Lister was always in agreement. Others besides Gudin, working on similar lines, have achieved a form of integral asepsis. Trenel filters the air by means of cotton and oil in a closed circuit, and then regulates its temperature. Masmonteil uses superheated sterile steam, which is extracted when it has produced the desired effects in precipitating the microbes. Both Trenel and Masmonteil, of course, have an arrangement of successive rooms, in order to avoid the bringing in of germs by operator, assistants, or by the patient himself. The result in the way of sterilization seems to be quite effective. Masmonteil states that bacteriological tests show that the air is almost sterile, that the walls are sterile in 85 per cent. of cases, and the floor in 75 per cent. Here also, then, we appear to have an acceptable solution of the problem under discussion. Are the practical results, however, worth the effort of rebuilding our operating theatres, and the trouble of completely altering our ways of working?

Gudin compares the usual healing by first intention, under conditions not entirely germ-free, with healing that is really aseptic, which he terms Pasteurian, and which he believes to have positive advantages. Is he right? In theory there can be little doubt that he is. No one could reasonably say the contrary. In actual practice it is clearly possible, though perhaps not yet completely proved. Only a long comparative experience can decide, because it is a question of minute differences, and shades of difference, very difficult to appreciate. It would, however, be a mistake for us to abandon interest in the subject.

It may be that in integral asepsis we have a condition of progress—a progress of which we do not yet know the full significance. I have given full weight to all the theoretical objections that might be made to Gudin's conception—difficulty, inconvenience, relative uselessness, expense. None of these is of determining value. We must never forget that, even in Lister's time, no one but Lister at first saw any reason for changing the traditional practice of surgery. Everything was as good as it need be! All that was done was perfect. And yet what terrible failures! And, in more recent years, have we not seen numerous surgeons who strenuously opposed the use of masks or of gloves? Even a surgeon is a creature of habit.

For my part, I believe that some international organization might well undertake a fresh study of the subject of integral asepsis, a subject which is of interest to the whole world. Such an organization might well supply the necessary facilities of study to a group of investigators noted for their powers of research and of discrimination. For

the present, only one definite statement can be made in this connexion—namely, that the Listerian idea still dominates the evolution of surgical thought, and that it has not yet ceased to give promise of fruit.

Treatment of Wounds

The Listerian conception, however, was not concerned merely with operative antisepsis. Its primary purpose was to achieve the disinfection of wounds and their sterilization by chemical means. Where do we stand to-day with regard to this very problem?

Up to the year 1914 we had very little exact knowledge of the changes involved in the healing of wounds, or of how wounds might be disinfected. Four years of war surgery, however, brought under our eyes all at once an enormous mass of experimental data, which we had the opportunity of examining from start to finish. No laboratory worker had ever before dreamed of experimentation on so vast a scale. One had only to observe. Each day brought us its fresh batch of completed experiments. The inevitable result of the infection was beyond dispute. The conditions were obvious. Definite conclusions could really be arrived at, conclusions free from any ambiguity, provided that due care were taken to employ really comparable series of cases. In my view, in spite of the successes of many chemical substances—of bipp, of eusol, of the hypochlorites, of the various aniline dyes and essential oils—and in spite of the magnificent results of the Carrel treatment, the conclusion is inevitable that the large-scale experiment of 1914 to 1918 established the failure, relative but none the less unmistakable, of antiseptics. It showed that what determined the fate of a wound was, above all, the vital quality of the tissues exposed to infection, and not the use of this or that chemical product. Any wound that was clean-cut and free from contusion, if left freely exposed to the air, healed of itself, without any antiseptic aid.

On the other hand, an irregular, ragged wound with separated edges and necrotic walls suppurred for a long time, even when treated by appropriate antiseptics; while if it were carefully excised it remained sterile and could be sutured. The essential point was that the excision should be carried through healthy tissues. Direct surgical action, then, superseded chemotherapy. Without such surgical intervention nothing was of any use. With it, success was generally assured.

Certain it is that about the year 1917 we realized that, without the slightest chemical treatment, and without risk, we might proceed to suture a well-excised wound even if it still harboured staphylococci or perfringens bacilli, provided that the neighbouring tissues were of satisfactory vitality. Note that these observations are quite in line with conclusions drawn from bacteriological studies carried out on the operative wounds of present-day aseptic surgery. Here, too, infective complications do not develop, in spite of a certain degree of contamination, if haemostasis is efficient and the tissues are not traumatized.

What I have just said applies particularly to fresh wounds treated within a few hours of their reception. May the same be said of wounds clinically infected? *A priori* one might say of these that certain antiseptics have proved their efficiency in such cases. But, even so, their effects require analysis.

Nowadays we know well enough, after seeing so many wounds suppurate despite all our efforts, that antiseptics are only successful in disinfecting wounds that are laid completely open. And we know that, whenever there are ragged edges, recesses, lacerated necrotic tags of tissue,

blind tunnels, or dead spaces, even the best antiseptics invariably fail. And the best antiseptics are not those which, *in vitro*, have the greatest germicidal power, but those that have a powerful proteolytic action, such as Dakin's fluid, which rapidly destroys dead or dying tissues, and acts, therefore, like a chemical bistoury. It would probably be true to say that the process by which the germs disappear is of little moment so long as they do disappear. And indeed such a proposition would not be actually incorrect. Nevertheless, if we are to make real progress in our art we must understand things aright and not be satisfied with mere words. We must try to formulate the essential conditions of disinfection of infected wounds. Only then shall we be able, on the basis of actual knowledge, to see and understand the laws governing antiseptic action.

The Essential Conditions of Disinfection

The first condition has reference to the wound itself—its characters and architecture. The only wounds that have the capability of spontaneous disinfection or that are readily disinfected by antiseptics are those that are freely exposed to the air in every part. A deep wound cannot be effectively disinfected by the use of a chemical substance and a drainage-tube. Neither of these can achieve the simple beneficial effect of a clean cut with scissors. The most valuable of all antiseptic agents is the bistoury, by the use of which the whole of the injured area can be given free exposure; and Carrel's method only achieved success when it came to be realized that an initial and complete surgical preparation was a necessary preliminary to its application.

Even now many surgeons fail to realize the practical importance of the systematic opening up of wounds, and in peace-time, even as in war-time, the majority of failures registered in the treatment of infected wounds are due to surgeons failing to respect this definite law.

The second condition of disinfection is the complete and prolonged immobilization of the wound and part. In order to understand fully the importance of this condition it is necessary to specify exactly what happens in the case of a wound imperfectly immobilized—a wound not properly put at rest. We all know that the dressing of an infected wound, especially if it is situated near a bone or joint, is generally followed by a brisk rise of temperature. What is less well known is that prior to the dressing a blood culture may be sterile, and yet two hours after the dressing it is no longer so—the culture is positive. Movement, therefore, in the case of certain infected wounds, gives rise to a transient septicaemia. This accepted fact of clinical experience adequately explains the organic disequilibrium that follows certain dressings.

That was why Ollier, reasoning from simple facts of observation such as these, long ago made a systematic practice in all his bone surgery of changing dressings as seldom as possible. He dressed his resections only after a long interval—a month, six weeks, or even longer, after the operation. This practice has indeed remained traditional at Lyons for quite three-quarters of a century, and has hardly been changed at all. More recently, in America, the advantages of immobilization have been rediscovered and proclaimed by Orr, and it cannot be disputed that these advantages are generally very remarkable.

Even serious and acute infections of bones or joints may quiet down within a few days if the affected part or limb is adequately immobilized in plaster—and this in spite of the unpleasant odour which the dressing soon

acquires. Within a few days the temperature may come down to normal and the general conditions of the patient alter for the better. His appetite returns and his whole appearance is different. After several weeks, it may be, when the dressing is changed for the first time, the wound is found covered with abundant fleshy granulations, well vascularized, of a good colour and remarkably healthy appearance. The erythematous skin around the wound is covered by a thick, sticky, wax-like exudate. Bacteriological examination of this exudate shows an abundance of micro-organisms, just as may be found in the dressing material and bandages. On the other hand, however, only a few germs are to be found on the surface of the wound itself—not more than two or three perhaps in the microscopical field. And in the depths of the wound the tissues are germ-free. I have proved this fact many times.

What exactly has happened? One cannot yet say. The micro-organisms have not merely died out, since outside the actual wound they are active and very numerous. It would appear as if they had been simply washed out of the wound by the exudation of lymph, the direct result of the local congestion of the deeper tissues whose normal static equilibrium, on which the humoral balance depends, is defective or entirely absent. The same phenomenon can be seen after exposure of wounds to the sun, even as it can be demonstrated in cases treated by hypertonic solutions, as advocated by Sir Almroth Wright. In any case, there is the fact, and there are the results. We know that immobilization is perhaps the most valuable of all our therapeutic means of combating wound infection.

Listerization by the Intra-arterial Route

When the two conditions of wound disinfection which I have just mentioned have been fulfilled, then, but only then, may chemical antiseptics be used in addition. Their employment is not a mere prelude to or accessory in treatment of questionable value. But what antiseptics ought one to employ? Surgeons of the present day no longer make use of carbolic acid, but they are not forgetful of what surgery owes to it. In an Address on Lister, delivered in 1927 during the Lister Centenary Celebrations, my distinguished friend Archibald Young recorded and contrasted in a very striking way the pre-Listerian and post-Listerian statistics of the Glasgow Royal Infirmary. They speak for themselves.

Ought we to remain faithful still to carbolic acid, as Lucas-Championnière in Paris, and my master, Poncet, in Lyons—who was a disciple of Lister from the very beginning—remained to the end of their lives? Or should the vogue of carbolic acid be accepted as a thing of the past? We no longer know how to use it. In its stead we make use of the lytic action of the hypochlorites and we have recourse to germicides of the type of mercurochrome. Given the conditions specified above, these are certainly of great service in the treatment of wounds, but even they have only a limited scope. Their action in reality is only superficial. Infection in the deeper tissues is unaffected by them. Direct contact is required between the germicidal agent and the micro-organisms if a successful result is to be achieved. The germicide very quickly decomposes, or tissue debris shuts it off from close contact with the germs it ought to encounter. Hence the necessity, as Carrel pointed out, for a constantly renewed supply of germicide.

That is one reason why, in ordinary daily practice, chemical disinfection is difficult to use, especially in complicated cases, and why it meets with many failures. It is not sufficient merely to register these failures. They

ought to have the effect of urging us to give careful consideration to the evidences that come before us from day to day. Our observations teach us, surely, that real disinfection acts from within outwards, beginning in the depths, and later reaching the surface. Why, then, should we shut our eyes to the evidence that we have, especially since we have at our disposal nowadays means of attacking the germs even in the tissue depths?

And in speaking of this, I am not thinking of the intra-venous route—wasteful as it is—of which one wonders if it can possibly be efficacious in a peripheral lesion. Nor am I thinking even of the intracardiac route, which I have employed in certain cases of septicaemia. I have in mind, rather, the intra-arterial route of Goyanes, which has been taken up again by Reynaldo dos Santos, and regarding which dos Santos has recorded some striking results. Dos Santos, over ten years ago, demonstrated that certain serious infective conditions could sometimes be arrested by injecting through the skin, and directly into the main artery of the affected region, either gonacrin or mercurochrome. His collaborator, Cunha Lamas, published statistics of 120 cases of different types of infection treated in this way by intra-arterial Listerization.

At the Strasbourg Clinic, since the year 1929, we have been applying the intra-arterial method in quite a considerable number of conditions. We have stopped counting up the successes it has given us, in suppurative arthritis of all sorts, in early cases of osteomyelitis, in lymphangitis, and in the pregangrenous infective lesions of arteritis.

Only the other day I had under treatment a young man who was suffering from a suppurative arthritis of the elbow following upon injury. He had had a temperature of 104° F. for three days. He received an injection of 10 c.cm. of a 1 per cent. aqueous solution of mercurochrome. This was given into the axillary artery, and within a few hours his temperature fell to 99.5° F. and remained down.

Our experience of ten years, then, would seem to justify us in claiming that disinfection by the introduction of antiseptics by the arterial channel is destined to become the method of choice. Further, in view of the continuing progress of chemotherapy, and the great hopes we have of the prontosil group of drugs, it is probable that very soon we may be able to arrest, without much sacrifice of tissue, the progress of many infective lesions that hitherto have demanded serious mutilation.

Obviously the method calls for technical and manual dexterity. Its employment requires judgment. It is not a panacea. It has its limitations. Infective conditions produce anatomical disturbances which may render its action incomplete or useless. It therefore has its failures. Yet, with a due sense of the importance of what I am saying, I have no hesitation in claiming that Listerization by the intra-arterial route is a great step forward, and such a forward step may lead to further advance.

Intra-arterial Novocainization

Observation of patients treated in recent years by novocainization has enabled us to analyse more closely the physiological mechanism which infection brings into operation in the tissues. A considerable disturbance of the local vegetative equilibrium is brought about. It follows that, if novocain is infiltrated into and about the region involved in sufficient quantity to block vasomotor innervation, an incipient infective process may be aborted. In the same way novocain infiltration of sympathetic ganglia may at once arrest the further development of a local or latent infection. In view of observations such as these at the Strasbourg Clinic, we were led ultimately to inject novocain into the main artery of a limb in which

an acute infective process was beginning. The infection subsided.

It would seem, then, quite probable that, in addition to intra-arterial Listerization—frankly antiseptic in its purpose, and aiming at the direct destruction of germs—we have here a possible method of getting at the very mechanism of the inflammatory process and of averting by physiological means the deleterious consequences of an infective inoculation. We are continuing our researches along this line, and have already obtained some unquestionably satisfactory results in salpingitis and in certain pelvic infections, as already recorded by Daniel.

Here again, then, we have ground for substantial hope. The main problem has assumed a new phase. Up to the present all efforts to treat infective processes have been anti-microbial. These later efforts are directed to the patient, the microbe's host. Just as it takes two to make a quarrel, two elements are necessary for the development of a disease—the host and the infecting organism. It can hardly be disputed that much is to be gained by looking after the one as well as the other. This is only a further demonstration that the Listerian conception is still capable of bearing fruit in our minds.

The Lesson of Lister

Such fruitfulness does not surprise us. Ever since I received the honour of being asked to address you I have been trying to penetrate the official and almost legendary figure of Lord Lister and appreciate the real man. Any man of outstanding personality is apt very soon to be depicted in legend and history on a somewhat artificial pattern. With the passage of time the hero becomes more and more an almost abstract figure, often quite different from what he was in life.

Through the pages of many addresses and panegyrics I have sought to find out what, after all, was the inner rhythm or essence of Lister's mind. Having analysed him to the best of my ability, I desired to reconstruct for myself a picture of the man. It is clear that that is not an easy thing to do. Even in the case of living people we never see more than the shadowy outline of their personality, and we see it only through ourselves. On the other hand, the work that a man has done may well reflect the man himself, and we are often able to construct for ourselves a picture of the man through the lessons he has taught us—and, in so doing, not go very far astray.

We have learned from Lister the supreme virtue of intuition, the strength of reason based on experimental research, the creative power of persevering effort, of disinterested effort that is never satisfied but always strives to do better still. And so Lister appears to me, above all, a man of unusual type, to whom truth was directly intuitive, a man of complete honesty of mind and thought, and deeply sensitive to everything human. I picture him to myself a more or less solitary figure, working at his researches alone, his entire life centred on his purpose, ever master of himself, and determined yet restrained in all his ceaseless experimentation.

His genius was not the kind that, in tumultuous fashion, carries everything before it. It was the sort which achieves victory slowly through the radiant glory of truth itself. Lister's work was securely based upon the solid rock, and if ever work was beneficial to mankind his surely was. Gratefully, therefore, and with all respect, I salute Lister, the man, and to you, gentlemen, I tender my sincere thanks for having favoured me with the opportunity of doing so in this place.

OBSERVATIONS ON THE SURGICAL TREATMENT OF THYROTOXICOSIS

BY

J. W. LINNELL, M.D., M.R.C.P.

*Consulting Physician to the L.C.C. Thyroid Clinic, New End ;
Physician, Metropolitan Hospital*

AND

GEOFFREY KEYNES, M.D., F.R.C.S.

*Consulting Surgeon to the L.C.C. Thyroid Clinic, New End ;
Assistant Surgeon, St. Bartholomew's Hospital*

It is difficult for those who are daily observing the condition known as toxic goitre in its many clinical varieties to obtain a true view of its relative frequency in the population. A large proportion of these patients naturally gravitate to goitre clinics, and, in addition, the physician or surgeon who is fully "goitre-conscious"—that is, whose eye is fully trained to observe the less obvious signs of toxicity—is continually encountering the disease in unexpected places. Sometimes the signs have not been noticed by the medical men who are attending patients for apparently unrelated conditions. It would seem, indeed, that the disease is commoner than is even now generally realized, and for this reason we add the following observations to the already large volume of literature on the subject.

The Question of Operation

Some physicians are still opposed—almost, it seems, on principle—to operating for toxic goitre. Many spontaneous cures, it is said, have been seen. It is true that a few patients, usually among the younger ones, will permanently lose the toxic symptoms within a few months of their onset without treatment of any kind, or with rest and sedatives only. A few more, able and willing to submit to a semi-invalid existence for a long period, perhaps for years, may also be cured without operation. For this reason we often advise a period of observation for young patients in whom the symptoms have only recently appeared. Unfortunately, however, it is impossible to know whether any given patient is potentially a member of the spontaneously curable class, so that it is usually unwise to prolong the observation period beyond a few months unless the patient is obviously improving. Among the older patients the nodular form of goitre is common, and it is quite certain that when it becomes toxic it is virtually never cured by medical treatment alone. Even among the apparent "cures" of toxic colloid goitres it is found that in the great majority toxic symptoms recur after varying periods. Thus the patients eventually come to surgical treatment, having forfeited normal activity for several years of their lives in vain. In fact, many of the so-called cures are more apparent than real. Every patient with thyrotoxicosis shows alternating phases of exacerbations and remissions, and in the milder examples a remission may easily be mistaken for a "cure." In addition, a thyroid gland which has been apparently quiescent for years may be reactivated by conditions such as pregnancy, miscarriage, lactation, the menopause, an acute infection (for example, tonsillitis), an anxiety state, and mental or emotional shock. An anxiety state may, indeed, be induced merely by the presence of the goitre itself. Often quite small goitres make their presence felt by a slight sensation of pressure in the throat. The patient is thus constantly led to feel it and examine it in the mirror, until she has convinced herself that it is enlarging and may be harming her in various imaginary